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SCIENTIFIC PAPERS

OF

ASA GRAY

SELECTED BY

CHARLES SPRAGUE SARGENT

VOL. I.

REVIEWS OF WORKS ON BOTANY AND RELATED SUBJECTS

1834–1887

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INTRODUCTION.

Asa Gray's first scientific paper was published in 1834; his last was written in 1887, a few weeks before the end of his life. The number of his contributions to science and their variety is remarkable, and astonishes his associates even, familiar as they were with his intellectual activity, his various attainments, and that surprising industry which neither assured position, the weariness of advancing years, nor the hopelessness of the task he had imposed upon himself, ever diminished.

Professor Gray's writings may be naturally grouped in four divisions. The first in importance contains his contributions to descriptive botany. These with few exceptions were devoted to the flora of North America, and although it did not fall to his lot, as it did to that of some of his contemporaries, to elaborate any one of the great families of plants, the extent and character of his contributions to systematic botany will place his name among those of the masters of the science.

His works of a purely educational character are only second in importance to his writings on the flora of North America; and their influence upon the development of botanical knowledge in this country, during the half century which elapsed between the publication of the first and the last of the series, has been great and must long be felt. No text-books of science surpass them in the philosophical treatment of the subjects they embrace, or in the beauty and clearness of their style.

A series of critical reviews of important scientific publications, and of historical accounts of the lives and labors of botanical worthies, may be conveniently grouped in the third division of Professor Gray's writings; while in the fourth fall

time, the most important part in elevating the science to which his whole life was devoted.

The second volume of this series contains a few essays of general interest, and a selection of the biographical sketches of the principal botanists who have died in recent years.

C. S. S.

Brookline, April, 1889.

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REVIEWS.

LINDLEY'S NATURAL SYSTEM OF BOTANY.[1]

The cultivators of botany in this country are generally acquainted with the former edition of this work through the American reprint, edited by Dr. Torrey, and published by Messrs. Carvill of New York, in the spring of 1831. Dr. Lindley's treatise was, at the time of its appearance, the only introduction to the Natural System in the English language, if we except a translation of Achille Richard's "Nouveaux Elemens de la Botanique," which was published about the same period. It is unnecessary to state that a treatise of this kind was greatly needed, or to allude either to the peculiar qualifications of the learned and industrious author for the accomplishment of the task, or the high estimation in which the work is held in Europe. But we may very properly offer our testimony respecting the great and highly favorable influence which it has exerted upon the progress of botanical science in the United States. Great as the merits of the work undoubtedly are, we must nevertheless be excused from adopting the terms of extravagant and sometimes equivocal eulogy employed by a popular author, who gravely informs his readers that no book, since printed Bibles were first sold in Paris by Dr. Faustus, ever excited so much surprise and

[1] *A Natural System of Botany;* or a systematic view of the Organization, Natural Affinities, and Geographical Distribution of the whole Vegetable Kingdom; together with the uses of the most important species in Medicine, the Arts, and rural or domestic economy. By John Lindley. Second edition, with numerous additions and corrections, and a complete list of genera and their synonyms. London: 1836. (American Journal of Science and Arts, xxxii. 292.)

wonder as did Dr. Torrey's edition of Lindley's "Introduction to the Natural System of Botany." Now we can hardly believe that either the author or the American editor of the work referred to were ever in danger, as was honest Dr. Faustus, of being burned for witchcraft, neither do we find anything in its pages calculated to produce such astonishing effects, except, perhaps, upon the minds of those botanists, if such they may be called, who had never dreamed of any important changes in the science since the appearance of good Dr. Turton's translation of the "Species Plantarum," and who speak of Jussieu as a writer who "has greatly improved upon the natural orders of Linnæus." [1] We have no hesitation, however, in expressing our conviction that no single work has had such a general and favorable influence upon the advancement of botanical science in this country, as the American edition of Dr. Lindley's Introduction to the Natural System. This treatise, however useful,

[1] Dr. Lindley is quite right in his remark that the chief difficulties the student has to encounter in the study of botany, upon the principles of the Natural System, have been very much exaggerated by persons who have written upon the subject without understanding it. To refer to a single instance. In the fifth edition of the Manual of Botany, by Mr. Eaton, an account of the Natural Orders of Jussieu is given, in which the genera Ambrosia and Xanthium are referred to *Urticeæ;* and in a note it is added, "Some botanists place the last two genera in the order *Corymbiferæ* also in the Linnæan class *Syngenesia*. I see no good reason for these innovations." Now Linnæus, in his artificial arrangement, certainly did place these genera (and also Parthenium and Iva) in *Monœcia Pentandria;* but the *innovator* in this instance is Jussieu himself, who never referred these two genera to *Urticeæ*, but places them in his order *Corymbiferæ* (*Compositæ*), where they truly belong. The descriptions of Natural Orders in Eaton's Manual, purporting to be taken from Jussieu, bear a very remote resemblance indeed to the ordinal characters of the admirable Genera Plantarum of that author, while the occasional criticisms on its supposed errors afford the clearest proof that the work was not understood by the author alluded to. It should be recollected that, previously to the reprint of Dr. Lindley's Introduction, Mr. Eaton's Manual was the only work professing to give a view of the Natural System within the reach of the great majority of the botanical students of his country, excepting, perhaps, the American edition of Smith's Grammar of Botany.

was indeed not absolutely indispensable to the favored few, who, aided by the works of Jussieu, Brown, De Candolle, the elder and younger Richard, etc., were already successfully and honorably pursuing their studies and investigations; but to the numerous cultivators of botany throughout the country, who could seldom be expected to possess, or have access to, well-furnished libraries, and to whom the writings of these great luminaries of the science were mostly unknown except by name, this publication was a truly welcome acquisition, conferring advantages which those alone who have pursued their studies under such unfavorable circumstances can fully appreciate.

A second and greatly improved edition of this work having appeared within the past year, it occurred to the writer of these remarks that a cursory notice of it might not be unacceptable to the readers of the "American Journal of Science." We do not intend, in these observations, to engage in a defence of what is called the Natural System of Botany, but take it for granted that the science can by no other method be successfully and philosophically pursued; or, to employ the forcible language of Linnæus, "Methodus naturalis primus et ultimus finis botanices est et erit . . . Primum et ultimum in hoc botanicis desideratum est." The few persons who remain at this day unconvinced of its advantages are not likely to be affected by any arguments that we could adduce. A somewhat larger number may perhaps be found in this country who admit the importance and utility of the natural arrangement in the abstract, but decline to avail themselves of the advantages it affords in the study of plants, because, forsooth, it is too much trouble to acquire the enlarged views of vegetable structure which are necessary for the application of its principles. It would almost seem, from the views and practice of such botanists, that they considered it the chief object of a classification to afford the means of ascertaining the name of an unknown plant by the slightest examination of its structure, and with the least possible expenditure of thought.

In the first edition, Dr. Lindley entered into some detailed

explanations to show the fallacy of the common opinion that the artificial system of Linnæus is easy, and the Natural System difficult of application. The sentiments of the public have undergone so great a change upon this subject within the last five or six years that he finds it no longer necessary to adduce these considerations, and accordingly commences at once with a development of the principles on which the Natural System is founded, namely, "That the affinities of plants may be determined by a consideration of all the points of resemblance between their various parts, properties, and qualities; that thence an arrangement may be deduced in which those species will be placed next each other which have the greatest degree of relationship; and that consequently the quality or structure of an imperfectly known plant may be determined by those of another which is well known. Hence arises its superiority over arbitrary or artificial systems, such as that of Linnæus, in which there is no combination of ideas, but which are mere collections of isolated facts, not having any distinct relation to each other." (*Preface*, p. vii.)

We have never met with a more clear and succinct account of the principles upon which the primary divisions of the vegetable kingdom rest than that comprised in the following extract. Those acquainted with the first edition will perceive that the author has changed his opinions respecting the number of these primary divisions or classes; the *Gymnospermæ*, or Flowering plants with naked ovules (comprising the *Coniferæ*, *Cycadeæ*, and, according to Brongniart and Lindley, the *Equisetaceæ*), and the *Rhizanthæ*, as originally established by Blume, being here admitted to the rank of independent classes. Their claim to this rank, however, can as yet be hardly considered as fully established.

"One of the first things that strikes an inquirer into the structure of plants is the singular fact that while all species are capable of propagating their race, the mode in which this important function is accomplished is essentially different in different cases. The great mass of plants produce flowers which are succeeded by fruits, containing seed, which is shed or scattered abroad, and grows into new individuals. But in

Ferns, Mosses, Mushrooms, and the like, neither flowers, nor seeds properly so called, can be detected; but propagation is effected by the dispersion of grains or spores which are usually generated in the substance of the plant, and seem to have little analogy with true seeds. Hence the vegetable world separates into two distinct groups, the Flowering and the Flowerless. Upon examining more closely into the respective peculiarities of these two groups, it is found that flowering plants have sexes, while flowerless plants have none; hence the former are called Sexual and the latter Asexual. Then again the former usually possess a highly developed system of spiral or other vessels, while the latter are either altogether destitute of them, or have them only in the highest orders, and then in a peculiar state: for this reason flowering plants are also called Vascular, and flowerless Cellular. More than this, all flowering plants when they form stems, increase by an extention of their ends and a distention or enlargement of their sides; but flowerless plants appear to form their stems simply by the addition of new matter to their points; for this reason, while the former are principally Exogens or Endogens, the latter are called Acrogens. Flowering plants are also for the most part furnished with respiratory organs or stomates, while flowerless plants are to a great extent destitute of them. No one then can doubt that in the vegetable kingdom, two most essentially distinct divisions exist, the Flowering and the Flowerless, and that these differ not in one circumstance only, but are most essentially unlike in many points both of organization and physiology.

"In like manner, Flowering plants are themselves divisible into equally well-marked groups. Some of them grow by the addition of the new woody matter to the outside of their stem beneath the bark; these are Exogens: others grow by the addition of new woody matter to the inside of their stem near the centre; these are Endogens. But Exogens have two or more cotyledons to their embryo, and hence are called Dicotyledons; while Endogens have only one cotyledon, and are, therefore, Monocotyledons. Exogens have the young external wood connected with the centre by medullary

processes; Endogens, having no occasion for such a provision, are destitute of it. In Exogens the leaves have their veins disposed in a netted manner; in Endogens the veins run parallel with each other. The number of parts in the flower of an Exogen is usually five or its multiples; in an Endogen it is usually three or its multiples. In germination the young root of Exogens is a mere extension of the radicle; but of Endogens it is protruded from within the radicle; hence the former have been named *Exorhizæ*, and the latter *Endorhizæ*. In this case, then, as in the last, we have two groups differing entirely from each other in their germination, the structure of their stem and leaves, their mode of growth, the arrangements of the parts of the flower, and in the organization of their embryo. It is impossible, therefore, not to recognize such groups also as natural.

"To this separation of the vegetable kingdom into Exogens, Endogens, and Acrogens, or by whatever synonymous names these groups may be known, many botanists confine themselves. But there are two others, of subordinate importance, perhaps, but nevertheless characterized by circumstances of a similar nature, and therefore, I think, to be esteemed of equal dignity with them. In true Exogens and Endogens, the fertilizing principle is communicated to the young seeds through the medium of a stigma which terminates a case or pericarp in which they are inclosed. But in some plants otherwise Exogens, the fertilizing principle of the pollen is applied immediately to the seeds, without the intervention of any pericarpial apparatus, and they bear the same relation to other Exogens as frogs and similar reptiles to other animals. These plants, therefore, are separated as a distinct class, under the name of Gymnosperms. Like the other groups of the same grade, these are also found to possess peculiarities of a subordinate nature. For instance, they have in many cases more cotyledons than two, whence they have been called Polycotyledons; their radicle usually adheres to the albumen in which the embryo lies, and that circumstance has given rise to the name *Synorhizæ*. The veins of their leaves, when they have any veins, are either

simple or forked; in which respect they approach Endogens on the one hand, and Acrogens on the other. And finally, their vascular system is very imperfect compared with that of other Exogens of an equal degree of development.

"The other group, called *Rhizanthæ*, is far less correctly known, but it seems to stand as it were between Endogens and Acrogens of the lowest grade; agreeing with the latter in the absence or very imperfect state of the vascular system, in a general resemblance to Fungi, and in the apparent seeds being mere masses of sporules; but apparently according with Endogens in the ternary number of their floral envelopes, and in the presence of fully developed sexes.

"Certainly there is no possibility of obtaining such important primary groups as these by any kind of artificial contrivance." (*Preface*, pp. x.–xii.)

The grand natural divisions of the natural kingdom are, therefore, perfectly obvious, and may be very clearly defined. With our present knowledge of vegetable structure no great difficulty is experienced in characterizing the orders of natural families, and all subordinate groups. The great desideratum has ever been to effect such an arrangement of the orders under the primary classes that each family should be placed next to those which it most nearly resembles. This might easily be accomplished, if the idea once so strongly insisted upon by poets and metaphysicians, of a chain of beings, a regular gradation, by a single series, from the most perfect and complicated to the most simple forms of existence, had any foundation in truth. On the contrary, nothing is more evident than that almost every order, or other group, is allied not merely to one or two, but often to several others, which are sometimes widely separate from each other; and, indeed, these several points of resemblance, or affinity, are occasionally of about equal importance. A truly natural lineal arrangement is therefore impracticable, since by it only one or two out of several points of agreement can be indicated. As this method is, however, the only one that can be followed in books, all that can be done is to arrange the orders in such a manner as to offer the least possible interruption to their

natural affinities. The number of orders is so large that practical convenience seems to require their arrangement into groups subordinate to the primary classes; and when manifestly natural assemblages cannot be recognized, we are obliged to employ those which, being less strongly marked, and distinguished by a smaller number of characters, are apparently of a more artificial nature. The arrangement employed by the learned Jussieu, in his celebrated "Genera Plantarum," although to a considerable extent artificial, has been almost universally adopted, until within the last few years.

In this method Dicotyledonous plants are primarily divided into three groups: the first including those with a polypetalous corolla; the second, those with a monopetalous corolla; and the third, those destitute of a corolla. These sections are subdivided (as also the Monocotyledons) by means of characters taken from the insertion of the stamens (or corolla), whether hypogynous, perigynous, or epigynous. The arrangement here pursued, which is too well known to require further notice, is substantially adopted by De Candolle, the difference being more in appearance than in reality. Dr. Lindley discarded these subdivisions in the first edition of his work; but the new distribution of the orders therein proposed possesses few advantages, and indeed seems not to have satisfied the author himself. In the same year with the publication of the work just mentioned, the "Ordines Plantarum" of Bartling appeared, in which a more natural arrangement of the orders is attempted by the formation of aggregate or compound orders, as originally proposed, and in several instances successfully accomplished, by Robert Brown. An analogous plan was pursued by Agardh in his "Aphorismi Botanici" (1817), and again in his "Classes Plantarum" (1825); but these attempts, however ingenious, do not seem to have obviated, in any considerable degree, the inconveniences of lineal arrangement.

We now return to our author, whose views upon this subject have been materially modified since the original publication of his Introduction of the Natural System. The method

now employed was first sketched in the "Nixus Plantarum" (1832), and afterwards in the "Key to Structural, Physiological, and Systematic Botany"[1] (1835), and is more fully developed and illustrated in the work before us. He now admits, as we have already seen, five primary classes, two of which, however, are much smaller than the others and of subordinate importance, and may be considered as transition classes, namely, *Gymnospermæ*, which connect Exogens with the higher Acrogens, and *Rhizanthæ*, which form the transition from Endogens to Acrogens of the lowest grade. The great class *Exogenæ* (*Dicotyledones* of Jussieu) is divided into three subclasses, namely: —

1. *Polypetalæ;* those with the floral envelopes consisting of both calyx and corolla, the latter composed of distinct petals.

2. *Monopetalæ;* those with the petals combined in a monopetalous corolla.

3. *Incompletæ;* those always destitute of a corolla, the calyx also often incomplete or absent.

Thus far this mode of subdivision is nearly the same with that of Jussieu; Dr. Lindley, however, neglecting altogether the character afforded by the insertion of the stamens, divides the polypetalous orders into seven, and the *Monopetalæ* and *Incompletæ* each into five sections or groups. As a specimen of this plan, we copy the names of the groups of the first subclass, with their synoptical characters.

1. *Albuminosæ.* Embryo very considerably shorter and smaller than the albumen.

2. *Epigynosæ.* Ovary inferior, usually having an epigynous disk.

3. *Parietosæ.* Placentation parietal.

4. *Calycosæ.* Calyx incompletely whorled; two of the sepals being exterior.

5. *Syncarposæ.* None of the characters of the other groups, and with the carpels compactly united.

[1] This excellent little work consists of an augmented edition of the author's Outlines of the First Principles of Botany, with a revised translation of the Nixus Plantarum.

6. *Gynobascosæ*. Carpels not exceeding five, diverging at the base, arranged in a single row around an elevated axis or gynobase. Stamens usually separate from the calyx.

7. *Apocarposæ*. None of the characters of the other groups, but with the carpels distinct, or separable by their faces, or solitary.

Next, every group is divided into smaller groups, each of which includes one, two, or several orders. These minor groups are called *Alliances*, and are distinguished by the termination *ales*. Thus, under the Albuminose group, we have : —

Alliance 1. *Ranales*, comprising the *Ranunculaceæ*, *Papaveraceæ* (with its suborder, as Lindley, following Bernhardi, considers it, *Fumarieæ*), *Nymphæaceæ*, (to which *Hydropeltideæ* is improperly joined), and *Nelumbiaceæ ;*

Alliance 2. *Anonales*, which comprehends the Nutmeg tribe, the *Anonaceæ*, *Magnoliaceæ*, etc. ;

Alliance 3. *Umbellales*, including the Umbelliferous tribe, with the nearly allied *Araliaceæ ;*

Alliance 4. *Grossales*, consisting chiefly of the *Grossulaceæ* or currant tribe ; and lastly,

Alliance 5. *Pittosporales*, which strikes us as a singularly heterogeneous assemblage, bringing together into one group the *Vitaceæ*, *Pittosporaceæ*, *Olacaceæ*, *Francoaceæ*, and *Sarraceniaceæ*.

All the subclasses and groups, both of Exogens and Endogens, are subdivided in a similar manner ; but we cannot here proceed further with our enumeration. It will be borne in mind that the chief object of an arrangement of this kind is to facilitate the study of the natural orders, by dividing the extensive primary classes into sections of convenient size, and to dispose these groups, and the orders they comprise, as nearly in accordance with their respective affinities and relationships as a lineal arrangement will allow. It is impossible, in the present state of our knowledge, to say how far the views of our author will ultimately be approved. Every attempt of the kind must necessarily be very imperfect, so long as the structure of only a limited portion of the whole vegetable kingdom has been attentively and completely examined ;

and the author is well aware "that this part of the work will require many great changes and improvements before it can be considered at all established." Notwithstanding the objections to which it is liable in many particulars, we agree with the author in the opinion, "that even in its present state it will be found to be attended with many advantages, and that every step which may be taken in determining the limits of the natural groups subordinate to the primary classes must be a decided gain to the science. So rapid is the advance of our knowledge of the vegetable kingdom, and so numerous are the new types of structure that present themselves to the systematic botanist, that it is to be feared lest another chaos should be brought on by the masses of imperfectly grouped species with which the science will soon abound."

The names of natural orders, as first established, do not appear to have been framed in accordance with any uniform rule, as to derivation or mode of termination. They were sometimes intended to express some characteristic feature (Ex. *Leguminosæ*, *Labiatæ*, *Cruciferæ*, *Umbelliferæ*, *Coniferæ*, etc.), but more commonly some genus was selected as the type of the family, which was designated either by the plural of the genus simply (as *Myrti*, *Lilia*, *Irides*, *Euphorbiæ*), or with a slight prolongation (as *Orchideæ*, *Jasmineæ*, etc.), or with the termination still further modified (as in *Cyperoideæ*, *Aroideæ*, *Borragineæ*, or *Ranunculaceæ*, *Rosaceæ*, *Cucurbitaceæ*, etc.). The derivation of the name of the order from some prominent genus is now the universal practice; and for the sake of uniformity as well as to distinguish such names from those of genera in the plural number, the termination *aceæ* is given to orders, and that of *eæ* to suborders, etc. The advantages of uniformity in this respect is manifest, and Dr. Lindley therefore insists upon the adoption of the rule in all cases. In the "Key to Botany," published the year previous to the appearance of the second edition of the present work, the termination in *aceæ* is employed, not only in the names of orders formed from those of genera, but also in the few still in use which relate to some peculiarity in the habit of the family. Thus instead of *Cruciferæ*, *Umbelliferæ*,

Coniferæ, etc., we have *Cruciaceæ*, *Umbellaceæ*, and *Conaceæ*. These are, however, very properly abandoned in the work before us, in which the author inclines to give up the old and familiar names of these orders, and to substitute those formed in the customary manner from well-known genera. *Brassicaceæ*, *Apiaceæ*, and *Pinaceæ* may certainly be as good names as any other when we once get accustomed to them, but it seems hardly necessary to make any change in names of this kind. Dr. Lindley, as we have already seen, gives to the names of Alliances the termination *ales*, and to groups that of *oseæ*. The chief advantage of this system is, that the name of any group at once indicates its rank and importance.

The value of this work is greatly increased by the complete list of genera (so far as known at the time of publication), with the principal synonyms, appended to each order and properly arranged under their several suborders, sections, etc. This laborious and difficult task is upon the whole very faithfully executed. We observe, however, several errors, typographical and otherwise, which are not noticed in the appendix; and in a few instances the same genus is referred to two different orders. The whole catalogue will doubtless be rendered more perfectly accurate in a future edition.

The whole number of genera comprised in this enumeration, exclusive of synonyms, is 7840. Sprengel's "Systema Vegetabilium," which was finished in 1827, contains (exclusive of the appendix) only 3593 genera, or not quite half the number now known; while the twelfth edition of "Systema Naturæ" (the last of Linnæus himself) comprises 1228 genera, or only about a third more than are now known in a single family.

This great and rapid increase is perhaps chiefly owing to the discovery of new plants; but it is also attributable in a good degree to the more accurate knowledge of those already known. In either case, it is the natural result of the progress of discovery; and instead of embarrassing the student, as is often supposed, does in reality render the study of the science much more clear and satisfactory. Notwithstanding the great increase of genera within the last few years, it may

be safely said that at no previous period could a really useful knowledge of the vegetable kingdom be acquired with so little labor. In hazarding this remark, it is of course taken for granted that the student will avail himself of all the advantages of modern physiological botany and of the natural system: for so rapid has been the discovery of new and strange forms of structure, for which the artificial arrangement of Linnæus makes no provision, that the student who takes that system as his guide has indeed a hopeless task before him.

The essential characters of the orders appear to have been very carefully revised in this edition, as also the remarks upon their affinities, geographical distribution and sensible properties. Did our limits allow, we might call the attention of our readers more particularly to this part of the work. We cannot bring our remarks to a close, however, without suggesting what we consider a very desirable improvement upon the manner in which the seed is described, not only in this, but in almost all modern systematic works. It is very necessary that an organ which affords such important characters, both as to its situation in the fruit, and particularly as to its internal structure, should be described with the greatest possible clearness and precision, and in a uniform manner. The prevalent fault of which we complain is thus noticed, as long ago as the year 1811, by that most acute botanist, the late L. C. Richard.

"Cæsalpinus, Adanson, Jussieu, and Gærtner, always take into view the direction of the embryo relative to the pericarp merely. This method appears to me improper: first, because it does not indicate with precision that direction which is most important to be understood; secondly, because the pericarpic direction of the embryo is often difficult to be ascertained, and is sometimes variable or even wholly different in the seeds of the same fruit. I have already shown by numerous examples in my 'Analyse du Fruit,' that the best method is to indicate the direction of the seed relative to the pericarp, and of the embryo relative to the seed."

In very many descriptions, the direction of the embryo relative to the seed can only be inferred from the pericarpic

direction, or, which is still more objectionable, the same structure is described by very different language in different instances, thus rendering unnecessarily complicated an investigation which of itself is not usually difficult. We may adduce as an example the five orders comprised in the alliance *Ranales*, which stands at the commencement of Dr. Lindley's treatise. We have no means of ascertaining, from the essential character of any one of these orders, either the spermic direction and position of the embryo, or the situation of the chalaza and micropyle relative to the hilum, from which the former may be inferred. It is commonly stated that the embryo is situated at the base of the albumen; but it is not specified whether the radicle is next the hilum (as in *Papaveraceæ*, *Nymphæaceæ*, etc.), or points in the opposite direction (as in *Nelumbiaceæ* and *Cabombaceæ*); a matter of essential importance, since the seeds result in the one case from the ripening of anatropous, and in the other of orthotropous, ovules.

The students of botany in this country are greatly indebted to the learned editor and the enterprising publishers of the first American edition of this work. May we hope to have our obligations increased by the reprint of this greatly improved edition?

DE CANDOLLE'S PRODROMUS.

The second part of the seventh volume of De Candolle's "Prodromus"[1] — with which our notices may appropriately commence — was published at the very close of last year, and comprises the following orders, namely, *Stylideæ*, *Lobeliaceæ*, *Campanulaceæ*, *Cyphiaceæ* (a very small order, founded on the Cape genus Cyphia, and here first proposed by Alphonse De Candolle), *Goodenovieæ*, *Gesneriaceæ*, *Napoleoneæ*, *Vaccineæ*, *Ericaceæ*, *Epacrideæ*, *Pyrolaceæ*, *Francoaceæ*, and *Monotropeæ*. Of these, the *Lobeliaceæ*, *Campanulaceæ*, and *Cyphiaceæ* were elaborated by Professor Alphonse De Candolle, the well-known son of the distinguished author; the *Vaccineæ* by Professor Dunal of Montpelier; and the tribe *Ericeæ* (the Heath-tribe) was prepared by Mr. Bentham. It will be observed that De Candolle has disposed the *Ericaceæ* nearly in the manner first proposed in the "Théorie Elémentaire," considering the *Vaccinieæ*, *Monotropeæ*, *Pyrolaceæ*, etc., as so many distinct families; a view, however, which will not probably be ultimately adopted. Among the uncertain or little known Ericaceous plants, De Candolle has introduced the genus Pickeringia of Nuttall (which was founded upon *Cyrilla paniculata* of the same author, published in the fifth volume of this Journal); this, however, has been long since ascertained to be a species of Ardisia, which belongs to a very different order; and Mr. Nuttall has accordingly recently dedi-

[1] De Candolle. *Prodromus Systematis Naturalis Regni Vegetabilis*, etc. Pars VII., sectio ii. Paris, 1839 (American Journal of Science and Arts, xxxix. 168). — Pars VIII. Paris, 1844 (Ibid., xlvii. 198). — Pars IX. Paris, 1845 (Ibid., 2 ser., i. 174). — Pars XI. Paris, 1847 (Ibid., 2 ser., v. 449). — Pars XII. Paris, 1848 (Ibid., 2 ser., vii. 309). — Pars XIII., sectio ii. Paris, 1849 (Ibid., 2 ser., viii. 300). — Pars XIV., sectio ii. Paris, 1857 (Ibid., 2 ser., xxv. 290). — Pars XVI., sectio i. Paris, 1870 (Ibid., 2 ser., xlvii. 125). — Pars XVII. etc. Paris, 1873 (Ibid., 3 ser., vii. 66).

cated to Dr. Pickering a curious Leguminous plant from California. The genus Galax, De Candolle has appended to *Pyrolaceæ* (tribe *Galaceæ*), a view which seems to be confirmed by an unpublished plant from the mountains of North Carolina, which, in compliment to an assiduous and well-known American botanist, will bear the name of *Shortia galacifolia*.

The prior portion of the seventh volume (published in 1838), as well as the whole of the fifth (1836) and sixth (1837), is exclusively devoted to the immense family of the *Compositæ* (the class *Syngenesia* of Linnæus), which fills more than 1700 closely printed pages, the immediate preparation of which occupied the indefatigable author for seven years! We may take this family as a fair example of the increase in the number of known species within the last eighty years. The whole number of Syngenesious plants described by Linnæus in the first edition of the "Species Plantarum" (published in 1753), including the few *Compositæ* referred to other classes, is 555, which is about 150 less than the now described species of the single genus Senecio. We have not time nor space to enumerate the species of the order in succeeding systematic works, so as to show the progressive increase. Suffice it to say that the whole number known to Linnæus and published during his lifetime cannot exceed 800 species, while the number described by De Candolle is in round numbers about 8700, which are disposed in 893 genera. If to these we were to add the species which have been since published, or are being published in works now in progress, and also the very numerous unpublished species which exist in all large collections, making at the same time reasonable allowance for nominal species, the number of *Compositæ* at present known would scarcely fall short of 10,000, which considerably exceeds the whole number of both flowering and flowerless plants described by Linnæus or his contemporaries. Of the 8700 species given by De Candolle, more than 3000 are described for the first time in this work. In the general disposition of the order, the clear and simple classification of Lessing is to a great degree adopted. It is first divided into three great series, namely: —

1. TUBULIFLORÆ; those with the perfect flowers tubular and regularly five (or rarely four) toothed.

2. LABIATIFLORÆ; those with bilabiate, or two-cleft, perfect flowers.

3. LIGULIFLORÆ; which have all the flowers strap-shaped.

The first series includes about four fifths of the whole family, which are arranged in five tribes, namely, *Vernoniaceæ*, *Eupatoriaceæ*, *Asteroideæ*, *Senecionideæ* and *Cynareæ*. The second series consists exclusively of the *Mutisiaceæ* and the *Nassauviaceæ*, chiefly South American plants; a single species of Chaptalia is, we believe, the only North American representative. The third series, comprising the *Chicoraceæ*, so readily known by their milky juice, and by having all their florets ligulate, contains many North American representatives.

So many orders or separate genera of Monopetalous plants have been the subject of recent monographs, and so much valuable assistance is also engaged for the ensuing portions of the "Prodromus," that several volumes may be expected at no very distant period. It may not be improper to state that Mr. Boissier of Geneva is engaged in the preparation of the *Plumbagineæ;* Mr. Duby of Geneva will prepare the *Primulaceæ;* Professor Dunal of Montpelier, the *Solaneæ;* Mr. Decaisne of Paris, the *Asclepiadeæ;* and Mr. Bentham, the *Scrophularineæ* and *Labiatæ*.

We can at length announce the publication of this important volume (viii.); the first of the series under the editorship of the son of the great Genevan botanist,[1] and which he has appropriately dedicated to the memory of his illustrious father. We are glad to state, that arrangements have been made to expedite the publication of the succeeding volumes. The printing of the ninth, it is said, has already commenced, and its appearance may be expected in the autumn of the present year. It will contain the *Loganiaceæ*, *Bignoniaceæ*, *Cyrtandraceæ*, *Sesameæ*, and *Borragineæ*, from the notes prepared by the late Professor De Candolle; the *Hydrophyllaceæ*, by Alphonse De Candolle; the *Gentianaceæ*, by Grise-

[1] The elder De Candolle died September 9, 1841.

bach; the *Polemoniaceæ*, by Bentham, and the *Convolvulaceæ*, by Choisy. The tenth volume will be occupied with the *Solanaceæ*, by Dunal, and the *Scrophularineæ*, by Bentham; in the elaboration of which orders, these two distinguished botanists are now actively engaged. The first order in the volume before us, the *Lentibularieæ*, is prepared by the editor. The North American species of Utricularia are distributed into three sections, namely: 1. Megacista, where the verticillate foliage is floated by inflated petioles; 2. Lentibularia, where the capillary segments of the submersed foliage are utriculiferous; and 3. Oligocista, where the leaves are few, undivided, and disappear after flowering; the roots strike into the soil or mud, and generally bear the utriculi, when these are present. *U. resupinata*, discovered by B. D. Greene, Esq., and first mentioned in the Massachusetts Catalogue of Plants and Animals, 1835, is wrongly placed by De Candolle among the yellow-flowered species of the second section. It has purple flowers and should stand next *U. purpurea*. The name *U. Greenei*, Oaks, in "Hovey's Magazine" must stand as a synonym, as there is no good reason for changing the prior name imposed by the discoverer.

The order of *Primulaceæ* is elaborated by M. Duby of Geneva, who follows Endlicher in the general distribution of the family. From some inadvertence, *Glaux maritima* is not cited as an American plant. *Naumburgia thyrsiflora*, Mœnch, = *Lysimachia thyrsiflora*, Linn., and *L. capitata*, Pursh. The *L. revoluta*, Nutt., is referred to *L. longifolia*, Pursh. The common Samolus, of the southwestern United States, which has smaller flowers than the true *S. Valerandi*, is referred to *S. floribundus*, HBK. *S. ebracteatus* is not noticed as a plant of the United States, although it is common along our southern borders, nor is it distinguished even as a subgenus, although, on account of its nearly free ovary and want of sterile filaments, a recent writer (M. Baudo, in Ann. Sci. Nat., Dec. 1843) has separated it, to form his genus Samodia. In the *Myrsinaceæ*, elaborated by the editor, we meet with two North American species, both natives of Florida, namely, *Myrsine Floridana*, A. DC.,

and *Ardisia Pickeringia*, Torr. and Gr. To the small order *Theophrastaceæ*, A. DC., our author has joined Jacquinia, a West Indian genus, one species of which extends into Florida. In the order *Sapotaceæ*, the editor has proposed one new North American species of Bumelia. In *Ebenaceæ* we have only our Persimmon. From this the order *Styraceæ* (embracing *Symplocineæ* and *Halesiaceæ* of Don) is distinguished chiefly by the position of the cells of the ovary opposite the lobes of the calyx. Hopea is kept as a mere section of Symplocos; including a dozen Asiatic species as well as our *S. tinctoria*.

The order *Oleaceæ* is published from the manuscripts of the late Professor De Candolle. The American species of Fraxinus still require the labors of a monographer.

The order *Jasmineæ* is made to comprehend Bolivaria (of which there is at least one Texan species) and Menodora; and the family *Bolivariaceæ* is shown to have been founded upon misconceived characters.

For the elaboration of the *Apocynaceæ*, we are indebted to the younger De Candolle. The only North American genera are Amsonia (is not *Echites Fraseri*, Roem. and Schultes, the *A. ciliata*, Walt.?), Apocynum and Forsteronia (*F. difformis*, DC., = *Echites difformis*, Walt.).

The order *Asclepiadeæ* has been very faithfully studied by Decaisne. All the North American representatives belong to the tribe of true *Asclepiadeæ*, with the exception of Gonolobus, of which we have several species (one of them, collected by Dr. Short, forms the new *G. tiliæfolius*), and one, or possibly two species of Chthamalia, Decaisne. *Metastelma Fraseri* is probably a native of the West Indies, not of Carolina. *Enslenia albida*, we notice, is about to be figured in the forthcoming volume of Delessert's Icones; as also is Podostigma. Acerates includes ten, chiefly North American species. Asclepias is reduced to forty-four species, all of which are American, and the greater part extra-tropical. We are happy to learn that the plates of the fifth volume of the "Icones Selectæ" of the liberal Delessert — chiefly devoted to the illustration of the eighth volume of the "Prodromus" — are already in the hands of the engraver.

A year ago we had the pleasure to notice the eighth volume of this indispensable work, the 1st of the series under the editorship of Professor Alphonse De Candolle. The ninth volume, now before us, was issued on the 1st of January last; and the forthcoming portions are in course of preparation under such favorable circumstances that we may now confidently look for the appearance of a volume a year, and for the full completion of this "Species Plantarum," according to the natural system, at no very distant period. We have already mentioned the arrangements that are made to secure this desirable consummation, and by which the work becomes as it were a series of separate monographs, prepared by the most skilful hands, under the superintendence of a common editor. Every botanist is aware of the improvement of the successive volumes as they appeared from the unrivalled hands of the elder De Candolle; and a further improvement is manifest in the later portions, elaborated or revised by his son, especially in the introduction of characters drawn from æstivation, placentation, the structure of the ovule, and other points which have only quite recently been turned to special account by systematic botanists. A particular account of a volume which is or soon will be in the hands of every working botanist, cannot be necessary, and we have not time at present for special enumeration. The ninth volume commences with the *Loganiaceæ*, by Alphonse De Candolle. The genus Cœlostylis, Torr. and Gr., is correctly reduced to Spigelia. Under this order we have a tribe created for the long-vexed Gelsemium, which we suspect is not yet finally at rest. Next follows the *Gentianaceæ*, elaborated by Grisebach, whose recent monograph of that family, which forms the basis of the present arrangement, was duly noticed in this Journal. The order *Bignoniaceæ* is edited from the manuscripts of the elder De Candolle; as are also the orders *Sesameæ* and *Cyrtandraceæ*, which last has been reduced by Mr. Brown to *Gesneriaceæ*. The *Hydrophyllaceæ* are elaborated by Alphonse De Candolle, in which, by attributing generic importance to the presence or absence of the appendages or nectariferous scales within the tube of the corolla, the number of genera is

perhaps too greatly increased. The *Polemoniaceæ* are admirably worked out by Bentham, who has reduced to sections of Gilia his Hugelia, Fenzlia, Linanthus, Dactylophyllum, Leptosiphon, Leptodactylon, and the Ipomopsis, Michx. The elaboration of the *Convolvulaceæ* by Professor Choisy does not appear to give entire satisfaction to botanists. The term "infelicissime intricatus" is perhaps still applicable to their family; and the genera are probably unduly increased in number. Of the *Borragineæ*, printed from the elder De Candolle's manuscripts, with valuable notes and additions by the editor, we have the first three tribes, namely, *Cordieæ*, *Ehretieæ*, and *Heliotropeæ*. But for the true *Borrageæ* we must wait until the appearance of the tenth volume, which is already in press.

The long-expected eleventh volume of this work was published at the close of November last, or rather in December, and is now in the hands of botanists throughout Europe and America. It comprises, first, the *Orobanchaceæ*, by Mr. Reuter of Geneva; the true genera of which are Epiphegus of the United States; Phelipæa, containing thirty-eight species of the Old World, and two of North America west of the Mississippi; Conopholis (for the *Orobanche Americana*, L.); Orobanche of over ninety species belonging to the temperate regions of the Old World; Boschniakia of two high northwest American and Siberian species; Clandestina of one south European species; Lathræa of two Old World species; and Anoplanthus, Endl. (Anoplon, Wallr.), which is made to embrace not only the three genuine species of North America, but also a separate section corresponding with Anblatum, Endl., of two species of Central Asia. The earlier recent name of this last genus is Gymnocaulis, Nutt.; the next is Anoplon, by Wallroth. This was changed by Endlicher to Anoplanthus, because there is an Anoplon in zoölogy — an insufficient reason — with which, however, we need not trouble ourselves, since all three must give way to the early and excellent name of Aphyllon, founded by Mitchell, and published in 1748, on the typical species afterwards called *Orobanche uniflora* by Linnæus. This name, the revival of which is

demanded not only by statute law, but also for the avoidance of the intricately conflicting names recently imposed, was adopted in the MSS. for this family prepared for the "Flora of North America," and also in the writer's "Manual of the Botany of the Northern United States." Besides *A. uniflorum*, we have in the West and North, *A. comosum*, and *A. fasciculatum*. To the Genera affinia vel dubia, Reuter appends Obolaria, which is correctly described, except that the insertion of the ovules over the whole inner surface of the ovary is overlooked; the present writer's illustration of this genus not having reached Geneva until after this family was printed off.

The great family of this volume, occupying almost four hundred pages, is the order *Acanthaceæ*, which is contributed by Nees von Esenbeck. This chiefly tropical or subtropical family, founded less than forty years ago upon a small number of genera and species, now ranks among the largest of the monopetalous series, and is arranged by Nees under two suborders, eleven tribes, and 146 genera (including those of the appendix). We have few *Acanthaceæ* in the United States, so that an analysis of the family would not interest our readers. We are pleased to find that the writer's reference of the *Ruellia justiciaflora*, Hook. (the Eberlea of Riddell), to the genus Hygrophila, R. Br. (vide Pl. Lindheim, p. 22, note), is confirmed by Nees von Esenbeck. It appears that the species is also Mexican, and had been already described by Schlechtendal. Our Ruelliæ belong to Dipteracanthus and Calophanes. Our *Dianthera Americana*, L., with its allies, is included in the large genus Rhytiglossa, established by Nees (in Lindl. Introd. Nat. Syst., ed. 2) a few years ago, on some Cape of Good Hope species, from which the author suggests that the American species may differ generically. However that may be, we urgently protest against this suppression of the old Gronovian and Linnæan genus, Dianthera, which was founded upon our species; and which name, although unaccountably overlooked by Endlicher, who is usually careful, as well as by Nees, who is careless, as to questions of priority, must surely be continued for the genus,

however bounded, which includes the *Justicia pedunculosa* and *J. humilis* of Michaux.

The remainder of the volume, contributed by Schauer of Breslau, comprises, first, *Phrymaceæ*, another of these inconvenient little orders established on a single genus of a single species, which, however, differs remarkably from *Verbenaceæ* as well as *Labiatæ*, by the monomerous ovary, with a single orthotropous ovule erect from the base of the cell, an embryo with the radicle superior, and convolute cotyledons; and finally, the *Verbenaceæ*, composed of three tribes (the *Verbeneæ*, *Viticeæ*, *Avicennieæ*), ten subtribes, and forty-two genera. The genus Verbena, with which we are principally concerned in the United States, comprises seventy-one species, besides a dozen dubious ones appended at the close.

We had nearly forgotten the small family *Myoporaceæ*, which is elaborated by Professor Alphonse De Candolle himself. These twelve genera are all Australian or Oceanic, except Bontia, which is a Caribbean genus of a single species.

The twelfth volume, which will contain the *Labiatæ* by Bentham, is promised for June.

The twelfth volume of the "Prodromus," delayed somewhat by the convulsions of the continent, will be welcome to botanists. It concludes the series of *Monopetalæ*, with the exception of the small family *Plantaginaceæ*, and the large one of *Solanaceæ*, which last, Professor Dunal — from whom it has long been due — appears to find by no means easy to elaborate. Of the present volume, all but one hundred pages are occupied by the *Labiatæ*, from the hand of the most unwearied and best of monographers, Mr. Bentham. The plants of this large order, arranged in 101 genera, are thrown into eight tribes, instead of the eleven in the "Labiatarum Genera et Species;" the *Satureieæ* now being made to comprehend the *Menthoideæ* and the *Melissineæ* of the earlier work, and the *Scutellarineæ* being merged in the *Stachydeæ*. The *Ocimoideæ*, comprising nineteen genera, are represented in extratropical North America solely by one or two species of Hyptis, which inhabit our southeastern frontiers.

Of the *Satureieæ* we have Mentha, Lycopus, a single Cu-

nila, Pycnanthemum (the whole seventeen species), an anomalous Satureia (*S. rigida*, Bartr.), three species of Micromeria; while to Calamintha is now referred the former *M. glabella* and *M. Nuttallii*, as well as the *Gardoquia Hookeri*, Benth., with the new *C. canescens*, Torr and Gr., MSS., and *C. Caroliniana*, Sweet (Thymus, Michx.); also two species of Dicerandra (of which *D. densiflora* is a new one from Florida); the California Pogogyne; Hedeoma, including *H. ciliata* (*Keithia ciliata*, Benth., Lab.); and Collinsonia, of which six species are recognized.

Of the tribe *Monardeæ* we possess a small portion of the vast genus Salvia, which is increased to 407 species; Audibertia of California, of six species; Monarda, of six species (*M. didyma* and *M. fistulosa* being retained nearly as in the earlier monograph); and Blephilia, of two species.

The tribe *Nepeteæ* affords us Lophanthus; a single Dracocephalum; and a Cedronella.

The tribe *Stachydeæ* furnishes Prunella, for which Alphonse De Candolle restores, with satisfactory reasons given, the orthography, Brunella; Scutellaria, of eighty-six species, sixteen of them North American; Physostegia, of which two species are admitted; Brazoria, Engelm. — Gray (in which the synonym "*Physostegia truncata*, Hook. Botanical Magazine, t. 3494," should be cited under *B. scutellarioides*, as shown in Chlor. Bor.-Amer., and not under *B. truncata*): Macbridea and Synandra, each of a single species; and Stachys (from which Betonica is now excluded), of 168 species.

The tribe *Prasieæ* belongs entirely to the Old World and to the Sandwich Islands; and the tribe *Prostantheræ* is exclusively Australian.

Of the *Ajugeæ* we have Isanthus, Trichostemma (in which a corrected view is taken of the inflorescence of the typical section), and Teucrium.

The small order, *Selaginaceæ*, contributed by Professor Choisy of Geneva, consists of eight almost exclusively South African genera, to which the sub-arctic genus Gymnandra is doubtfully appended. Of three Siberian species of this genus

two are found on our northwest coast, two are Himalayan, and one has recently been found at Aucher-Eloy in the mountains of Armenia.

The order *Stilbaceæ*, prepared by Professor Alphonse De Candolle himself, consists of three genera each of a single known species, and of one with five species; all of them natives of the Cape of Good Hope.

The *Globulariaceæ*, by the same author, comprises the typical genus, with eight species, and a new one of a single species; all of Europe and of Eastern Asia, except one in the Canary Islands.

The order *Brunoniaceæ*, also by De Candolle, contains a single genus of two Australian species; both made known by the prince of botanists whose name they bear.

Mr. Boissier, the most active and promising botanist of the Genevan school, has elaborated the *Plumbaginaceæ*. The tribe *Staticeæ* comprises six genera, namely, Ægialitis, R. Br., of the shores of eastern tropical Asia and Australia; Acantholimon, Boiss., of forty-two Central Asian species, and Goniolimon, Boiss., of seven North-Asian species, — both distinguished from the following by their capitate instead of filiform stigmas: Statice itself, reaching to one hundred and ten species; Armeria, with fifty-two species, and Limoniastrum, of two Mediterranean species. The Statice of our own coast, *S. Caroliniana*, Walt., Mr. Boissier distinguishes from *S. Limonium* by its fistulous scape, stricter branches, pyramidal instead of corymbose panicle, the distant one-flowered spikelets, and the very acute calyx-lobes; the Californian plant he introduces is a new species. The tribe *Plumbageæ* consists of the Siberian Plumbagella, the European and tropical Plumbago, the Abyssinian Valoradia, and the African and North Indian Vogelia.

In the Corrigenda to the volume we notice that Bentham has corrected the orthography of Trichostemma, so printed in the "Genera" of Linnæus, and by mistake in the "Labiatarum Gen. et Sp.," to Trichostema, as written by Gronvius, by Linnæus in the "Hortus Cliffortianus," and as the derivation requires.

This half volume (Pars XIII., sectio posterior) has appeared very nearly at the date announced for it, last autumn, when the twelfth was published. It is the second part, anticipating the first, which is to contain the *Solanaceæ* and the *Plantaginaceæ*, two families which will finish the Monopetalous series, as this begins the *Apetalæ* or *Monochlamydeæ*. It comprises the *Phytolaccaceæ*, *Salsolaceæ* (*Chenopodeæ*), *Basellaceæ*, and *Amarantaceæ*, elaborated by Moquin-Tandon of Toulouse, and the *Nyctaginaceæ*, by Professor Choisy of Geneva. Of *Phytolaccaceæ* we have in the United States, only *Petivera alliacea*, which grows in Florida (probably not in "Carolina"), *Rivina lævis* (to which we are surprised to see *R. portulacoides*, Nutt., joined), and *Phytolacca decandra*, which last is now so widely dispersed over the world that its native country is uncertain.

The large family of *Salsolaceæ* comprises 72 genera, disposed nearly as in Tandon's "Chenopodearum Enumeratio," in two suborders and seven tribes, most of which are further divided into subtribes. Our genera of the *Cyclolobeæ* (those with the embryo nearly annular) are Aphanisma, Nutt., a Californian plant discovered by Mr. Nuttall; *Teloxys aristata*, which is credited to us because Linnæus referred his *Chenopodium Virginicum* to *C. aristatum*, but it is doubtful if we possess the genus; Cycloloma (*Salsola platyphylla*, Michx.); Chenopodium, to which Tandon now reunites the greater part of his Ambrina (*C. ambrosioides*, *C. anthelminticum*, etc.), leaving in Roubieva only the original species, recently illustrated in this Journal by Mr. Carey; Blitum, to which the author now refers, as a section, his former genus, Agathophyton (*Chenopodium Bonus Henricus*, L.); Monolepis, Schrad. (*Blitum chenopodioides*, Nutt.); Atriplex, of which too many of the older species are credited to the United States; Obione, Gærtn., of which nine species are North American, including (apparently with sufficient reason) the Pterochiton, Torr., Grayia, Hook. and Arn., of a single species; Eurotia; a doubtful Kochia; a Corispermum (which Tandon seems not to know as also a native of this country); Salicornia, in which we have *S. herbacea?* *S. Peruviana* (Caro. Fraser), and *S.*

Virginica, to which last he evidently would refer *S. mucronata*, Bigelow, a name unknown to him (and he has also dropped, apparently by accident, the homonym of Lagasca, so that the point in which we are interested is not elucidated); Arthrocnemum (*A.? ambiguum* = *Salicornia ambigua*, Michx.) being still kept distinct. Of the *Spirolobeæ* we have in North America, Chenopodina, a genus newly founded for the *Chenopodium maritimum*, L., which was formerly referred to Suæda, besides which species Tandon also gives us *C. linearis*, Ell., which, however, he thinks may be a variety of *C. prostrata*, which again he thinks may not prove distinct from *C. maritima*, and *C. depressa* (*Salsola depressa*, Pursh); of Shoberia, we have *S. calceoliformis* (*Chenopodium calceoliforme*, Hook.), which is stated also to be found "near New York"; of Salsola, we have *S. kali* only. The singular genus Sarcobatus of Nees (the Fremontia of Torrey in the reports of Fremont's first and second journeys) is enumerated among the Genera exclusa, and said to be "dubiæ sedis." Probably the author had not seen the figure of the fertile plant published by Dr. Torrey. Acnida, following the aspect and inflorescence, is here referred to the *Amarantaceæ*.

The order *Basellaceæ*, familiar to us only by the *Boussingaultia baselloides*, which is cultivated as an ornamental climbing plant, contains six genera, entirely of tropical plants.

The order *Amarantaceæ* includes forty-five genera, arranged under three tribes. There are credited to this country: Celosia, one Californian species; Amaranthus, about nine species; Mengea of Schauer, a Californian species which has much the aspect of *Amarantus Blitum;* one or more species of Euxolus, Raf. (*Amarantus lividus*, L. etc.); Acnida, in which *A. rusocarpa* appears to be mixed up, in a manner that requires much investigation to unravel, with *Amarantus tamariscinus*, Nutt., which again, though entirely distinct from Acnida itself, nearly accords in character with Moquin-Tandon's section Montelia; Banalia, a new genus, one section of which includes an Oregon species (*Halomocnemis occidentalis*, Nutt., ined.); an obscure Polycnemum;

Gossypianthus, Hook., two Texan species; Iresine, two species; Alternanthera, one species (*Achyranthus repens*, L.); besides the *A.* (Cladothrix, Nutt.) *lanuginosa*, which Lindheimer and Wright find abundantly in Texas, and which will certainly stand as a separate genus, if a striking peculiarity in respect to its fruit, observed by Dr. Torrey, proves to be a normal condition. *Telanthera ficoidea* and *T. polygonoides* appear to be only introduced plants along our southern coast. Frælichia (Oplotheca, Nutt.) has three North American species. Phyllepidium of Rafinesque is not identified and probably never will be.

The remaining family, *Nyctaginaceæ*, includes eighteen genera, in three tribes. Of Mirablis, though no species are credited to us, we have one or more in Texas, as well as the three species of Nyctaginia, Choisy. Of Oxybaphus, six North American species are indicated; and the Peruvian *Alliona incarnata* comes also from California. Four species of Abonia are described, besides *A.?* (Tripterocalyx) *micrantha*, Torr., which Dr. Torrey has since raised to the rank of a genus. *Pisonia aculeata* is found on Key West. Boerhaavia furnishes us three or four species; and there shall remain some undescribed Texan representatives of the family.

The second part of the fourteenth volume of the "Prodromus" contains the *Thymelæaceæ* by Meisner, the *Elæagnaceæ* by von Schlechtendal, the *Grubbiaceæ* by De Candolle, resting merely on one of those outlying or anomalous genera which there is too great tendency to raise to ordinal importance, merely because the author knows not what to do with them, — and *Santalaceæ* by De Candolle. Of the first order we have only Dirca, peculiar to this country, and with no congener known. There is nothing to add respecting our three species of *Elæagnaceæ*. As to our few *Santalaceæ*, it is interesting to remark that one of our characteristic genera, Pyrularia (the Oilnut), is found to have two representatives in the Himalayas (Sphærocarya, Wall.), and apparently two more in southern India (Scleropyrum, Arn.). Also that a European species is introduced into our Comandra (the *Thesium elegans* of Rochel), and the genus itself shown to be hardly

distinct from Thesium. And Darbya, Gray, published in this Journal twelve years ago, is reduced to a subgenus of Comandra, to which we are not disposed to object. But we take the new species of true Comandra (*C. pallida*) to be a mere variety of *C. umbellata ;* which, by the way, we did not state to be eight or ten feet, but only as many inches in height. De Candolle thinks that the hairs which connect the anthers of Comandra, and of most Thesia also, with the perianth, belong to the latter, not to the former, as the generic name implies. Our own observations, and especially some made by Mr. H. J. Clark upon very young flower-buds, confirm this view. The discovery, announced in this Journal in 1854, that the striking genus Buckleya, Torr., is truly dichlamydeous in the female flowers, proves a capital fact for M. De Candolle ; who draws from it the confident inference that the floral envelope which in all other plants of the order occurs alone, and has the stamens opposite its lobes, is corolla and not calyx, and consequently so in the *Loranthaceæ* and *Proteaceæ* also. Our author's views are presented in detail in an article, "Sur la Famille des Santalaceæ," in the "Bibliothèque Universelle," published last autumn, and they appear wellnigh convincing. An analogous case is found in Zanthoxylum (only here the suppression is the rare case), *Z. Americanum* plainly wanting that which in *Z. Carolinianum* is the corolla ("Genera Illustr." 2, p. 148). Nyssa offers a good instance of the limb of a calyx so reduced as to have escaped notice until four years ago. For what to De Candolle seem to be petals (p. 622, note in char. of order *Santalaceæ*), were seen to be so, and the observations recorded in the fifth volume of the "Memoirs of the American Academy," p. 336, and afterwards extended in the "Manual Bot. U. S.," ed. 2, p. 162 (1856). It is singular that De Candolle should remain so uncertain of the place of Nyssa in the Natural System. If he will compare it and Mastixia with Cornus he will surely be convinced that Nyssa is a true Cornaceous genus. So of Cevallia, the true place of which our author seems not to know, although given in the "Flora of North America" many years ago, under the sanction (we may add) of the very

highest authority. Indeed, so plain is its relationship to Gronovia that Fenzl soon saw and corrected his mistake in referring the genus to *Calycereæ*. And if at this date any should doubt that these are Loasaceous plants, let them turn to the characters of Petalonyx, in the "Memoirs of the American Academy," v., p. 319.

Leaving these details, let us consider our pleasing prospect in respect to the continuation (at least through the *Dicotyledoneæ*) of the great work upon which the De Candolles, father and son, and other excellent botanists, have bestowed so much labor and talent. The great order of *Lauraceæ* was to have been included in the present volume. It would have extended the volume unduly. But, unfortunately, or fortunately, as the case may be, Professor De Vriese has gone to Java on a government mission without finishing the work; and the indefatigable Meisner now takes it in hand. It is to form the leading part of volume xv., the *Begoniaceæ* by De Candolle himself, and the *Aristolochiaceæ* by Duchartre being appended, and perhaps the *Euphorbiaceæ*, also by De Candolle, except the genus Euphorbia which Boissier undertakes. The sixteenth volume is intended to commence with the *Urticaceæ* proper, by Weddell, or the *Monimiaceæ* by Tulasne. We are pleased to learn that Professor Anderson of Stockholm is to elaborate the *Salicineæ*.

The other section of the sixteenth volume of De Candolle's "Prodromus" has just been issued. The two parts form indeed independent volumes, and are paged and indexed as such, so that for all time botanists will have to quote *D C.* Prodr. xvi. (I), p., etc., which is to be regretted, but there is no help for it. The present (prior) part, of 450 pages besides 65 intercalated ones, contains the *Buxaceæ* and some other plants excluded from the *Euphorbiaceæ*, by Dr. Müller; the *Empetraceæ* by Alphonse De Candolle himself (Empetrum reduced to one species, Corema of two, and a Ceratiola); *Cannabineæ* by the same (the *Ulmaceæ* and *Artocarpeæ* postponed not being ready), the *Urticaceæ* (i. e. the *Urticeæ*) by Weddell; *Piperaceæ* by Casimir De Candolle (the *Saurureæ* made a mere tribe, and the *Pipereæ* mainly included under Piper of

635 species, Peperomia of 389!); *Chloranthaceæ* by Solms-Laubach of Halle, reduced to three genera; and finally, *Garryaceæ* by the editor, comprising nine species of Garrya. It appears that the latter end of the volume was printed first, which explains the omission of *G. buxifolia*, a species discovered in northern California by Bolander, and published a year and a half ago. If one or two collaborators will now bring up their arrears, the editor may very soon have the great satisfaction of announcing the completion of the great Dicotyledonous series.

The seventeenth volume of De Candolle's "Prodromus" contains, in the first place, certain small outlying orders, or some of them genera that have to do duty as orders, which on various accounts have been left out of the "Prodromus" as it went on, namely, *Sarraceniaceæ*, *Phytocreneæ*, *Cardiopteridæ*, *Salvadoraceæ*, *Cynocrambe*, *Batidaceæ* (one Batis), *Lennoaceæ* (by Solms-Laubach), *Podostemaceæ* (by Weddell). Then *Nepenthaceæ*, by Dr. Hooker; *Cytinaceæ*, by the same; *Balanophoraceæ*, by Eichler; *Ulmaceæ*, by Planchon; *Moraceæ*, by Bureau; and a synopsis of the genera of *Artocarpeæ*. For the complete elaboration of the last family the volume has been a good while kept back, and has at length been issued, and (sad to say) the work concluded without it. It is to be hoped that whenever M. Bureau finishes his undertaking, the publishers of the "Prodromus" may print the *Artocarpeæ* uniformly with the rest, so that it may be appended.

Then follow a few pages of Genera omissa, with brief references, indicating that they are, or may be, so far as has been made out or conjectured; and finally, the wearied editor appends his "Prodromi Historia, Numeri, Conclusio." It is a terse and highly interesting account of this work, which (including the two preceding volumes of "Systema") occupied his celebrated and indefatigable father from the year 1816, or earlier, down to the end of his life, in 1841, and himself to the close of the past year: an enumeration of the contributors who have worked up particular families or genera; an enumeration of the orders, specifying the volume which contains

each, and the number of genera and species described, 5134 genera and 58,975 species, which the missing *Artocarpeæ*, it is estimated, would bring up to 5163 genera and about 60,000 species of Dicotyledones. Among a few statistical data which are given, the ten orders are enumerated which contain the greatest number of genera, beginning with *Compositæ* and ending with *Cruciferæ*. Then the ten which most abound in species, which begin with *Compositæ* (8561 species) and end with *Umbelliferæ* (1016). *Leguminoseæ* are the second in both lists, and next *Rubiaceæ* in the former and *Euphorbiaceæ* in the latter. But the long interval between the publication of many orders, say between *Cruciferæ* and *Euphorbiaceæ*, much diminishes the value of such comparisons. The reasons which have prevented a more rapid publication of the volumes of the "Prodromus," especially since the work has been largely distributed among collaborators, are hinted at; and finally the regrettable announcement is made that the publication is now relinquished, at the close of the Dicotyledones. A full Index, down to genera and their sections, filling 170 pages, closes this great work.

We sincerely congratulate the editor upon the successful completion of this great undertaking at the limits he felt obliged to prescribe, and thank him heartily for his long and faithful service and many sacrifices. As it may be hoped that he has still years of good work in him, all will regret that he could not bear this burden through a few of them, while a half dozen collaborators, who might be named, elaborate the Monocotyledonous orders. But, as he declares that he should doubtless perish under it, we prefer the living botanist to the completed "Prodromus." We may expect from him original work instead of editorial drudgery, perhaps a new edition of his Geographical Botany, or new researches upon the same subject, investigated with his impartial judgment, under the new light which was just dawning when that comprehensive treatise was published.

Since these remarks were written we have received an interesting pamphlet, separately issued from the "Archives des Sciences" of the "Bibliothèque Universelle" for November,

entitled, "Reflexions sur les Ouvrages Généraux de Botanique Déscriptive." In this M. De Candolle gives the history of the "Prodromus" and its forerunner with considerable fullness, explains more particularly his editorial trials and burdens, and the reasons why the work could not be made to get on faster, and gives his views as to the most practicable method of combining the labors of the botanist of another generation in the production of the new "Systema Vegetabilium" which will be demanded. An estimate is made of the time it must needs require, even with all the available monographers of the day enlisted in the service. The increased difficulties, or at least the augmented labor, of systematic botanical work, under the present demands of the science, are indicated. It appears that while in his father's time one could elaborate at the rate of about ten species a day, a faithful monographer now, under the modern requirements, can seldom exceed three or four hundred species per annum, that is, about a species a day! We suppose that the case on the whole is not overstated.

ENDLICHER'S GENERA PLANTARUM.

THIS is one of the most important works [1] of the age; and we are anxious to make it more generally known to the botanists of this country. It is not too much to say, that without this, and Lindley's introduction to the Natural System (or some equivalent work), no person who does not possess the advantage of a large library and an extensive general collection of plants, can obtain any correct idea of the present state of systematic botany. The work is published in parts, of eighty pages each, in an imperial octavo or a kind of oblong quarto form, closely printed in double columns. The eleventh fasciculus, which is the last we have received, reaches to page 880; but probably two or more additional numbers have by

[1] Endlicher's *Genera Plantarum secundum Ordines Naturales disposita.* Vienna, 1836–40. (American Journal of Science and Arts, xxxix. 176.)

this time appeared. It is stated in the original announcement that the work will not exceed ten or twelve numbers; we imagine, however, that four or five additional numbers will be required for its completion. It commences, like the "Genera Plantarum" of Jussieu, with the plants of the simplest or lowest organization (Thallophyta, Endl.); a plan which is now the most common and perhaps the most philosophical, but which is attended with many practical inconveniences to the tyro.

The first edition of the "Genera Plantarum" by Linnæus was published at Leyden in the year 1737; the second and third were published at the same place, the one in 1742, the other in 1752; the fourth and fifth were published at Stockholm; the latter (termed the sixth in our copy) in the year 1764, which is the last by Linnæus himself, is the edition generally cited, and was reprinted at Vienna in 1767. This last Stockholm edition forms the excellent model of all the succeeding editions, as they are termed, edited by various authors. It comprises 1239 genera, which in an appendix are reduced as far as possible to their proper natural orders. The first edition after the death of Linnæus is, we believe, that of Reichard, published at Frankfort in 1778, about the same time with the edition of the "Systema Plantarum" by the same author. To this succeeded the edition by Schreber (published also at Frankfort, 1789–1791, in two volumes), who is chiefly famous for having in this work changed all the unclassical names of Aublet and others for new ones made according to the Linnæan canons. Succeeding authors in plucking these borrowed plumes have despoiled him of some rightful feathers; as in the case of the genus Brasenia, for which most botanists have retained Michaux's name, Hydropeltis, which was published a dozen years later. The number of genera is here increased to 1769. About the same time (1791) an edition was published by Haenke at Vienna, which is apparently carefully digested. The latest edition of the "Genera Plantarum" which bears the name of Linnæus, and is arranged according to the artificial system, is that of Sprengel, and published at Göttingen in 1830 and 1831 (2 vols. 8vo),

which is the latest complete work in which the known genera are characterized. He gives the date of the publication of each genus, and references to the principal figures. The whole number of the genera described is 4159.

The "Genera Plantarum secundum Ordines Naturales disposita" of the immortal Jussieu, with which a new era in botany commenced, appeared in the year 1789. This work has never been reprinted in France, and but once out of it, and is now very scarce. Until the commencement of Dr. Endlicher's work, a period of about half a century, it has remained the only Genera Plantarum according to the natural system. There is but one living botanist upon whom the task of preparing a new Genera of Plants would seem most appropriately to devolve; but since it cannot be expected from that quarter, we are glad it has been undertaken, and we may almost say completed, by so learned and careful a botanist as Dr. Endlicher. The only fault we have to notice is, that there is no mode of distinguishing directly the generic characters, which are compiled altogether from preceding authors, from those drawn from the plants themselves. An author can only be considered responsible for the latter; yet unless there be some means of distinguishing those which have been verified from the remainder, he becomes somewhat implicated in the mistakes of his predecessors. Dr. Endlicher being scarcely less distinguished as a classical scholar than as a botanist, this work is a perfect model of the classical style.

Simultaneously with this work, which it is in part intended to illustrate, the author is publishing an "Iconographia Generum Plantarum." It appears in quarto parts, with about twelve uncolored plates in each, executed in a very superior manner, with full analyses, which leave nothing to be desired in this respect. Seven or eight parts are already published. It is the cheapest illustrated work of the kind with which we are acquainted, and at the same time one of the very best.

HARVEY'S SOUTH AFRICAN PLANTS.

This volume[1] was written, printed in very handsome style, and published at the Cape of Good Hope. It was prepared, not, as we might suppose, for the purpose of making Cape plants better known to European botanists, but for the use of the students and lovers of flowers at the Cape. It is arranged, moreover, according to the Natural System, and is throughout a work of genuine science. Truly, if popular botanical works, based on the Natural System, are deemed most advantageous for students at the Cape of Good Hope, we may indulge the expectation that this method will in due time be universally adopted in Europe and the United States. Mr. Harvey, who, while occupied with his duties as colonial secretary, has been enabled to do so much for the botany of that rich and interesting region, both by his own researches and by encouraging the labors of others, was requested to recommend some introductory work on botany.

Had a mere introduction to the elements of the science alone been needed, the desideratum might easily have been supplied. "But I soon found," says Mr. Harvey, "on cross-questioning, that something very different was required. One lady told me that she knew already what 'calyx, corolla, stamens, and pistils, and all that' meant; and another had penetrated the mystery of Monandria, Diandria, etc., and did not want to be told that over again; what they desired was a book in which they could discover the names of every plant that struck their fancy in rambling through the fields, — in short, a Flora Capensis. Here I found myself completely at fault, for there seemed little use in recommending the Flora of Thunberg, or the more ancient writings of Burmann; for even could they be procured, which would not be without much difficulty, they would have proved perfectly useless to my lady friends, who, not being blue-stockings, could have derived little instruction

[1] *The Genera of South African Plants*, arranged according to the natural system, by William Henry Harvey, Esq. Cape Town, 1838. (American Journal of Science and Arts, xxxix. 173.)

from the crabbed Latin in which they were written." Mr. Harvey then conceived the idea of writing a Flora Capensis; but it at once occurring that such a work must consume a long series of years in preparation, he decided upon rendering that more prompt, though less complete assistance, which a work like the present is calculated to afford. "The Genera of South African Plants" is the result of this determination; for which the author deserves the thanks, not only of the lady friends whose benefit he had chiefly in view, but of all the cultivators of botanical science. Although much more time would be required for its preparation, the work would have been more valuable had Mr. Harvey placed still less dependence on preceding authors, and drawn his characters, in every practicable instance, from the plants themselves; but only those who are accustomed to prepare their works in this manner are aware of the vast amount of labor it involves. The general plan of the work, as the author informs us, is taken from Beck's "Botany of the Northern and Middle States of North America," and Nuttall's "Genera of North American Plants"; in the arrangement and characters of the orders, Dr. Arnott has chiefly been followed. The number of genera described is 1086, distributed under 135 orders. Many South African genera have been published in still more recent general works or particular memoirs, or in those which have not reached the Cape in time to be employed by Mr. Harvey, so that the number of Cape genera may be safely estimated at 1200.

SIEBOLD'S FLORA OF JAPAN.

THIS work[1] is, we believe, wholly arranged and prepared by Professor Zuccarini of Munich, from notes and specimens furnished by Dr. Siebold of Leyden, accumulated during his

[1] *Flora Japonica.* Sectio prima, Plantæ ornatui vel usui inservientes, digessit Dr. J. G. Zuccarini. Fasc. 1–10. Leyden, 1835–39. (American Journal of Science and Arts, xxxix. 175.)

long official residence in Japan. The admirable plates are executed in Munich: they are engraved upon stone after a peculiar method, which is now frequently employed, and are certainly not excelled in beauty or accuracy by any copper-plate engraving in the same style. The portion already published comprises only the ornamental or otherwise interesting plants, the general account of the Japanese flora being reserved for a future part of the work. The flora of Japan presents such striking analogies to that of the temperate part of North America as to render this work of more than ordinary interest to American botanists. To show this, we select from the forty-six species described and figured by Zuccarini, the following list, placing opposite the Japanese plant the related North American forms.

FLORA OF JAPAN.	FLORA OF NORTH AMERICA.
Illicium religiosum,	*Illicium Floridanum* and *parviflorum*.
Kadsura Japonica,	*Schizandra coccinea*.
Benthamia Japonica,	*Cornus florida*.
Corylopsis, two species,	*Hamamelis* and *Frothergilla*.
Aralia edulis,	*Aralia racemosa*.
Symplocos lucida,	*Hopea tinctoria*.
Styrax Japonicum, etc.	*Styrax*, several species.
Deutzia, three species,	*Philadelphus*.
Schizophragma Hydrangeoides, *Platycrater arguta*,	*Decumaria* and *Hydrangea*.
Diervilla, several species,	*Diervilla Tournefortii*.
Viburnum tomentosum,	*Viburnum lantanoides*.
Wisteria (or, as it should be, *Wistaria*) *Japonica*, and two other species,	*Wistaria frutescens*.
Paulownia imperialis,	*Catalpa cordifolia*.

While about half the species thus far published are nearly related to (chiefly characteristic) North American plants, only eight, besides those given above, belong to genera which have no representatives in this country. The list might be greatly extended by comparisons from other sources. Thus *Hoteia Japonica* of Morren and Decaisne (which belongs to the earlier established Astilbe, Don.), which was by Thunberg mistaken for *Spiræa Aruncus*, closely resembles our own *Astilbe decandra*, which has been more than once confounded with

Spiræa Aruncus. On some future occasion we hope to make a somewhat extended comparison between the flora of temperate North America, and that of Japan and Middle Asia.[1]

Professor Zuccarini, the author, in conjunction with Dr. Siebold, of the excellent "Flora Japonica" now in progress, has recently published the first part of a brief memoir, entitled, "Floræ Japonicæ familiæ Naturales, adjectis generum et specierum exemplis selectis: Sect. i. Plantæ dicotyledoneæ polypetalæ." [2] It is interesting to remark how many of our characteristic genera are reproduced in Japan, not to speak of striking analogous forms. Thus the flora of Japan has not only Wistaria, Lespedeza, Sieversia, Chimonanthus (in place of our Calycanthus), Philadelphus, several species of Rhus closely resembling our own, and two peculiar genera of *Juglandeæ*, but also a Pachysandra, some Berchemias, a Staphylea, and a peculiar genus of the tribe (Euscaphis) besides; not only a dozen Maples, but also a Negundo, a Stuartia, two Tilias, a Phytolacca, an Opuntia (surely not indigenous), a Sicyos referred to our own *S. angulata*, two Droseras, a Nelumbium, a Nuphar, and two species of Nymphæa, Gynandropsis, a real Dicentra (Dielytra) and an allied new genus, with several species of Corydalis, a Trollius, our own Coptis and two new ones like the western *C. asplenifolia*, an Isopyrum, two species of Aquilegia, one of them near *A. Canadensis*, a Cimicifuga, a Trautvetteria, an Illicium, some Magnolias, Kadsura and Sphærostemma in place of Schizandra, a Mitellopsis, two species of Astilbe (Hoteia), many Hydrangeas as well as peculiar Hydrangeaceous forms, a Hamamelis with two other characteristic genera of the family, some true Dogwoods, as well as Benthamia the analogue of our *Cornus florida*, some true Vines, and two species of Am-

[1] This short paper is of peculiar interest. It contains the earliest record of Professor Gray's investigations into the flora of Japan, and its relations to that of eastern North America — investigations which many years later enabled him to explain the distribution of plants through the northern hemisphere by tracing their direct descent from ancestors which flourished in the Arctic region during the latest tertiary periods, and established his reputation as a philosophical naturalist. — C. S. S.

[2] American Journal of Science and Arts, 2 ser., ii. 135.

pelopsis, three species of Panax, and four of Aralia, one of which is near our *A. nudicaulis;* and among Umbelliferæ are Hydrocotyle, Sanicula, Sium, Angelica, but what is most remarkable, Cryptotænia, Archemora, and Osmorhiza. Further cases of generic conformity abound in the remaining divisions of the vegetable kingdom; thus, for example, Diervilla, Mitchella, Maclura, Liquidambar, Torreya, and Sassafras, are represented in the flora of Japan.

VEGETABLE MONSTROSITIES.

This interesting treatise[1] on Vegetable Monstrosities is very properly prefaced by a statement of what is meant by the normal structure of plants, by vegetable individuality, and vegetable symmetry. The author proceeds to consider, first, those slighter deviations which are called varieties; and secondly, those more grave and mostly congenital anomalies which bear the name of monsters. As to the latter the author remarks, that nearly every monstrous or abnormal condition that has been observed is to be met with as the normal state of other vegetables; and that between a monstrous and a normal flower, the only difference often is, that the former is the occasional, and the latter the habitual state. "La monstruosité est donc, en général, l'application insolite, à un individu ou à un appareil, de la structure normale d'un autre appareil ou d'un autre individu. C'est un organisation transposée, c'est une loi changée de place. On l'a dit avec raison, la monstruosité ne se trouve pas en dehors de la nature, mais seulement en dehors de la coutume." It is clear, therefore, that while abnormal states may always be explained by the laws which regulate the normal structure, monsters themselves, as the etymology of the name indicates, often show us the true structure when it could not be certainly inferred from the

[1] *Elémens de Tératologie Végétale, ou Histoire abrégée des anomalies de l'Organisation dans les Végétaux.* Par A. Moquin-Tandon. Paris, 1841. (American Journal of Science and Arts, xli. 374.)

habitual condition. The author arranges monstrosities under four primary classes: those of volume, of form, of disposition, and of number. These are divided, the first class into monsters by diminution of volume (*Atrophy*), and by augmentation (*Hypertrophy*); the second class into monsters by alteration of form, whether irregular (*Difformation*) or regular (*Pelorias*), and monsters by the transformation of one organ into another (*Metamorphosis*); the third class into monsters by the abnormal connection of parts, or by the disunion of parts habitually united, and into those caused by change of situation, or displacement; the fourth class into monsters by diminution of number, or abortion, and those by augmentation of number. Under these heads the monstrosities of the different organs of plants are considered in detail, and in a philosophical and very interesting manner. This brief notice of the plan of Moquin-Tandon's work, we are confident, will suffice to commend it to the attention of the botanists of this country.

AGASSIZ'S ZOÖLOGICAL NOMENCLATOR.

This great work,[1] which must have cost an extraordinary amount of labor, is now almost completed. Trusting that some one of our able zoölogists will duly give an account of a work which is indispensable to every votary of their science, we propose at this time merely to call attention to the preface, published last year with the ninth and tenth fasciculi, and to express unqualified admiration of the manner in which a subject of interest to all naturalists, that of nomenclature, is there treated. While botanists are enjoying the benefits of a sedulous adherence to the wholesome rules imposed by the father of natural-history nomenclature, and of nearly unanimous agreement in the few changes which the progress of science and the multiplication of its objects have rendered needful, the zoölo-

[1] *Nomenclator Zoölogicus, continens Nomina Systematica Generum Animalium, tam viventium quam fossilium*, etc. Auctore, L. Agassiz. Soleure, 1842–46. (American Journal of Science and Arts, 2 ser., iii. 302.)

gists on the other hand, who have too generally allowed every one to do that which was right in his own eyes, are reaping in consequence a plentiful harvest of confusion. The difficulty of a reform increases with its necessity. It is much easier to state the evils than to relieve them; and the well-meant endeavors that have recently been made to this end are some of them likely, if adopted, to make "confusion worse confounded." Probably no living zoölogist is so conversant as Professor Agassiz with the actual state of the nomenclature of the animal kingdom, and so well qualified to judge of the practical working of proposed rules, which often involve consequences that the propounders never dreamed of. Our author's views are therefore entitled to great weight. We are glad to perceive that they entirely concur with those quite unanimously adopted in the other great department of natural history for which the Linnæan canons were originally framed. As these canons were the foundations of our nomenclature, Professor Agassiz has very properly reproduced them, *totidem verbis*, from the "Philosophia Botanica," adding now and then a short but pithy commentary. He then proceeds to examine the rules proposed by the Committee of the British Association, and shows that while some of them are mere iterations of the Linnæan canons, which should never have lost their authority, others are contrary to them, or threaten greater evils than they are intended to remedy. In most respects his criticisms concur with those already made by Dr. Gould in a former volume of this Journal (XIV. p. 1). We agree with Professor Agassiz in thinking these English canons worthy of adoption only when they agree with the letter or spirit of the Linnæan rules, which indeed they generally do. Those which conflict with them have not received, and probably will not receive, the general assent even of British naturalists. Hence, in our opinion, the American Geological Association has too hastily reaffirmed them, while they have, indeed, improved their form in several respects. It may be well to notice the comments of Professor Agassiz upon the more objectionable propositions.

Their first rule, "that the name given by the founder of a

group or the (first) describer of a species should be permanently retained," cannot be too firmly insisted on; for upon it rests the stability which is the most essential requisite of nomenclature. Their second rule, that since "the binomial nomenclature originated with Linnæus, the law of priority is not to extend to the writings of antecedent authors," restricts the former too arbitrarily, and conflicts, as Professor Agassiz states, both with the canons and the example of Linnæus, not less than with the conscientious practice of good naturalists ever since. Linnæus was not the founder of genera or of generic nomenclature, and, "far from making new names in every instance, he retained all names given by his predecessors, provided they could be received into his system." It is generally thought that Linnæus erred by adopting, not too many, but too few of the unobjectionable and well-established generic names of his predecessors, such as Tournefort, etc. Now when, in the natural progress of the science, a Linnæan genus is resolved into two or more Tournefortian ones, for instance, are the names of Tournefort to be excluded from use? In the breaking up of the Linnæan genus Lonicera, had not the Diervilla and Xylosteum (and if the division were to go farther, the Periclymenum and Caprifolium) of Tournefort, as well as the Symphoricarpos of Dillenius, an indisputable right to restoration? Indeed Linnæus was here plainly wrong in not adopting one of these prior names for the whole genus, instead of creating the new one. This, however, was to be submitted to; for, as Professor Agassiz remarks, "the names sanctioned by Linnæus are to be held as established above all others. Linnæus, for instance, received very few genera of Echinodermata. Nowadays this class numbers many, among which some of those founded by Klein, Link and Breynius, long anterior to Linnæus, hold their place with the modern ones of Lamarck, Müller, etc. But no one now prefers that new names should be made for such genera, rather than that such approved anterior ones should be brought into use again. I certainly see no cause why we may not call to life the names of former authors when we divide the genera of Linnæus." We think those naturalists blame-

worthy who do not. The third, fourth, and fifth of the British canons are accordant with Linnæan rules, and are regularly followed in botany. The next four relate to matters which follow as a consequence of the law of priority; but as to what relates to the use of synonymous names Professor Agassiz intimates that their rule is perhaps too absolute, and even contradictory to the Linnæan canon, § 244: "Nomina generica, quamdiu synonyma digna in promptu sunt, nova non effingenda."

The tenth rule, namely, "A name should be changed which has before been proposed for some other genus in zoölogy or botany, or for some other species in the same genus, when still retained for such genus or species," is not as well worded as the equivalent Linnæan canon, § 217, "Nomen genericum unum idemque ad diversa designanda genera assumptum, altero loco, excludendum erit." Mr. Agassiz remarks, greatly to our surprise, that the enforcement of this rule would demand the sacrifice of almost half the generic names made in recent times. In our opinion, while the same names ought not to be given both in zoölogy and botany, the time is passed when received names are to be changed on this account. While writers in the different departments of zoölogy alone have doubly employed the same name "in ten thousand instances," we must see that cases of this sort between zoölogists and botanists, occupying such widely separated fields, are inevitable, at least until as perfect lists of zoölogical names shall be compiled and kept up as is done in botany. Besides, it is now utterly impossible for any single naturalist, or any joint committee of botanists and zoölogists, to determine, in half the cases that arise, whether a particular genus is to be suppressed or retained in one department, so as to require or forbid a change of the posterior homonymous name in the other; hence the practical application of the Linnæan rule would now create tenfold more confusion than it can relieve. Each well-founded change of the sort does no more than obviate a possible inconvenience, while every needless one, in a genus of numerous species, draws after it a load of useless synonyms, which do not

serve, like genuine synonyms, to tell the history of the genus and mark the progress of our knowledge. The whole subject is forcibly presented by Professor Agassiz, in another section of his preface (p. xxviii *et seq.*), where he states that he now knows three thousand generic names common to botany and zoölogy, which the Linnæan rule would require to be changed in one or the other department. But surely this number must comprise a host of synonyms long since laid on the shelf, as well as names of somewhat different formation or termination, although of the same derivation. In this case a small matter should give them impunity. If these changes must be made, no one could do the work for zoölogy better than Mr. Agassiz; but he affirms it to be a task quite beyond his power, and justly concludes that, "in the present state of the science, generic names ought not to be changed solely on account of their being employed in both kingdoms of nature." To this conclusion the American Association evidently accede.

As to generic names doubly or triply employed in the several classes of the animal kingdom (which, we are astonished to learn, already number nearly ten thousand), the necessity of applying the Linnæan canon is obvious, and would be imperative had not the evil reached such a height as to baffle remedy. The *summum jus* which demands the immediate change of nearly a moiety of the received zoölogical names would surely become *summa injuria* to the science, even if any naturalist were equal to the task of applying it. Justice must here be delayed in order that it may be rightly administered, and, as our author recommends, the business of gradually bringing this part of nomenclature under rule must be left to monographers and future systematists. But let those upon whom the *cacoëthes nominandi* is strong, obey our author's advice, desist from proposing new names in mere catalogues, and never attempt, while revising the genus which rightfully claims a particular name, to impose new names upon the homonymous genera in other classes, but leave that for their own respective monographers. It will be soon enough to give them new names, if such are needed, when the validity of these several genera is well made out.

Upon the 11th rule of the British Committee, namely, that "a name may be changed when it implies a false proposition which is likely to propagate important errors," Professor Agassiz remarks that the less this liberty is used the better, lest it should lead to licentiousness.

The 12th rule ordains that "a name which has never been clearly defined in some published work should be changed for the earliest by which the object shall have been so defined." This law, our author remarks, "has become very necessary, since dealers in natural objects have begun to arrogate the authorship of books collected from catalogues, and demand that authors shall receive their names for dividing species. It is the same with names which remain unpublished in public or private collections, and to which the proprietors or curators sometimes lay claim. But priority is to be conceded only to publication in a work which is accessible to the learned throughout the world. Yet while we strictly press the observance of this law in respect to the publication by learned men of the results of their observations, so much the more must we brand with infamy those impudent parasites who prowl about museums to pick materials for their *opuscula*, without mentioning the sources whence they have derived their spoil, and sometimes even furtively describing the species, the names of which they claim." The people alluded to well deserve this censure. On the other hand, not less blameworthy are those who purposely pass by, instead of courteously adopting, appropriate names under which naturalists often distribute their species in advance of publication. This felony is the more atrocious because remediless, and to be prevented by no rule except that of courtesy; for the public good requires that priority should be conceded to actual publication alone.

The two remaining laws (13th and 14th) are agreeable to, or identical with, Linnæan canons, and are approved by all good naturalists.

The rules recommended by the British Committee for the future improvement of nomenclature are next considered; and as they are far the most commendable and in general use

among good naturalists, we shall only notice those that Mr. Agassiz criticises, or we have occasion to comment upon. The writer of the British Report has chosen to enforce the direction, to avoid harsh and inelegant or sesquipedalian names, by citing, as an example of the kind, the "*Enaliolimnosaurus crocodilocephaloides* of a German naturalist;" for which he is strongly censured by our author, who declares that no naturalist has ever proposed this name. Surely, if one is inclined "to cast stones into his neighbor's garden," as our author says, there is no lack of legitimate opportunity, nor necessity for fabricating hard names.

The British Committee condemns the future employment of generic names which have been superseded by the rule of priority. But this is contrary to the canon, § 245 — "Nomen genericum unius generis, *nisi supervacaneum*, in aliud transferri non debet" (and to *obs.* under § 244), no less than to the practice of Linnæus and of subsequent naturalists. For instance, Saururus of Plumier became a synonym of Piper, but this did not debar Linnæus from the subsequent application of the name to a new genus. Sisyrinchium of Tournefort being included in Iris, Linnæus gave the name to a different genus; nor did he hesitate to adopt the genus which Ellis had dedicated to Hales, on account of an earlier Halesia of Browne, which had already sunk to a synonym. Why should a good name be forever tabooed in such cases, and why not, if occasion offers, allow it to be remarried to a new genus? We should be careful, however, not to reproduce names which are likely ever to be resuscitated in their former relation.

The British Committee objects to the practice of giving to a genus the name which it bore as a species of a former genus. But, as Professor Agassiz justly remarks, when a species, which proves to be the type of a new genus, has a good proper name already, it seems quite as admissible to take that name for the genus and make a new one for the species, as to coin a new generic name, since either way a new name must be introduced: indeed it is preferable, because such Linnæan species frequently are found to comprise several, hitherto confounded, no one of which has a paramount

claim to the specific name: *e. g. Cyprinus Gobio, C. Leuciscus, C. Barbus*, L. We go further, and maintain that the proper specific names are, *cæteribus paribus*, always to be preferred for genera in these cases, not only because that they are already familiar, but because they are most frequently old generic names which may claim under the law of priority. For example, *Lonicera Diervilla*, L. = *Diervilla*, Tourn.; L. *Symphoricarpos*, L. = *Symphoricarpos*, Dill.; *Rhamnus Paliurus*, L. = *Paliurus*, Dod.; R. *Zizyphus*, L. = *Zizyphus*, Dod.; *Rubus Dalibarda*, L. = *Dalibarda*, L.; and so of hundreds of proper specific names which have rightly resumed their generic rank.

The next proposition of the British Committee, namely, that specific names, even when substantive or borrowed from persons or places, should uniformly be written with a small (instead of capital) initial, is so contrary to long usage and offensive to good taste, that we are surprised that it should anywhere find favor. Mr. Agassiz pointedly condemns it. The only reason assigned for the change is, that some people might not be able to distinguish the specific from the generic name without the aid of typography. But, as Dr. Gould has already remarked in this Journal, such persons would be misled by almost anything; and the propounders of the rule should follow it consistently by writing their own cognomen with a small initial letter. We do not wonder that the Committee of the American Association refused to reaffirm this rule, as applied to proper names from persons; and we are quite sure that naturalists generally will not hesitate wholly to reject it; surely the committee would not approve the practice of a late botanical author of this country, who reduced the proper scientific names of Linnæus into adjective conformity, by writing "*Ranunculus flammulus*," instead of *R. Flammula*, "*Thymus serpyllus*" in the place of *Thymus Serpyllum*, and so on.

Professor Agassiz severely condemns the proposition to restrict the names of families to a uniform termination in *idæ*, and their subdivisions to *inæ*, without considering whether the words in question will receive that particular suffix kindly.

This is quite too straight-laced, and gives rise to many awkward forms, or

> "Sesquipedalia verba
> Vel nocitura sono, guttur læsura loquentis,"

which is not worth while to encounter needlessly, for the sake of mere technical uniformity, at least when they may be avoided by some liberty of choice in the mode of prolongation.

The proposition, D. of the British Committee, which directs that the name of the original propounder of a species should adhere to it when transferred to a different genus, is warmly defended by some naturalists in England and, in a modified form, in our own country also. Few naturalists are now so well qualified to judge of the practical operation of this scheme as Professor Agassiz. He declares his opinion that, if received, "it will introduce horrible and remediless confusion," and that no possible multiplication of synonymy is likely to lead to so many difficulties as this new practice. He therefore strenuously opposes it by arguments drawn from the precepts and practice of Linnæus, who meant the specific name to be subordinate to the generic, and never intended it to be inferred that he who applied to a plant or animal a certain name was therefore its discoverer, or even its first systematic describer. He affirms that Linnæus would have expressly rejected "*Tyrannus crinitus*, Linn. (*sp.*)," were the innovation proposed in his day, and have written *T. crinitus*, Swains., had he thought best to approve the division of his genus Muscicapa. On the other hand, if he disapproved the division, we may add, he would not have thanked a contemporary for making him seem to adopt it. The hardship is still greater when the question is not of the division of an old genus, but of the proper place of a species among admitted genera, when it is surely improper to cite an author as referring to one genus, while he expressly maintains that it belongs to another. In fact, the remedy is much worse than the disease which the English doctors would cure. Linnæus maintains that he is the true naturalist who understands

genera; but from the new practice it will inevitably follow, as Professor Agassiz asserts, that the proper establishment and definition of genera which demand the highest powers of the naturalist, will be less esteemed than the mere distinguishing of species; a result which, far from promoting science, will especially retard the progress of that part of zoology in which there is most to be done, and in which the science of the animal is still far behind that of the vegetable kingdom. It is of the greatest importance that we should be able to thread our way back through entangled synonymy and mistaken references to the original sources. Here our difficulties would be greatly multiplied, unless two sorts of synonyms are used. For who, as Mr. Agassiz says, can find out what Linnæus has said of *Muscicapa crinita*, without a direct reference to the genus in which Linnæus himself placed it? And when, as often happens, the Linnæan species is mistaken, so that the *Tyrannus crinitus*, Linn. (*sp.*) according to Swainson, is not the *T. crinitus*, Linn. (*sp.*) according to some other author, the confusion becomes inextricable, unless we encumber ourselves with two modes of annotation, the old for expressing synonymy, and the new for the names really adopted. "Then the two modes will not agree with each other, nor can one know whither to turn himself. Surely the authors of this new rule cannot have considered these inconveniences, else they would have themselves discarded it. Therefore I entreat and pray them, by all the interests of the science they wish to promote, to abandon their proposition, and not to introduce a new schism into natural history, but to return again to the system of Linnæus, the most simple of all, and the least likely to errors and Babylonish confusion in nomenclature."

The Committee of the American Association more wisely adopted the mode, afterwards employed by Mr. Dana in his great work on Zoophytes, namely, that of appending to the specific name the original authority for the species in brackets, and adding without brackets the name of the author who first described the species under the later received genus. To this plan there can be no objection, except that

it is rather cumbrous, if it is to be used in every brief mention of the species, and, in our opinion, quite superfluous in a systematic treatise, where the synonymy is given in proper historical order. The recommendation to make subgeneric names agree in gender with that of the genus, Professor Agassiz thinks is of no consequence, unless the new annotation, just animadverted upon, should come into use. Besides, it would often interfere with the rule of priority, which requires synonyms, when they exist, to be adopted for sectional names. But he strongly commends the rule, that the etymology of names should always be stated by the proposer.

Justly does Mr. Agassiz condemn the practice of those who change the authority of a genus when they extend or narrow its bounds, or correct a faulty orthography. Thus he would write Lepidosteus, Lacep., although Lacepede wrote Lepisosteus; and especially would he write Perca, Linn. (Cuv.), not Perca, Cuv.

Hearty and just, also, is his censure of the custom of those French zoölogists who use vernacular appellations in scientific works, either to the exclusion of the systematic name, or in precedence of it.

Our author closes this part of his preface with some excellent reflections on the study of genera in the animal kingdom, and the need of a thorough reinvestigation of the grounds upon which natural families are constituted; remarks which we would gladly copy, if our limits allowed.

VON MOHL'S VEGETABLE CELL.

We desire in a special manner to commend this condensed treatise[1] not only to botanists, but to animal physiologists, to

[1] *Grundzüge der Anatomie und Physiologie der vegetabilischen Zelle.* Hugo Von Mohl: Braunschweig, 1851. English translation (*Principles of the Anatomy and Physiology of the Vegetable Cell*) by Arthur Henfrey, London, 1852. (American Journal of Science and Arts, 2 ser., xv. 451.)

medical students, and to all who would obtain a clear view of the present state of vegetable anatomy and physiology, — a knowledge of which, most interesting in itself, is almost indispensable to the correct understanding of the minute anatomy and physiology of animals. Professor Mohl is, without question, the first of vegetable anatomists, and his statements carry with them the highest authority on this class of subjects. We copy the short preface which he has contributed to the English translation, as it gives a clear view of the nature and scope of the work.

"Mr. Arthur Henfrey having informed me that he intends publishing an English translation of the present treatise, I take this opportunity of making known to the English reader the purpose I had in view in the preparation of the book. The following pages were not originally intended to appear as an independent work, or to give a summary of the wide subject of the Anatomy and Physiology of Plants, but appeared as an article in the 'Cyclopedia of Physiology,' published by Dr. Rudolph Wagner of Göttingen, drawn up to furnish students of Animal Physiology, and more particularly the medical profession, with a review of the anatomical and physiological conditions of vegetables (of the cell), in order to enable them to form a definite judgment upon the analogies which might be drawn between the structure and vital functions of animals and plants. This intention, together with the circumstance that I was compelled to crowd the whole exposition into the space of a few sheets, rendered it necessary to direct especial attention to the individual cell, as the fundamental organ of the vegetable organism. Since, however, the cell only presents itself in anatomical and physiological independence in the lowest plants, and since, in the more highly organized plants, both the structure and the physiological functions of the individual cells become subject to greater dependence upon the other parts of the plant, in proportion as the collective organization of the vegetable is more complex; moreover, since functions then present themselves, of which no trace can be found in the lower plants, it became requisite to take account of the plants of higher rank, and of the various organs

which these possess. The treatise, therefore, contains, if an imperfect, still in many respects a more extensive résumé of Vegetable Physiology than might have been conjectured from the title.

"Unhappily, the Physiology of Plants is a science which yet lies in its earliest infancy. Few of its dogmas can be regarded as settled beyond doubt; at every step we meet with imperfect observations, and consequently with the most contradictory views; thus, for example, opinions are still quite divided regarding the doctrines of the development of the cell, of the origin of the embryo, and of the existence of an impregnation in the higher Cryptogams. Both in these and in other cases, the small compass of the present treatise forbids a more extensive detail of the researches upon which the opposing views are founded; I hope, however, that I have succeeded in making clearly prominent the chief points upon which these contests turn, and thus in facilitating the formation of a judgment by the reader; and I have never neglected to indicate the literature from which further instruction is to be derived."

It may be well to notice the views of so excellent an observer upon sundry points which have been more or less matters of controversy. As to the milk-vessels, or vessels of the latex, Mohl inclines to adopt the view that considers them as intercellular passages which have acquired membranous linings (p. 2). He denies that the membrane of nascent cells is soluble in water, as Schleiden states (p. 9). He briefly states the ground on which, in his controversy with Harting and Mulder, he successfully maintains that the primary cell-membrane is thickened by successive concentric layers of cellulose deposited on its inner face. The combination of spiral markings and pits on the wood-cells of Taxus and Torreya, as also in the Linden, is explained by considering the former to belong to a second layer or deposition within that to which the pits belong. This tertiary membrane or deposit forms the spiral fibre or band in the cells of the seed-coat of Collomia, the hairs of the achenium of Senecio, etc. (p. 18). The whole subject of spiral and other markings, rings, dots,

slits, reticulations, etc., on the walls of cells is expounded in a masterly and convincing way. Mohl maintains (p. 28), as he has done in the "Botanische Zeitung," that cellulose forms the basis of all vegetable membranes in the higher plants, and that what Mulder regarded as peculiar compounds are combinations of cellulose with foreign infiltrated deposits, which interfere with the chemical reactions of cellulose, but which may be removed by previous maceration in caustic potash and nitric acid. He now maintains, in opposition to his early views (still defended by Schleiden), that the intercellular substance is a product or secretion of the cell, and not a universally distributed mass in which the cells are imbedded (p. 33). He shows that the thickened "cuticle" of Unger, Mulder, Harting, etc., consists of secondary layers of cell-membrane, deposited from the inside, and infiltrated with some substance that is colored brown by iodine; with the exception of an extremely thin external pellicle, the real cuticle of Brongniart, which is probably a secretion from the surface of the cell, like that which forms the outer coat of pollen-grains (p. 35). He insists that the layer of protoplasm, his primordial utricle, lining the cell is a soft and delicate membrane, and not a mere layer of mucilage (p. 37); the mode in which this is constructed and a partition formed by an inward growth at the fold, in the multiplication of cells by division, is very clearly explained (p. 50–53). Free cell-formation is said to occur, in Phænogamous plants, only in the embryo-sac, in which both the embryonal vesicle, or rudiment of the embryo, and the cells of the albumen, originate in this manner; in the Cryptogamia, only in the formation of spores in certain cases, as in the Lichens (p. 58); contrary to the view of Schleiden, who long maintained this to be the universal, and lately a general, mode of cell-production. Schleiden's original account of the process of the formation of a free cell from a nucleus is directly controverted in all essential points; the nucleus, according to Mohl, being always central, and at no time connected with the cell-membrane, but always enclosed in the primordial utricle. A nucleus, or mass of nitrogenous substance, first has the primordial utricle, or nitrogenous mem-

brane, developed over its surface; and then the cell-membrane (of cellulose) is deposited upon this. In Cryptogamous plants, such masses, of larger or smaller size, may become coated with a cell-membrane without any proper nucleus appearing (pp. 57–60). Thus much for what relates to the anatomical condition of the cell.

Under the second head, the Physiological Condition of the Cell, our author treats, first, of the cell as an organ of nutrition, next, as an organ of propagation, and finally, as an organ of motion. He pronounces against the Knightian doctrine, that plants ultimately degenerate and perish when propagated for generation after generation by division (from the bud) (p. 64). That the crude sap, though absorbed by the parenchymatous tissue of the root, ascends through the woody tissue, and that the assimilated sap returns through the bark, and thence more or less into the wood by means of the medullary rays, is very neatly shown. "A few simple experiments leave no doubt about this. . . . If the bark of a plant, best of a tree, is cut through in a ring down to the wood, there is no interruption in the flow of sap to the parts situated above the wound; but if the wood is cut through, the greatest care being taken to avoid injuring the bark, that portion of the plant above the wound dries up at once. From the wood of the stem and branches the sap flows onward into the leaves, as is proved by the powerful expiration of watery vapor from them. Before the sap has reached the leaves it is incapable of being applied to nutrition; consequently the vegetation of a plant comes to a stand-still when it is deprived of its leaves. The sap ascending from the root to the leaves is thence termed the crude sap. It undergoes a chemical change in the leaves, rendering it fit to be applied to the nutrition of the plant. To this end the sap flows backwards from the leaves through the bark to the lower parts, as the following circumstances testify. If the bark is cut off the stem in a ring, the growth of the portion below the wound stands as it were still; the stem becomes no thicker; in the potato plant no tubers are produced, etc.; but, on the other hand, the growth above the wound is increased beyond the usual measure, thicker

layers of wood are deposited, more fruit is perfected, this ripens sooner, etc. The deposition of starch which occurs in the cells of the medullary rays in autumn, goes to prove that the portion of assimilated sap which is not used for nutrition on the way to the root, runs back to the wood through these horizontal medullary rays; and thus the sap describes a kind of circle, not, indeed, in determinate vessels, but in a definite path leading through the different parts of the plant. It is difficult to see how in recent times the results of these experiments could have been so questioned, and the existence of the descending current in the bark denied. Certainly it is no improvement on the theory cast aside when the increased growth above the annular wound is explained by the artificial interruption of the upward current of crude sap, in consequence of which the sap contained in the upper part of the plant must soon become greatly concentrated and potential for development (Schleiden, 'Grundzüge,' 2d ed., ii. 513). When we can succeed in fattening an animal by depriving it of a portion of its accustomed food, this explanation may be received as satisfactory" (p. 70, 71). After some excellent points of criticism, Mohl concludes that the discovery of endosmose has not fully solved the problem of the movement of the sap in plants, although in all probability it does play an important and perhaps the principal part in its absorption and conveyance (p. 77). To the question, whether plants live on inorganic food alone or take in also organic matters, Mohl gives a sensible answer, rejecting the extreme view of Liebig, while still fully recognizing the great office and result of vegetation (p. 78). According to Mohl, however, it is proved that plants do not absorb the carbonic acid dissolved in water with the latter by means of their roots (p. 81); but this seems hardly reconcilable with several facts stated on the next page, from which it is justly concluded that carbonic acid is carried up with the ascending sap into the leaves. From the fact that plants perish so soon in air deprived of all oxygen gas, that sensitive leaves lose their irritability under such circumstances, etc., Mohl concludes, apparently with good reason, that the absorption of oxygen and the exhalation of carbonic

acid in plants is a true physiological function, intimately connected with the life of the plant; and that this (rather than the opposite and predominant nutrient process, through which carbonic acid is decomposed and oxygen evolved) should be considered as the respiration of plants, if we use the term at all (p. 86). "The roots of plants, and not the leaves, take up the substances which furnish plants with nitrogen, while, on the contrary, the leaves play the essentially active part in the absorption of carbonic acid" (p. 88). The analogy of the milky juice of plants to the blood of animals, as propounded by Schultz, is thoroughly refuted by our author, and the flowing movement in the milk-vessels described by Schultz is positively denied to take place in an uninjured plant, except as produced by mechanical causes. That the milky juice is not a nutrient material, still less the nutrient juice, is also manifest (p. 96).

The cell as an organ of propagation is treated, first, as respects the multiplication of plants by division; second, by spores; and third, by seeds. The conjugation of certain Confervoid *Algæ*, such as Zygnema, is said to bear no analogy to sexual reproduction (p. 113); a conclusion which may be questioned. A good summary is given of the facts known respecting the free and spontaneous movements of the spores of the lower *Algæ* (p. 115); and also of the recent discoveries respecting the bisexual reproduction of the higher *Cryptogamia*.

The reprint of Henfrey's Report, in the January and March numbers of this Journal, has placed our readers au courant with the present state of knowledge on this interesting subject. It should be noticed that Mohl denies the existence of antheridia in lower *Cryptogamia*, or *Thallophytes;* but maintains that the small bodies, moving by two cilia, discovered by Decaisne and Thuret in the *Fucaceæ*, are more properly a second kind of spores, analogous to the small spores of the *Florideæ*, than of the nature of the seminal filaments of Ferns, Mosses, etc. (p. 117). The latter researches of Itzigsohn, Thuret, Tulasne, etc., however, lead rather to the conclusion that the lower *Cryptogamia* (except the very low-

est) are likewise bisexual.[1] Under the head of Reproduction by Seeds, Mohl gives an interesting and critical account of the development and structure of the pollen and the ovule, and of the origin of the embryo. The latter arises by cell-multiplication of the germinal vesicle, a cell produced by free cell-formation in the embryo-sac usually before the pollen-tube has reached the latter. The germinal vesicle and the extremity of the pollen-tube are separated by the thickness of the parieties of the embryo-sac. The penetration of the pollen-tube into the latter or into an introverted portion of it, and the formation of the embryo from the apex of the pollen-tube itself, as taught by Schleiden, are wholly repudiated; and indeed the Schleidenian doctrine may now be considered as thoroughly demolished, by the direct observations of Amici, Mohl, Muller, Hofmeister, Unger, Henfrey, and Tulasne. The Cell as an Organ of Motion is considered as respects movements of individual cells through the agency of vibratile cilia, as respects the directions and curvature assumed by organs, and as respects movements by irritation of stimuli, etc., giving an excellent summary of our knowledge on these points, with much admirable criticism; which want of space prevents us from noticing in detail.

[1] Itzigsohn in *Botanische Zeitung*, May, 1850. — Here it is announced that the black dots on the surface of the frond of *Riccia ciliaris* contain antheridia, that is, cells from which escape animalcular-like corpuscles that move freely in water, and are similar to those of Mosses and Liverworts. Later, after stating that others had failed to detect those movements, he announces that they had been observed by Rabenhorst, after many ineffectual trials. He also (December, 1850, February, 1851) states that these "spermatozoids" do not manifest viral movements until after the maceration of the Lichen in water for several days.

Tulasne (*L. R.*) *Memoire pour servir a l'Histoire Organographique et Physiologique des Lichenes*; in *Ann. Sciences Naturelles*, 3 ser., xviii., No. 1, 2, 3, 4 (1852), with 16 plates. — A most admirable and complete memoir, elucidating in an unequalled manner the whole structure and morphology of the Lichens. It is to be hoped that the author will publish it in a separate form, as it introduces a new era in Lichenography. On the subject of the so-called antheridia (which alone we can here notice), M. Tulasne has recognized the universal occurrence of these bodies in Lichens, has ascertained their structure and development; but he has never detected any free movement of the corpuscles, except the general

ON THE GROWTH OF PLANTS IN GLAZED CASES.

The first edition of this little treatise,[1] published in 1842, is doubtless well known to many of our readers; and some may remember Mr. Ward's original account of his interesting discovery of a method of growing every sort of plant in the dun atmosphere of the smokiest part of London, pub-

molecular or Brownian motion common to all minute particles. He therefore gives to the so-called antheridia the name of *spermogonia*, and to the contained corpuscles the name of *spermatia*. He unhesitatingly recognizes in them an apparatus of reproduction, doubtless analogous, at least in function, to those of the Florideous *Algæ*, in which the corpuscles are equally motionless, and of certain *Fungi*, and therefore probably representing male organs. Tulasne likewise calls attention to the fact that these dark tubercles or dots were particularly noticed by Dillenius, more than a century ago, in *Borrera ciliaris*; and that Hedwig, in 1784, expressed the opinion that they constituted the male apparatus of Lichens.

Decaisne & Thuret, *Recherches sur les Antherides et les Spores de quelques Fucus*; in *Ann. Sci. Nat.*, 3 ser., iii. p. 5. — Here the corpuscles known to the earlier Algologists, and considered by Agardh and Montagne as a second kind of spores (a view which Mohl adopts), are announced to be the spermatozoids of the antheridia of *Fucaceæ*; their active movements are described, and the discovery of the two cilia is announced by whose vibration the movement is effected.

Thuret, *Recherches sur les zoöspores des Algées et les Antherides des Cryptogames*. — These researches were communicated to the Academy of Sciences, Paris, and were rewarded by the great prize for natural sciences, in 1847. A copious abstract has been published in the *Ann. Sci. Nat.*, 3 ser., xiv. and xvi. (1850, 1851), with 30 plates. As to antheridia, bodies like the free moving corpuscles of the *Fucaceæ* are shown likewise to occur in all the *Florideæ*, except that they do not exhibit spontaneous movements; nevertheless, M. Thuret does not hesitate to attribute to both the same functions as those which the seminal filaments of the higher *Cryptogamia* fulfil. The Antheridia of Chara (in which Thuret first discovered the cilia by whose vibration the coiled filaments are moved), of the Liverworts, Mosses, and Ferns, are also admirably illustrated; but nothing of consequence is added to the facts mentioned in Henfrey's Report.

Léveille, in *Ann. Sci. Nat.*, 3 ser., xv. p. 119, has indicated the probable existence of the analogues of antheridia in *Fungi*.

[1] *On the Growth of Plants in closely glazed Cases*. By N. B. Ward. London, 1852. (American Journal of Science and Arts, 2 ser., xvi. 132.)

lished in the "Companion to the Botanical Magazine" in 1836. This new edition if reduced in size is increased in interest, and is embellished with tasteful illustrations on wood, several of them exhibiting approved forms of those glazed cases with which the name of our author is inseparably connected. The first chapter, on the natural conditions of plants, their relations to heat, light, and moisture, and the necessity of attending to the particular conditions, or combinations of circumstances, under which each species flourishes, is illustrated by ingenious and often novel observations. The second chapter treats of the causes which interfere with the natural conditions of plants in large towns, and gives some idea of the obstacles which prevent the cultivation of even ordinary plants in the open air of London, and to some extent in other large British towns. The third, on the imitation of the natural conditions of plants in closely glazed cases, tells us how a simple incident (the accidental growth of a seedling fern and a grass in a glass bottle, in which the chrysalis of a Sphinx had been buried in some moist mould), carefully and wisely reflected on, taught Mr. Ward how to overcome these obstacles, and thus to surround himself with his favorite plants, in beautiful vegetation, while living in one of the murkiest parts of London, and even to grow with complete success such ferns as the *Trichomanes radicans*, which is utterly uncultivable in any other way. A fourth chapter treats of the conveyance of living plants on shipboard; which brings to view one of the most important practical applications of Mr. Ward's discovery.

Sir William Hooker states that "the Wardian Cases have been the means, in the last fifteen years, of introducing more new and valuable plants to our gardens than were imported during the preceding century; and in the character of Domestic Greenhouses, *i. e.*, as a means of cultivating plants with success in our parlors, our halls, and our drawing-rooms, they have constituted a new era in horticulture." Formerly only one plant in a thousand survived the voyage from China to England. Recently, availing himself of our author's discovery, Mr. Fortune planted 250 species of plants in these cases in

China, and landed 215 of them in England alive and healthful. The same person lately conveyed in this way 20,000 growing tea-plants, in safety and high health, from Shanghai to the Himalayas. In fact, this mode of conveyance is now universally adopted, and has proved so successful, whenever properly managed, that it is no exaggeration to say that, probably, "there is not a single portion of the civilized world which has not been more or less benefited by the invention." An indispensable requisite to success in the transmission of living plants by this method is, that the glazed cases should be freely exposed to the light. Where this cannot be done, we must be content with the former method, of conveying plants in a passive condition, closely packed in peat-moss, — a plan, however, which is only partially successful in protracted voyages. Two additional and highly interesting chapters treat of the application of the "closed" plan in improving the condition of the poor; and on its probable future applications in comparative researches in vegetable physiology, and even in the treatment of diseases. To these, as to the other topics of the work, no justice can be rendered to our author's suggestions except by lengthened quotations, which the nature of this notice does not admit of. It must suffice to direct attention to this fascinating little volume. Those who read it and who have a true fondness for growing plants will scarcely be contented without a Ward case, of more or less pretension; which they will find an unfailing source of interest, especially during the long and total suspension of vegetation in our protracted winters. With proper management, and with the requisite amount of light, any plant may thus be cultivated. But we particularly recommend Ferns and Lycopodia, of the most delicate kinds, as requiring least care, and as making the prettiest appearance at all seasons. Most of these require little light; although our clear skies afford us this in abundance. So little bituminous coal is consumed, even in our largest cities, that the "fuliginous matter" with which all British towns are begrimed and rendered noxious to vegetation, here interposes no obstacle to rearing plants. Quite unlike England, the principal obstacle to the growth of

delicate plants in our houses in winter, and in our grounds in summer, comes from the dryness of the air. For this, the Ward case affords a perfect remedy; as nothing is easier than to furnish a saturated atmosphere for those plants that require it, or to supply and retain the degree of moisture which suits any particular species.

HOOKER AND THOMSON'S INDIAN FLORA.

One half of this volume[1] is occupied by the Introductory Essay, in which a series of important general topics, akin to those discussed in the introduction to Dr. Hooker's New Zealand Flora, are treated with equal boldness and judgment, and with the same freshness and originality of illustration. These are arranged under six general heads, namely: 1. The object, scope, and design of the "Flora Indica." 2. General considerations connected with the study of systematic botany. 3. The variation and origin of species, the effects of hybridization, and the geographical distribution of species. 4. Summary of the labors of Indian botanists. 5. Sketch of the meteorology of India. 6. Sketch of the physical features and vegetation of the provinces of India. To which two maps are added: one of monthly isotherms, from Dove; the other a large and original map illustrating the physical geography of India and its botanical provinces. A complete alphabetical index to this part of the work is appended, as well as a detailed table of contents.

To enumerate even the principal points which are discussed would require a space which we are unable now to devote to this subject. Some of them we may hope to consider hereafter in other connections. Among the conclusions or suggestions that strike us as most true and timely are: The great

[1] *Flora Indica:* being a systematic account of the Plants of British India. Vol. I. By J. D. Hooker and Thomas Thomson. (*Ranunculaceæ* to *Fumariaceæ*.) With an Introductory Essay. London, 1855. (American Journal of Science and Arts, 2 ser., xxi. 134.)

want on the part of many naturalists of clear and logical views in respect to classification and system; — "the prevailing tendency on the part of students of all branches of natural history to exaggerate the number of species, and to separate accidental forms by trifling characters;" — the unphilosophical and detrimental character of "the modern system of elevating every minor group, however trifling the peculiarities by which it is distinguished, to the rank of a genus;" in other words, of considering every group of species to form a genus, — evincing a want of appreciation of the true value and nature of classification; — the fact that in the vegetable kingdom we do not discover that close and obvious connection between structure and function which is almost universally apparent in the animal kingdom, giving to physiology a greater influence over classification in zoölogy than in botany, and offering a guide to determining the relative value of structural characters in the one kingdom which is comparatively little available in the other, but yet may not safely be neglected.

Our authors assume, as most accordant with known facts on the whole, that species are distinct creations, and not arbitrary assumptions of the systematists; and they adopt that idea of species which alone appears to give them a perfectly clear and intelligible, distinct, objective existence in nature, namely, that they consist of individuals which have originated each from a common stock. They assume not only their original, but their continued definiteness in nature; but their variations, surprising as they often are, are restricted within certain limits, to which we may add that these limits are not *a priori* determinable. Among the causes inducing variation, or tending to produce a blended series of individual forms, if such did not exist from the beginning, they first consider the effects of hybridization; and remark that recent experiments have led to the following results:

"1. It is a much more difficult operation to produce hybrids, even under every advantage, than is usually supposed. The number of species capable of being impregnated, even by skillful management, is very few; and in nature the stigma

exerts a specific action, which not only favors and quickens the operation of the pollen of its own species, but resists and retards the action of that of another; so that the artist has not only to forestall the natural operation, but to experience opposition to his conducting the artificial one.

"2. Even when the impregnation is once effected, very few seeds are produced; still fewer of these ripen; and fewest of all become healthy plants, capable of maintaining an independent existence.

"3. The offspring of a hybrid has never yet been known to possess a character foreign to those of its parents; but it blends those of each; — whence hybridization must be regarded as a means of obliterating, not creating, species.

"4. The offspring of hybrids are almost invariably absolutely barren, nor do we know an authenticated instance of the second generation maturing its seeds.

"5. In the animal kingdom hybrids are still rarer in an artificial state, are all but unknown in a natural one, and are almost invariably barren."

Perhaps some of these dicta are too unqualifiedly stated; indeed they are manifestly intended to affirm the results to which the whole evidence points, rather than those which can be said to be thoroughly verified.

The third proposition, however, is absolutely true; and, in connection with it, well do our authors say, that all we could legitimately conclude is, that were hybrids of the general occurrence which some botanists imagine, they would long ago have obliterated all traces of species as definite creations; whereas, exceptional in art, and not proven if not almost impossible in nature, they cannot be assumed to have produced any appreciable result. There is one point, however, which our authors do not take into consideration, but which should not be overlooked, namely, what is generally admitted as a fact, that a hybrid may readily be fertilized by the pollen of either of its parents; and that if hybrid plants are occasionally produced in nature, they would ordinarily stand a very good chance of being fertilized in this way. In such cases they are said to revert to the type of the species of the im-

pregnating parent; but would they return exactly to that type, inheriting as they do a portion of the blood of a cognate species? And where — as not unfrequently occurs — two or more generally well-marked forms in nature are connected by certain occasional individuals of intermediate character, is it not very supposable that two species may have partially blended in this way? At any rate there is a *vera causa*, or what passes as such, which requires to be taken into account, as has not yet been done, so far as we know. This doubtless has operated in the case of cultivated plants, and contributed, along with other causes, to the inextricable blending of certain species. But we are not disposed to exaggerate its influence in nature; since we suppose, with Dr. Hooker, that wild plants rarely hybridize. Yet the possibility and even the probability of the occurrence must not be overlooked in a thorough discussion of the general question of the limitation and permanence of species.

However it may be as a blending influence, hybridization is far from being a considerable, or the most potent cause of the variation of species, since "the offspring of a hybrid has never yet been known to possess a character foreign to those of its parents." And we equally agree with our authors that the known facts of the case "especially warn us not to consider the influence of climate as paramount in determining the distribution of species or the prevalence of forms," or even as the most efficient cause of variation. What the cause is that the legitimate offspring does occasionally possess a character foreign to those of its parents we are wholly unable to say; but the fact is undoubted, and perhaps of more frequent occurrence than is generally supposed. It is usual to say that the abnormal forms originate only in cultivated or domesticated individuals: it were perhaps better to say that they were perpetuated, or are favorably situated for continuation and full development, only under these circumstances, on account of the greater segregation; for of the very various species of plants which are cultivated none are free from the tendency to "sport" into races, whether of ancient or of recent introduction. Why their existence is so transitory in

nature, and so capable of being continued and further developed in domestication, it is not difficult to imagine. Our authors perhaps, in common with naturalists generally, do not sufficiently recognize the natural tendency to perpetuation of individual characteristics.

As regards ordinary variation between different individuals of the same species, the want of due consideration of what every good observer knows to be true, has indeed "mainly contributed to such an undue multiplication of species in the vegetable kingdom as botanists unfamiliar with large herbaria and exotic plants are slow to believe, and to the exaggerated estimates of the supposed known extent of the vegetable creation that gain common credence." Our authors believe that the number is swelled one third beyond its due extent by the introduction of bad species founded on habit, and on accidental variations produced by soil, exposure, etc.; and, we would add, on the imperfection of the materials from which the greater part of the species that crowd our books were originally described, most of them without due elaboration of already published species, and drawing after them an ever lengthening train of nominal species, founded on mere guesses at supposed differences from vague and incomplete descriptions, without any collection of specimens.

We have already exceeded our limits, while yet at the beginning of Drs. Hooker and Thomson's interesting and suggestive volume. We regret that we must omit all notice of their remarks upon habit as indicating specific difference, which, contrary to the general view, they regard as "most deceptive," and must pass over their important section upon geographical distribution in general, and its dependence upon specific centres. We only add, that whoever would attain a clear comprehension of the configuration, the diverse climates, and the general botanical geography of those extensive and widely varied regions which are comprised, and in most minds confused, under the general name of India, has only to study the admirable sections on the meteorology of India, and on the physical features and vegetation of its provinces, which occupy a large portion of the Introductory

Essay. The present commencement of the Flora itself, although comprising only fifteen natural orders, is also an inviting subject for extended comment and almost unqualified commendation.

DE CANDOLLE'S GÉOGRAPHIE BOTANIQUE.

The "Géographie Botanique" [1] of De Candolle is not only one of the most important works of our day, but one which addresses, and will greatly interest, a much broader circle of scientific readers than any other modern production of a botanical author. It is, and probably long will be, the standard treatise upon a wide class of questions, highly and almost equally interesting to the botanist, the zoölogist, the geologist, the ethnologist, and the student of general terrestrial physics. To its production the author has devoted no small portion of the best years of his life; and it bears throughout the marks of untiring labor, directed by a remarkably sound, conscientious, and thoroughly systematic mind. Along with the admirable methodical spirit which is his by rightful inheritance, the younger De Candolle brings to these investigations a particular aptitude for numerical and exact forms, an intimate acquaintance with general physical science, and considerable ethnological and philological learning; which last is turned to good account in his chapters on the history of cultivated and naturalized plants. The result in the work before us — even if there were no other claims to the distinction — may fairly be said to go far toward inscribing the name of De Candolle anew in that select list of philosophical naturalists in which his father holds so eminent a position.

To give some idea of the topics considered in these volumes, and of the order of investigation (which proceeds in an admirable course, from the more simple, general, and better

[1] *Géographie Botanique raisonnée, ou Exposition des Faites principaux et des Lois concernant la Distribution Géographique des Plantes de l'Europe Actuelle.* Alphonse De Candolle. Paris and Geneva, 1855. (American Journal of Science and Arts, 2 ser., xxii. 429.)

known facts and principles towards the more complex, hypothetical and obscure), we will copy the titles of the chapters, twenty-seven in number; which are arranged in four books, and subdivided into articles, and these again into sections, to such an extent as to fill eight closely printed pages with the bare enumeration. Indeed, this repeated subdivision gives a rigid and rather tedious aspect to some parts of the work, and involves occasional repetitions; but it would not be easy to collocate well and clearly so vast an amount of material in any better way.

The First Book is occupied with some preliminary considerations upon the way in which temperature, light, and moisture act upon plants. Its three chapters treat of the relations of plants to surrounding physical conditions, and especially to heat and light; and contain the author's happy distinction between the temperatures actually operative in vegetation, and those which (being below the freezing point, etc.) are altogether null for vegetation, and ought to be eliminated from the tables of mean temperature, when these are viewed in relation to the northern and southern geographical range of species.

The Second Book is devoted to Geographical Botany, or the study of species, genera, and families, from a geographical point of view. Chapter iv. relates to the limitation of species upon plains and upon mountains, and the probable causes of their actual limits, applied both to spontaneous and cultivated plants; and there is good endeavor to show that the northern limit of species is fixed rather by the sum of heat available for vegetation during the growing season, than by the mean temperature of the year. Chapter v. treats of the shape of the area occupied by a species, a very curious point; and it seems that the area of species inclines to be circular or elliptical. Chapter vi. treats of the associations or disjunction of the individuals of a species in its area. Chapter vii. treats of the area of species as to extent of surface, considered as to the families they belong to, as to stations, as to size and duration of the plant, and as to the character of the fruit and seed, whether affording facilities to dispersion or not. Chapter viii.

considers the changes which may have taken place in the habitation of species, and discusses with great fullness the whole subject of naturalization, the obstacles in the way, the causes and means of transport, and the interchanges which have been effected between the New and the Old Worlds. Chapter ix. is a very long and interesting one, on the geographical origin of the principal cultivated plants, not only those intentionally, but also those unintentionally cultivated by man, — a chapter full of valuable matter, carefully collected and well discussed.[1] Chapter x. treats of disjoined species, — those occupying two or more widely separated areas, and not in intermediate stations. Chapter xi. discourses of the early condition and probable origin of the existing species; and brings out the various facts which go far to prove the geological antiquity of the greater part of existing species; and that their creation was probably successive. Chapter xii. treats of genera and their geographical distribution, and maintains the view (in which we by no means coincide) that genera are truly naturally-limited groups, even more so than species. Chapter xiii. is devoted to the distribution of the species of a genus within its area. Chapter xiv. treats of the extent of surface occupied by genera. Chapter xv. discourses of the origin and duration of genera. Chapters xvi.–xix. treat of families, as to their area, geographical limits, the distribution of species within the area of the family, etc.

The Third Book is devoted to Geographical Botany, or the characters of different countries considered as to their vegetation. Chapter xx., of the characters of the vegetation of a country; considered, in Chapter xxi., as to the relative numbers in the great classes respectively. Chapter xxii., comparison of different countries in respect to those natural orders

[1] It is singular that M. De Candolle should be so slow to abandon the idea that the aborigines of Carolina, or any other part of North America, cultivated or knew anything of the Potato, which, if Raleigh obtained them in Carolina, were certainly imported thither. But, though our aborigines had no Potatoes, they had Pumpkins or Squashes and Beans, which all writers upon the history of cultivated plants have overlooked except the late Dr. Harris.

which abound most in species; and Chapter xxiii., as regards their most characteristic natural families. Chapter xxiv., on the variety of vegetable forms in different countries and in the world at large, *i. e.*, the probable number of species, the proportion of genera to species, and of orders of genera and species. Chapter xxv., the division of the earth's surface into natural botanical regions. Chapter xxvi., sketch of the vegetation of the different countries in respect to the probable origin of their existing species, etc.

The Fourth Book, of a single brief chapter, consists merely of a summary of the author's general conclusions. We give these entire, for convenience availing ourselves of a translation in Hooker's "Journal of Botany."

"The plants now inhabiting the globe have survived many changes, geological, geographical, and, latterly, historical. The history of their distribution is hence intimately connected with that of the whole vegetable kingdom.

"To explain existing facts, it is fortunately unnecessary to adopt any conclusion upon the most obscure hypotheses of cosmogony and palæontology, or on the mode of creation of species, the number originally created, and their primitive distribution. Botanical geography can indicate certain probabilities, certain theories, but the principal facts in distribution depend upon more recent and less obscure causes. It suffices to understand and to allow certain facts and theories, which appear probable, namely, that groups of organized beings under different hereditary forms (classes, orders, genera, species, and races) have appeared in different places and at different times; the more simple perhaps at first, the more complicated afterwards; that each of these groups has had a primitive centre of creation of greater or less extent; that they have, during the period of their existence, been able to become more rare or common, to spread more or less widely, according to the nature of the plants composing them, the means of propagation and diffusion they are possessed of, the absence or presence of animals noxious to them, the form and extent of the area they inhabit, the nature of the successive climates of each country, and the means of transport that the

relative positions of land and sea may afford; that many of these groups have become extinct, whilst others have increased, at least so far as can be judged by comparing existing epochs with preceding ones; and lastly, that the latest geological epoch, the Quaternary (that which preceded the existence of man in Europe, and which followed the last elevation of the Alps), has lasted many thousand years, during which important geographical and physical changes have affected Europe and some neighboring countries, whilst other regions of the globe have suffered no change, or have been exposed to a different series of changes.

"Thus the principal facts of geology and palæontology, reduced to the most general and incontestable, suffice to explain the facts of Botanical geography, or at least to indicate the nature of the explanation, which it requires the progress of many sciences to complete.

"The most numerous, the most important, and often the most anomalous facts in the existing distribution of plants, are explained by the operation of causes anterior to those now in operation, or by the joint operation of these and of still more ancient causes, sometimes of such as are primitive (connected with the earliest condition of the planet). The geographical and physical operations of our own epoch play but a secondary part. I have shown that in starting from an original fact, which it is impossible to understand, of the creation of a certain form, in a certain country, and at a certain time, we ought to be able, and sometimes are able to explain the following facts, chiefly by causes that operated previous to our own epoch: 1, the very unequal areas occupied by natural orders, genera, and species; 2, the disconnection of the areas that some of the species inhabit; 3, the distribution of the species of a genus or family in the area occupied by the genus or family; 4, the differences between the vegetations of countries that have analogous climates and that are not far apart, and the resemblance between the vegetation of the countries that are apart, but between which an interchange of plants is now impossible.

"The only phenomena explainable by existing circum-

stances, are: 1, the limitation of species, and consequently of genera and families, in every country where they now appear; 2, the distribution of the individuals of a species in the country it inhabits; 3, the geographical origin and extension of cultivated species; 4, the naturalization of species and the opposite phenomenon of their increasing rarity; 5, the disappearance of species contemporaneous with man.

"In all this we observe proofs of the greater influence of primitive causes, and of those anterior to our epoch; but the growing activity of man is daily effacing these, and it is no small advantage of our progressing civilization that it enables us to collect a multitude of facts of which our successors will have no visible and tangible proof."

An Appendix, indicating the researches now needed for the advancement of Geographical-botanical science, under several heads, addressed respectively to physicists and meteorologists, to geographers, to geologists, to vegetable physiologists, descriptive and traveling botanists, and to philologists, brings these most interesting volumes to a conclusion.

Our present object is to call the attention of American naturalists and natural philosophers to this work, not to criticise it. That would require much consideration and a wider range of knowledge than we can pretend to. There are, however, several topics upon which we are inclined to venture a few remarks, as fitting opportunities occur.

HENFREY'S BOTANY.[1]

This is a well-planned, compact, and comprehensive work, in which we may say, that the author has fairly accomplished his purpose, namely: — "to produce a good working text-book for the student, from which may be obtained a groundwork of knowledge in all branches of the science,

[1] *An Elementary Course of Botany; Structural, Physiological, and Systematic; with a brief Outline of the Geographical and Geological Distribution of Plants.* By Arthur Henfrey. London, 1857. (American Journal of Science and Arts, 2 ser., xxiv. 434.)

without the attention being diverted from the more striking features of the subject by details comparatively unimportant."

The work is divided into four parts. I. Morphology or Comparative Anatomy; treating, in successive chapters, 1st, of General Morphology; 2d, of the Phanerogamia, or the parts of Flowering plants and their modifications, and the laws which regulate them; 3d, Morphology of the Cryptogamia. Part II. Systematic Botany; treating, 1st, of the principles of Classification; 2d, of systems of Classification, and 3d, Systematic Descriptions of the natural orders, followed by an artificial analysis. Part III. Physiology; comprising, 1st, the physiological Anatomy of plants; 2d, general considerations on the Physiology of plants; 3d, Physiology of Vegetation; 4th, the reproduction of plants; 5th, Miscellaneous phenomena, under which are ranked the evolution of heat in plants, luminosity, and movements of plants. Part IV. Geographical and Geological Botany, very summarily disposed of in about forty pages.

It seems strange at first to interpose systematic botany between the morphological and the physiological; but if the anatomy and physiology of plants are to be completely disjoined from the study of the organs of the plant as a whole, the present arrangement is perhaps as good as any. It is adopted, as the preface shows, for the convenience of instructing medical students, who compose the principal part of classes in Great Britain as well as on the Continent; — for whom "one short course of lectures is devoted to this science, and three months is commonly all the time allotted to the teacher for laying the foundations and building the superstructure of a knowledge of botany in the minds of his pupils, very few of whom come prepared even with the most rudimentary acquaintance with the science." But the author remarks that "if the previous education of medical students prepared them, as it should, with an elementary knowledge of the natural sciences, we should make physiology the most conspicuous feature of a course of botany in a medical school."

While in England botany is scarcely an academical study,

here it pertains to collegiate and academical instruction where it is taught at all. In Europe not even an apothecary can be licensed without passing an examination in botany; in the whole United States, we believe, it forms no part, at least no regular part, of the medical curriculum; no medical school has a botanical chair; and no knowledge whatever of the science of the vegetable kingdom, which supplies the greater part of the *materia medica*, is required for the degree of Doctor in Medicine!

Professor Henfrey is chiefly known, and most highly esteemed, as a vegetable anatomist. Upon this subject he may speak with an authority which as a systematist, or even as a morphologist, he would not pretend to. We shall offer no apology, therefore, for making an occasional criticism, and for pointing out several errors in matters of detail. These are not intended to disparage the work, for if we had not formed a high opinion of it on the whole, we should not take this trouble.

As respects the first point noticed, our author, if wrong, is not alone. Still, we hardly expected him to teach that the radicle of the embryo is the true root; and we cannot let pass unchallenged his reiterated statement that in Monocotyledons, the radicle, or its inferior extremity, is never developed into a root in germination, but is abortive (pp. 14, 16, 18, 391, 537). Any one who will examine the germination of the seed of an Iris, an Onion, or even of a grain of Indian Corn, cannot fail to perceive that a primary root is developed, and that this is a direct prolongation of the extremity of the radicle. This, indeed, does not continue as a tap-root; neither does it in a great many Dicotyledons. In Squashes, Pumpkins, etc., there is no one primary root, but a cluster of rootlets from the first, all springing from the base of the stout radicle. In fact, this distinction between Monocotyledons and Dicotyledons is null. A character of certain monocotyledonous embryos, neither strictly peculiar to the class, nor by any means universal in it, should not be assumed as distinctive. As to the morphology of the radicle itself, we suppose that the germination of any of the larger *Cucurbitaceæ*, or of a bean,

would suffice to convince any observer that the radicle is simply the first internode of the stem, giving birth to the primary root from its inferior extremity, usually, — and indeed, from the exceptional cases where it does not we should draw additional proof of its cauline nature. In fact, we know of no character in which a root differs from an internode of a stem in which it does not also differ from the radicle, excepting its tendency to direct its inferior extremity downwards. Again, should the statement, that "the radicle of a monocotyledonous embryo is never developed" be held to mean that the radicle never lengthens, we remark, no more does it in the Pea and some other hypogæous Dicotyledons; and we are not quite sure that the statement is absolutely true of all Monocotyledons.

Root-hairs or *fibrillæ* are mentioned (p. 19) as "often" occurring on young roots. Do they not always occur? Surely it cannot be true that: "the branches of the axial root are originally growths from the apex of the root thrown off to the side," (p. 538). By some slip of the pen, *Myrica Gale* is adduced as an instance of whorled leaves (p. 45).

On p. 49 the expression "over the petiole," instead of above or within it, would lead to a misconception.

Something more might be said about the tendrils of *Cucurbitaceæ* (which, besides, are not always single); but are the students of King's College really taught that, "tendrils of the vine are metamorphosed flowering branches arising in the axils of the leaves"? (p. 62.)

"In all seeds except in those of the few orders which present an incomplete or acotyledonous embryo, we do not find the young plant possessed . . . of a plumule" (p. 66). Even some much developed embryos, such as those of Maple and Morning Glory, do not show the plumule until after the full development of the cotyledons. It may be said, indeed, that the plumule is *in posse* when not *in esse*, but so it is no less in the cases excepted from the statement.

Very singular is the statement (on p. 68) that in England "the terminal bud of the Lilac is generally killed by the frost in the winter;" since in our much colder winter it is as

completely hardy as the other buds whenever it happens to be formed, and, like them, is well developed before summer is over. As a general rule here, and we presume in England also, no terminal winter bud appears during the growing season, and so there is none to be killed by the frost of the following winter.

The deeply alveolate receptacle of the Cotton-Thistle is figured (on p. 78) as an illustration of a *paleaceous* receptacle.

Truly terminal flowers are said to be rare (p. 86): we do not quite understand this.

The interesting questions relating to the phyllotaxy and symmetry of the flower are clearly stated, but no new light is brought to bear upon them, — nor all of the old. The opposition of the stamens of *Rhamnaceæ* to the petals is, as usual, attributed to the probable suppression of an outer stamineal circle, although there is nothing in the blossom (as there is in *Geraniaceæ*, etc.) to base the supposition upon. And our author has overlooked the most natural of explanations for this and strictly like cases, the one moreover which tells directly against the doctrine of transverse chorisis, — namely, that in these cases of ante-position there is a return to normal phyllotaxy, *i. e.*, to the superposition of the corresponding elements of successive whorls, — a view first suggested, we believe, by Lestibudois.

"Real cases of collateral multiplication may probably be explained by comparison of a primary staminal leaf with an ordinary compound stem-leaf, and supposing the filament to subdivide like the petiole does [sic] in such cases." This is certainly the way we regard it; and as respects the application of this hypothesis to the stamens of *Cruciferæ*, we do not see what argument *Megacarpæa polyandra* brings against it; as the increase in the number of stamens is quite as explicable upon this as upon the ordinary theory. Indeed, our author's view that the glands represent suppressed stamens would seem to be negatived by this very case, since the glands have not disappeared with the increase of the stamens, but the contrary.

The abnormal fertile flowers of Viola and Impatiens are

not "achlamydeous," as our author states them to be (p. 90); generally they are not even apetalous.

In the botanical sense of the word, and as it is employed in the same sentence (p. 93), the petals of the vine cannot be said "to cohere above." The valvate petals are merely caducous for the most part before expanding, just as is more decidedly the case in many *Araliaceæ*. In passing, we remark that a valvate æstivation of the corolla in the latter is much less distinctive than our author supposes (p. 311). Aralia itself has the petals imbricated in the bud.

It is becoming common to regard the tube of a so-called superior calyx as a cup-like receptacle; and there appears to be reason for it in *Cactaceæ* and some other cases. Professor Henfrey would seem to apply this view universally; "for example, in *Rosaceæ*, *Umbelliferæ*, *Cucurbitaceæ*, *Compositæ*," [!] etc. But if applied to Rosa, why not to the *Sanguisorbeæ* and to other *Rosaceæ* with a calyx-tube lined with a disk bearing the stamens, etc.? And is the cup a receptacle in those *Melastomaceæ* which have an adnate ovary, but a calyx when the ovary is free? And how is it when the ovary and cup cohere only by the nerves of the latter?

For *paleæ* Professor Henfrey coins an English word, "pales" (p. 110), of which the singular would probably be "pale." We would propose to call them "palets."

There are convincing reasons why the perigynium of Carex cannot be regarded as a perianth, as our author takes it to be (p. 111).

It is not correct to say that the false dissepiments of Datura are formed "while the seeds are ripening" (p. 124); they equally exist in the ovary. And we doubt if the transverse false septa in Cathartocarpus and other *Leguminosæ* are "placental developments."

We are pleased to find that our author prefers to consider placentæ as belonging to the carpels rather than to the axis, although the close of paragraph 226 appears to imply the contrary.

We cannot agree that, "externally the campylotropous ovule resembles the anatropous, except that there is no

rhaphe" (p. 130). No attentive student could fail to recognize the difference, especially in the families cited (*Cruciferæ* and *Caryophyllaceæ*).

Ripening must be regarded in a remarkably broad sense when it is stated with emphasis, "that the distinction between endocarps and epicarps, in the common stone-fruits, arises entirely during the ripening of fruit." Also: "it is well known that the easy separation of the pulp from the stone is a sign of ripeness." When are cling-stone peaches ripe? Again: "In Taxus . . . during the ripening of the seed a succulent cup-like envelope grows up around it" (p. 136). Is ripening synonymous with the formation and growth, as well as the maturing of the fruit?

Lindley's system of the classification and nomenclature of fruits is adopted, with some modifications. It is well to have such a system, as an analysis of the diversities of structure; but of the thirty-six kinds so carefully defined and named only fifteen or sixteen are ever used in descriptive botany, or ever will be, it is devoutly hoped. There is much inconvenience in practice, and little advantage in designating every possible modification of the same organ or set of organs by a distinct substantive name, or in distinguishing by separate technical names fruits formed of a simple ovary from those of a compound ovary, or fruits with an adherent from those with a free ovary. Why not call the gooseberry and the grape equally a berry, instead of restricting this name to the former and naming the latter a *nuculanium;* and why name the pod of an Iris a *diplotegia*, while that of a Lily is called a *capsule?* And while we term the pod of *Saxifraga stellaris* a *capsule*, and that of *S. tridactylites* a *diplotegia*, what name are we to apply to that of *S. aizoides*, which is only half-superior?

Probably a wrong example is adduced on p. 148, for we cannot believe that any species of Ranunculus has the rhaphe averse from the placenta in the ripe fruit. By an oversight, on the same page, the fruits of *Labiatæ* are spoken of as seeds.

As respects the systematic part, the chapters on the prin-

ciples of classification, nomenclature, etc., strike us as sound and good throughout; and in the account of the natural orders a great amount of information, such as the medical student needs, is given in a comparatively small space. Errors or misconceptions will necessarily occur in the compilation of such an amount of materials, treating of structure, affinities, distribution, sensible properties, and medicinal or economical uses. They are not more numerous than was to be expected, and we are not disposed to make them the subject of criticism.

We may remark, in passing, that, as respects the morphology of the androecium in *Fumariaceæ*, the name of the writer of the present notice is referred to, by some misconception, as adopting Lindley's well-known hypothesis of the splitting of two stamens into halves; whereas he has maintained a very different view. And then this is mentioned as "offering a phenomenon of chorisis," which in that view is quite incomprehensible to us.

We were surprised at the statement that the bark and leaves of *Hamamelis Virginica* "are astringent and contain an acrid volatile oil" (p. 207). We trace it back to Lindley's "Vegetable Kingdom" (p. 784), and find, "The kernels of *Hamamelis Virginica* are oily and eatable. The leaves and bark are very astringent, and also contain a peculiar acrid essential oil;" and this, we find, comes from Endlicher's "Enchiridion." How did this bland and inert plant acquire such a reputation? Dr. Barton, who has figured it, says nothing of its possessing any sensible properties or useful qualities at all, except its use for divining-rods; nor do Pursh, Bigelow, Elliott, Darlington, etc., allude to any popular reputation of such qualities. No sign of any essential oil is to be detected in the foliage, and prolonged mastication of the leaves and bark while we write yields not the slightest trace of acridity and hardly any of astringency; no more, certainly, than a Beech leaf. We never heard of the seeds being eaten; and as they are "about the size of a grain of barley," or not much larger, and have a thick bony coat, they are not likely to become an important article of diet. After

some search, we find the source of these extraordinary statements in the "Medical Flora" of the eccentric Rafinesque. He says the seeds are called Pistachio nuts in the Southern States, are rather oily and palatable, etc., but he neglects to mention their size. He adds, "The bark and leaves are somewhat bitter, very astringent, leaving a sweetish pungent taste. The smell is not unpleasant. It has not been analyzed as yet, but probably contains tannin, amarine, extractive, and an essential oil." To all this, Endlicher, on the strength of "the sweetish pungent taste," has added the acridity; and so one of the blandest and most useless of shrubs gets a world-wide and wholly factitious reputation for active medical qualities and esculent seeds; and even Dr. Griffith, who must have known the shrub, has been induced to give it a place in his "Medical Botany."

Our remaining remark relates to the random way in which mere analogies are mixed up with affinities in estimating or expressing the relationship of orders, etc., in this as in some other more notable works. It is, or at least ought to be, well understood, that mere analogy, *i. e.*, likeness in some one respect only, however striking the imitation, is no indication of relationship, but that relationship rests upon affinity, *i. e.*, upon agreement or similarity in the whole plan of structure, and especially of floral structure, whether general or particular, as the case may be. To speak, therefore, of "evident" and "most distinct" affinities between *Coniferæ* and *Lycopodiaceæ* is an example of this prevalent misconception of what affinity is. This is more intelligible, however, than the "approach" suggested of *Aquifoliaceæ* to *Loganiaceæ* and *Apocynaceæ*, while their resemblance to *Celastraceæ* is thought to be of small account; or that of *Umbelliferæ* to *Rubiaceæ*, *Saxifragaceæ*, and even to *Geraniaceæ*, to which the resemblances do indeed "seem rather superficial." Again, *Xanthoxylaceæ* (*i. e.*, *Rutaceæ*) are said to have considerable affinity to *Oleaceæ*, because Ptelea, in the former, has a samaroid fruit, as has Fraxinus in the latter. May we add, as quite as much to the purpose, that the common Xanthoxylums have pinnate leaves, and are popularly called Prickly Ash?

The study of affinities is neither guess-work nor divination, but a matter of logical deduction from structure, based upon scientific principles, — principles recognized and acted upon by sound botanists with considerable unanimity, although they have never been reduced to a system, nor expounded in detail, so as to make them matters of elementary instruction. Until this desideratum is supplied, the young botanist can do no better than to take as models the writings of Brown, and of those botanists who, according to their ability, have most closely followed the footsteps of this master in science.

Having continued this review far beyond our intention at the outset, we have small space left for noticing the best part of Professor Henfrey's treatise, namely, the third or Physiological part. Suffice it to say, that, in the important chapter on the physiological anatomy of plants, our author writes from the fullness of his acquaintance with the writings and doings of all the continental phytotomists, and also with the authority of an experienced original investigator. And, so far as we know, it comprises much the best résumé of vegetable anatomy and development now extant in the English language, at once succinct, clear, trustworthy, and well brought up to the present state of the science. Perhaps the succeeding chapters, on the Physiology of Plants generally, the Physiology of Vegetation, and on Reproduction, are equally commendable in their way; but we have as yet barely glanced over the pages. We like the following definition, and the ensuing paragraph upon the rôle of vitality in plants.

"The physiology of plants is that department of botany in which we investigate the phenomena of the *life* of plants, manifested in a series of changes taking place in the diverse parts of which each plant is composed" — (p. 475).

"The physiological phenomena which indicate vitality are always of more or less complex nature, and admit of being analyzed into a number of factors, of which a large proportion are found to be purely physical or chemical. A very considerable part of the changes which accompany the process of organization are the results of the action of physical and chemical forces, [and] capable of being explained up to a

certain point, by the known laws of those forces. But in every case, after referring all the chemical and physical phenomena to their respective places, there remains a residual phenomenon to be accounted for, which is precisely the most important of all, — namely, that in living organic structures . . . the laws of inorganic matter are subdued under a higher influence, and caused to undergo modifications never occurring except in the presence of living matter, while — most important of all — the peculiar compounds of matter thus produced are not only made to assume forms, according to definite laws, totally unlike any forms of mineral matter, but [to] constitute bodies manifesting a continued interchange of material with the surrounding media, which, instead of resulting in decomposition, as in mineral bodies, effects a reproduction and increase of the already existing [organized] matter" — (p. 542).

In the paragraph on the longevity of trees (p. 549), we find renewed occasion to notice the longevity of unfounded statements, copied from one book into another long after the error has been pointed out. Here again the Adansonia of Senegal and the Wellingtonia or Sequoia of California figure as trees "whose age, deduced from the rings of growth of the stems, would amount to upwards of 3000 years." There is really no evidence to prove that the famous Baobabs described by Adanson are of such an age; and as to the Wellingtonia in question, an actual counting of the rings has shown that the tree was not half so old as it was vaguely computed to be.

The chapter on Reproduction appears to be excellent, as indeed we should expect. The geographical and geological part is necessarily very briefly treated.

NAUDIN ON THE GENUS CUCURBITA.

NAUDIN'S "Researches into the Specific Characters and the Varieties of the Genus Cucurbita"[1] are published in the 6th volume (4th series) of the "Annales des Sciences Naturelles," and are of no small interest, being founded upon a very conscientious investigation of nearly all the known forms, collected for the purpose, and cultivated under the author's eye at the *Jardin des Plantes*. These forms our author reduces to six species, and the alimentary sorts in cultivation to three, namely, *Cucurbita maxima*, *C. Pepo*, and *C. moschata*. The remaining three species are *C. melanosperma* of Braun, newly introduced from eastern Asia, and the two perennial and tuberous-rooted species, *C. perennis* and *C. digitata*, Gray, natives of our southwestern borders, the fruits of which are not esculent. Indeed, the Pumpkins and Squashes cultivated in Northern Europe, and with us, as now understood, belong to only two species, since the third, *C. moschata*, hardly comes to perfection north of the Mediterranean region. Of these, *C. maxima* is made to include *C. Melopepo;* and *C. Pepo*, comprising our Pumpkins and a large part of our Squashes, is made to include *C. ovifera*, *aurantia*, *verrucosa*, etc., and the species are defined by botanical characters, which apparently may be relied upon. The varieties of *C. maxima* fall into two main groups, characterized by their fruits, namely, the "Turbans," having crowned fruits, that is, the summit projecting beyond the adnate calyx-tube, a peculiarity found in no other species, and the crownless sorts, in which this peculiarity is not manifest. The innumerable varieties of *C. Pepo* are arranged in seven groups, according to the configuration of their fruits.

M. Naudin has not undertaken to discuss the questions respecting the birthplace of these plants. He remarks that *C. maxima* and *C. moschata* have been known in European gardens scarcely above two centuries; but that *C. Pepo* was perhaps known to the Greeks and the Romans in the time of Pliny.

[1] American Journal of Science and Arts, 2 ser., xxiv. 440.

The younger De Candolle, in his discussion of the history and origin of the principal cultivated plants, which forms a most interesting chapter of his "Géographie Botanique," although he is unable to assign them to any country as their home, confidently (perhaps too confidently) refers all the squashes and pumpkins to the Old World; but not to India, because they have no Sanscrit name. He will not believe that any of them came from America, and appears to think little of the current statements that squashes or pumpkins were in cultivation by our aborigines before the European settlement of the country. On the other hand, our lamented Dr. Harris — who, during the later years of his life, assiduously studied this question, and who was very cautious in drawing conclusions — had become satisfied that the North American Indians, as far north even as to Canada, cultivated squashes and pumpkins, one or both, along with their maize, before the whites were established here. We are unable at this moment to refer to his manuscripts, or to what he had too imperfectly published upon this subject. But we well remember his laying much stress upon the narrative of Champlain; and with good reason, as it appears to us on turning casually to the pages of "Les Voyages du Sieur de Champlain . . . ou Journal tres-fidèle des Observations faites et Découvertes de la Nouvelle France," etc., etc., edition of Jean Berjon, Paris, 1613, 4to; also "Voyages et Découvertes faites en la Nouvelle France depuis l'année 1615, jusques à la fin de l'année 1618," — second edition, published by Collet in 1627, small 12mo, — to which volumes we desire to direct M. De Candolle's attention. In Champlain's narrative of his own voyage along the coast of what is now the State of Maine, in the year 1604, and the two voyages of Le Sieur de Mons along the coast of New England in 1605 and 1606, *Citrouilles* and *Courges* are repeatedly mentioned, along with maize (*Bled d'Inde*) and beans; *e. g.*:

"Nous y vismes force *citrouilles*, *courges* & petum, qu'ils cultiuet aussi. . . . Pour les febues elles cõmẽçoiẽt à entrer en fleur, cõme faysoyẽt les courges et citrouilles" (p. 68).

"Ceux que nous auions enuoyes deuers eux, nous appor-

terent des petites citrouilles de la grosseur du poing, que nous mangeasmes en sallade comme coucombres, qui sont tres-bonnes" (p. 77).

See also pp. 83, 115, 116. Of course it does not follow that these esculents were natives of New England, any more than maize; but both may probably have been carried northward together. Whatever their origin, our Indians were found cultivating them together at this early date as well as in later times. According to Nuttall, the Indians along the whole Upper Missouri half a century ago were cultivating *Cucurbita verrucosa*. This common squash is, according to Naudin, a variety of *C. Pepo*, as also is *C. aurantia* (the *C. Texana* vel *ovifera*, Gray, "Pl. Lindheimerianæ"), which has every appearance of being indigenous in the western part of Texas, on the Rio Colorado and its upper tributaries. At least, this is the opinion of Mr. Lindheimer and of Mr. Charles Wright, two good judges. The latter personally informs us that, from the stations and localities in which alone it is met with, he could not suspect it to be other than an indigenous plant.

That the later Greeks and Romans possessed the bottle gourd or Lagenaria, and also some kind of summer squash, seems pretty clear; but we see no decisive reason for the opinion that they had any form of *Cucurbita Pepo*, as that species is now understood. According to De Candolle, the earliest figures referable to this species are, one of *C. ovifera* by Lobel in 1576, and one of *C. verrucosa* by Dalechamp in 1587, namely, about a century after the discovery of America, and long after maize had become well known in the south of Europe; and we have seen that some forms probably of this very species (undoubtedly originating in a warmer region) had by this time found their way in this country nearly as far north as the climate will permit of their cultivation. So that there appears to be about the same evidence for the American origin of some squashes and of pumpkins that there is for the American origin of maize.

A remaining argument brought by De Candolle against this view may also be turned the other way, namely, that no

certain species of the genus is known as indigenous to America. He has equally allowed that none is known to be indigenous to the Old World. Now of the six species recognized by Naudin, two only are known in their natural wild state, and these are our southwestern species with perennial roots, namely, *C. perennis* and *C. digitata*, to which we add that *C. Pepo* itself (*i. e.*, *C. ovifera* or *aurantia*) grows wild in the same district with *C. perennis*, and has the same appearance of being indigenous there. We leave the subject with these incidental remarks, as we did not intend here to investigate this question, and will briefly allude to another subject, upon which Naudin's investigations have thrown new light.

It is generally thought that the cultivated *Cucurbitaceæ*, and especially that the species of Cucurbita, cross-breed with extreme facility. According to Naudin this is true of the races only *inter se*. A good illustration of the immediate and great variation from this cause in the fruit of *C. Pepo* is given in Naudin's third plate, where fifteen different forms of the fruit are figured, taken from as many individual plants raised from seeds of one fruit, which had grown in the vicinity of other varieties. It is by no means certain, however, that all these forms originated from direct crossing. But the species themselves strangely refuse to hybridize. Naudin carefully experimented with the five species in cultivation at the *Jardin des Plantes* (namely, all known, except *C. digitata*); and out of seventy distinct trials all but five were utterly ineffectual. In five instances the fruit set, indeed, but in none of these was a single seed containing the vestige of an embryo produced! What are we to think, then, of the universal belief that squashes are spoiled by pumpkins grown in their vicinity, or pumpkins by squashes; and even melons (which are of a different genus) by squashes? The fact of some such influence seems to be well authenticated. Dr. Darlington, one of the most trustworthy of observers, speaks of it from his own knowledge, thus: "When growing in the vicinity of squashes the fruit [of the pumpkin] is liable to be converted into a kind of hybrid, of little or no value. I have had a crop of pumpkins totally spoiled by that cause, the fruit be-

coming very hard and warty, unfit for the table and unsafe to give to cattle." — ("Fl. Cestrica," ed. 2, p. 555.)

Now that this is not the effect of hybridation is clear from the fact that the result appears in the fruit of the season, not in that of the next year, namely, in a generation originated by the crossing. A clue is perhaps furnished by Naudin's observations, that the ovary is apt to set and even develop into a fruit in consequence of the application of the pollen of another species, although, as the result proves, none of the ovules are fertilized. And he hazards the conjecture that the pollen may exert a specific influence first upon the ovary, inciting its farther development, and then upon the ovules. To test this conjecture he was to examine the action, if any there be, of the pollen of Cucurbita upon the ovary of melons. The past summer — which has been as unusually warm in western Europe as it has been cool in this country — must have favored such researches in Paris; and we may expect soon to hear of the result. Improbable as such an influence seems to be, it is hardly more so than the now authenticated fact that the graft of a variegated variety of a shrub or tree will slowly infect the stock, so that the variegation will at length break out in the foliage of the natural branches; — an old observation, which, according to the Gardner's Chronicle, has recently been verified in several instances.

WEDDELL'S MONOGRAPH OF URTICACEÆ.

Dr. Weddell's preliminary studies upon the proper *Urticaceæ* were published a few years ago in the "Annales des Sciences Naturelles." Since then, botanists, aware from this and his other works that the subject was in most able hands, have been anxiously waiting for his full monograph. This, we understand, is now completed, although the last fasciculus has not yet reached this country. The greater part is before us, and an admirable monograph [1] it is, worthy of a place in

[1] *Monographie de la Famille des Urticées.* Par H. A. Weddell (*Archives du Museum*, ix., livr. 1–4), 1856–57. (American Journal of Science and Arts, 2 ser., xxv. 109.)

the "Archives" which contain that model one on the *Malpighiaceæ* of his lamented botanical master. It illustrates in detail about 470 species, under 40 genera, and is accompanied by twenty well-filled plates, drawn by the author. It opens with a conspectus of the members of the great group to which the true *Urticaceæ* belong (which the author inclines to receive rather as the orders of a class than as suborders of an extensive order, fully admitting, however, their close affinity *inter se*), followed by a brief indication of the principal investigators of these plants, and of the resources at his own command. A general account of the organs of vegetation and reproduction, of the affinities, and the geographical distribution of the plants of the group, and of their properties and uses, conclude the preliminary matter. The body of the work is occupied by their systematic arrangement and description.

Apetalæ being viewed as degenerations of *Polypetalæ*, our author searches among the latter orders for the nearest relatives of the great Urticaceous order of alliance, and finds them in the *Tiliaceæ*, that is, in the group of orders of which the *Malvaceæ* are the highest development. According to Weddell's happy illustration, *Malvaceæ* crown the summit of a three-sided pyramid, with *Sterculiaceæ*, *Byttneriaceæ*, and *Tiliaceæ* just below them, one upon each face; under the *Byttneriaceæ* he ranks the *Euphorbiaceæ* with the *Antidesmeæ*, and under these, at the very base of the pyramid, the *Scepaceæ*, the lowest degradation in this direction of the Malvaceous type. On the adjacent face, under the *Tiliaceæ*, and on the same level with the *Euphorbiaceæ*, he inscribes the *Urticaceæ*, with the *Cupuliferæ* perhaps underneath them. Upon this ingenious plan of representation, the apetalous orders throughout may be most conveniently and instructively ranked under their superior types; — bearing in mind that some types degrade as much within an order (*e. g.*, *Euphorbiaceæ*, *Onagraceæ* inclusive of *Haloragieæ*, *Caryophyllaceæ* including *Illecebreæ*), as others do through a series of two or three orders, or even as the same group does (*e. g.*, *Caryophyllaceæ*) through a series of orders on the other side of the pyramid.

The reason why this mode of representation will exhibit botanical affinities so well is, that (as we have elsewhere remarked) the vegetable kingdom does not culminate, — as the animal kingdom does, — and therefore offers no foundation in nature for a lineal arrangement even of its great groups. But it would appear that the Dicotyledonous orders might be arranged under a considerable number of short series, in groups converging upon the most fully developed or representative order of each type, so as to exhibit what we now know of the system of nature much better than in any other way.

We think that Dr. Weddell's idea of the affinity of *Urticaceæ* is a good one. The floral and seminal characters, the true *criteria* of affinity, are not abhorrent, but present some strong points of relationship, as do the organs of vegetation. These, once established, allow us to feel the force of the striking coincidence in the bast-tissue of the bark, so remarkable in all this alliance for the length, fineness, and toughness of the fibres, their union end to end, and their lateral independence, admirably adapting them for their use as textile materials, in which *Urticaceæ* vie with *Malvaceæ* and *Tiliaceæ*.

As to geographical distribution, Europe is very poor in *Urticaceæ*, poorer even than would at first view be supposed, as the author remarks. For as nettles like an enriched soil, the five or six European species of Urtica and Parietaria so abound around habitations that they make up in the multitude of individuals for the paucity of species, and perhaps cover nearly as much ground as the great number of intertropical species; two or three excepted, which are also weeds in the tropics. Temperate North America is not much richer in species than Europe. The greater part are found in the torrid zone, and in islands rather than continents; the Malay region, India, Mexico, and the West Indies together possess almost two thirds of the known species.

Our remaining remarks shall be restricted to one well-known plant described in the work, and to another of recent discovery, which unfortunately was not communicated in season to find a place in it.

The first is our common *Pilea pumila*. Dr. Weddell has overlooked the fact that Rafinesque had founded a genus (Adice or Adike) upon it, although the name is mentioned in the work cited by him, where the plant was first published as a Pilea, and although Dr. Torrey had adopted Rafinesque's genus, and figured the species, in an earlier and more considerable work ("Flora of the State of New York"), which, having unfortunately been published by the State, and in a large edition, has in consequence remained almost unknown to science. Considering that the three sepals of the fertile flower in this species are nearly equal and not gibbous, it may be doubted whether the single species of Blume's genus Achudemia, differing only in having five sepals, should not rather be appended to Pilea. We dare say that Dr. Weddell would have so arranged it, if Blume had not published the genus.

Since the appearance of the third part of Weddell's monograph, but before it had reached this country, Dr. Torrey has published, in the report on Dr. Bigelow's fine California collection made in Lieutenant Whipple's Railroad Survey to the Pacific, a new Nettle allied to Bœhmeria but with the penicillate stigma of Urtica, namely, his *Hesperoenide tenella* ("Pacific Railroad Reports," iv. p. 139). This little plant, it now appears, comes nearest to Wight's monotypic genus *Chamabaina* of India, of which better details than Wight's as to the female flowers and fruit are figured in the present monograph. The stigma is intermediate in character between that of Chamabaina and that of Urtica; and, moreover, as the sepals of the male flower want the pointed gibbous tips of the former, the stipules are inconspicuous, and the cotyledons are not only reniform but (which is unnoticed in the published description) pretty strongly emarginate at the summit also, the genus will probably be retained.

Great thanks are due to Dr. Weddell for his labors upon this family, which he found in a most unsatisfactory and difficult state, and has left in such condition that Nettles and their allies are easy and inviting objects of study.

RADLKOFER'S PROCESS OF FECUNDATION IN THE VEGETABLE KINGDOM.[1]

THIS gives in English, and in an accessible form, a systematic and historical survey of the whole subject of vegetable fecundation, including the recent discoveries of Pringsheim, Cohn, Braun, and Bary.

As to *Fungi* and *Lichens*, — thanks to the observations of Itzigsohn upon the latter, and the most useful and persevering investigations of Tulasne upon both families, — the analogues of male organs in all probability are discovered, and their general presence recognized; but the fact of fecundation is not made out.

In the lower or green *Algæ*, fecundation was first demonstrated by Pringsheim. The "horns" of Vaucheria which Vaucher half a century ago observed and conjectured to be male organs, Pringsheim proved to be so, having seen them open at the summit and emit a great number of free-moving corpuscles (spermatozoids), many of which found their way into the now open orifice of the protuberance, which contains the forming spore, and were seen crowding against it, after which a membrane of cellulose appears over the surface of the mass of protoplasm and completes the spore. Whether one or more of the spermatozoids actually penetrates the protoplasm and so is included within the cell-membrane is uncertain; but Pringsheim thought it was the case, from having detected a colorless corpuscle like one of the spermatozoids inside of the membrane. Next Pringsheim demonstrated a similar fecundation in Œdogonium. His results, briefly published in the proceedings of the Berlin Academy, and thence translated into French and English, are now given in detail in the first part of his "Jahrbücher," noticed above. Œdogonium consists of a row of cylindrical cells.

[1] *Der Befruchtungsprocess im Pflanzenreiche.* L. Radlkofer. Leipsic, 1857. (English translation by Arthur Henfrey in *Annales* and *Magazine of Natural History*, October and November, 1857.) (American Journal of Science and Arts, 2 ser., xxv. 112.)

Some of these cells, usually shorter than the rest, become tumid, and, without conjugation, have their whole green contents transformed into a large spore. Pringsheim has ascertained that other cells of the same individual plant have their green contents transformed into a multitude of active corpuscles or zoöspores, which, from their subsequent evolution and office, he names androspores; these escape by the opening of the mother cell moving about freely by the vibration of a crown of cilia attached near the smaller end. One or more of these androspores fix themselves by the smaller end upon the surface of the cell in which a large ordinary spore is forming, or in the vicinity, and germinate there, growing longer and narrower at the point of attachment, while near the free end a cross partition forms, and sometimes another, making one or two small cells; this is the true antheridium: for in it a crowd of spermatozoids are formed, also endowed with motivity by means of vibratile cilia. Now the top of the antheridium falls off as a lid, the spermatozoids escape; the spore-cells at this time open at the top; one of the spermatozoids enters the opening, its pointed end foremost; this becomes stationary upon or slightly penetrates the surface of the young spore, into which its contents are doubtless transferred, and a coat of cellulose is then, and not until then, deposited upon it, completing its organization as a spore, which in due time germinates, and grows directly into a plant like the parent.

But in Bulbochæte, and especially in Sphæroplea, so beautifully investigated by Cohn ("Annales des Sciences Naturelles," 4 ser., v.), the spore does not directly develop into the normal or fruit-bearing plant. Instead of this, by an alternation of generations (to adopt that well understood phrase), the spore proceeds to convert its contents by successive division into a large number of zoöspores, different from the androspores, namely, small oval or oblong bodies, furnished with two long cilia on a short beak at one end, and for a time moving actively about by their vibration. Coming to rest, these zoöspores germinate, by elongation and the formation of transverse partitions, into adult thread-like plants, consisting of a

row of cells. In Sphæroplea the whole contents of the cells of some adult individuals condense into large green spores, as yet without a coat; while those of different individuals give rise to myriads of slender spermatozoids, moving by means of a pair of cilia fixed at the narrow end. The latter escape from the parent cell through a small perforation which now appears, enter the spore-bearing cells of the fertile plant through a similar perforation in them, play around the spores, and at length one or more of them drives its pointed extremity into their naked surface; after which, fertilization being accomplished, a thick coat of cellulose is deposited to complete the spore. "Cohn does not consider that observations justify his assuming a direct penetration of the spermatozoids into the primordial spore-cell. It rather seemed to him as if they attached themselves on the outside of the spore, and were finally converted into mucilaginous globules."

Reproduction by conjugation of course had long been familiarly known in the lower *Algæ*. But it was questioned whether this was really analogous to sexual reproduction, since what appeared to be similar spores are often formed of the contents of a single cell without conjugation. Areschoug shows that these are abortive spores, incapable of germination; while those which result from actual conjugation will grow into new plants, without further metamorphosis; Vaucher's old observations to this effect having been confirmed by Braun and Pringsheim.

That in the *Fucaceæ* or olive-green *Algæ* the large spores are fecundated by spermatozoids, produced in antheridia, was demonstrated by Thuret in the year 1850. And in more recent memoirs he has shown that the fertilization takes place through direct contact of the spermatozoids with the naked surface of the unimpregnated spore, then having only a protoplasmic coating; and that these spores will not develop nor hardly acquire a cell-wall unless so fertilized. His experiments upon diœcious species are perfectly decisive upon these points. He observed the lively spermatozoids playing over the surface of the still naked spore, fix them-

selves to it by the ciliated end, apparently by one of the cilia, and at length come to rest in contact with it; but he could not detect any material penetration of them into the body of the spore. Pringsheim, confirming all Thuret's observations, thinks also that the spermatozoids actually penetrate the sperm-mass; but there is no direct proof of it. Indeed Thuret, in a very recent article ("Annales des Sciences Naturelles," 4 ser., vii., 1857), indicates the grounds of Pringsheim's probable mistake. The most interesting point in this last article by Thuret relates to the suddenness with which the cell-membrane is formed on the spore of Fucus after the access of the spermatozoids and the accomplishment of the act of fecundation. In six or eight minutes traces of the formation of the membrane are recognizable upon a considerable number of the spores. In ten minutes the presence of a membrane may be clearly made manifest by the application of chloride of zinc. In an hour the membrane has acquired considerable firmness and thickness, and the presence of cellulose is revealed by the action of sulphuric acid and iodine; an hour later and the blue coloration under the test is decided.

In the higher *Cryptogamia* and in the *Phanerogamia*, Radlkofer's treatise, though interesting for the history, offers nothing new to our readers. In fact, its date precluded it from containing much of what is referred to in the preceding paragraphs. But the subject is still to be continued.

DR. HOOKER ON THE BALANOPHOREÆ.

Although read before the Linnæan Society nearly three years ago, this fine memoir [1] was published only last summer. The delay has probably been owing, in a great part, to the time requisite for the engraving of the very beautiful and

[1] *On the Structure and Affinities of Balanophoreæ*, by J. D. Hooker, (separately issued from the *Transactions of the Linnæan Society* of London, xxii.) London, 1857. (American Journal of Science and Arts, 2 ser., xxv. 116.)

elaborate plates which illustrate the memoir. It is a clear, patient, and philosophical elucidation of an extremely anomalous group of plants, and a succinct exposition of the principal lessons to be learned from their study, both organographically and systematically; and it bears the impress throughout of the spirit, freshness, and independence which so distinguish this author, and make all his writings so attractive and instructive. While the whole subject is developed in proper order, the divisions are not quite clearly marked out in the essay. The first sectional heading is: "1. Parasitism and structure of the Rhizome." But there is no section 2 answering to the first, which moreover continues, without a break, to treat of the general anatomy, organography, and morphology of these plants, the structure of the flowers, ovules, and seeds, and of the diverse doctrines which have been propounded respecting them. The affinities of *Balanophoreæ* are then considered under a special heading; their Classification is then the subject of a few general remarks; also their Geographical Distribution and Variation. Then a Synoptical Table of the genera is given; and the fourteen genera with their known species (28 in all) are finally described and illustrated.

As to the structure and affinities of *Balanophoreæ*, and the curious questions that have arisen about their place in the natural system, Dr. Hooker, in the first place, affirms them to be truly phænogamous. It now seems strange that this should ever have been doubted. The arguments to the contrary, says our author, "all appear to have originated, on the one hand, in mistaking feeble analogies between the forms of organs that are not homologous, for affinities; and, on the other, in overlooking a multitude of positive characters. These arguments may be summed up as: — 1. An erroneous view of the nature of the seeds, by Endlicher, Martius, Blume, and others, who describe them as a sporuliferous mass, — a term which, even if it were applicable, has no meaning. 2. An erroneous view of their origin being in a diseased state of the plants they grow upon, adopted by Junghuhn and Trattinick. 3. A supposed similarity in appearance to *Fungi*, and an erroneous

idea that their appearance is meteoric and their growth rapid; — a theory advanced by Endlicher, who says of the horizontal rhizome of Helosis and Langsdorffia, 'mycelio Fungorum quam maxime analogum.' 4. The resemblance between the articulated filaments on the capitula of the *Helosideæ* and the paraphyses of *Musci;* and between the pistils of *Balanophoreæ* and the pistillidia of Mosses; strongly advocated by Griffith and Lindley. 5. The resemblance of the cellular and vascular tissues in some of their characters to some of those of Ferns, as indicated by Unger and Gœppert. 6. A very peculiar view of the nature and relations of the parts of the female flower entertained by Weddell; who hence considers *Balanophoreæ* (together with *Rafflesiaceæ*) to approach nearer to Gymnosperms than to any other group of plants." Instead of discussing at length opinions which "had the authors who advocate them been sufficiently furnished with specimens and facts they would never have entertained," Dr. Hooker merely recalls attention to the essential facts that these plants exhibit true flowers with stamens and pistils, genuine ovules, and even embryo, and so accord in no one particular with Cryptogams. He shows moreover that the embryo is dicotyledonous in the few cases where it is sufficiently developed to manifest the character, and that the stem is constructed upon the exogenous plan. Even with these facts before him, Lindley has retained his Rhizogens, as "logically a class"; as an intermediate form of organization between Endogens and Thallogens, and characterized by vegetation rather than fructification. But there is little or nothing really peculiar in their vegetation; and, as Lindley himself reduces the differences to questions of degree, it suffices to say that the classes are not founded upon degradation of type, but upon change of type.

Viewing *Balanophoreæ*, then, as degraded members of the Dicotyledonous class, Dr. Hooker follows Brown and Griffith in regarding *Rafflesiaceæ* as near to *Aristolochiaceæ*, and in denying all affinity between these and *Balanophoreæ*. In searching for the affinities of the latter, Dr. Hooker is guided by the sound rule of disregarding "the negative characters,

as those may be termed which are founded on the imperfections of organs;" and he takes the most perfectly developed species as the best exponents of the typical structure of any group, — a principle laid down, we believe, by Mr. Brown. This gives a substantial scientific basis for the estimation of affinity. Agreement in plan of structure is just what constitutes affinity; agreement in grade of evolution may indicate only distant analogy, can indicate only collateral relationship, — not to be neglected, indeed, but in itself of no account in assigning a family to its true position in a system. The principle as applied in the present case leads Dr. Hooker to the conclusion that the nearest relatives of *Balanophoreæ* are the *Heloragеæ*, a group itself, "consisting for the most part of reduced forms of *Onagrarieæ*," or, more strictly speaking, that the link which connects these plants with the higher forms of vegetation is furnished by Gunnera. The qualifying phrase above is appropriate; for it is hard to conceive of Gunnera with its minute embryo as a reduced *Onagracea*, while it is impossible to sever the chain of evidence which binds the genus to Loudonia and Haloragis. Be this as it may, Dr. Hooker has surely made a happy hit in seizing upon Gunnera as the key to the true affinities of *Balanophoreæ*. Of all the objections that may be urged against this approximation not the strongest, but rather the least valid, in our opinion (so long as the question is one of alliance and not of co-ordination), is that to be derived from the habit and the imperfection of the foliar organs. Any type is liable to have its parasitic phase, and this is generally a degraded one in these respects; the Gesneriaceous has it in *Orbancheæ*, which it might with the greatest propriety include; the Scrophulariaceous graduates insensibly into similar parasitic forms; the Ericaceous has them in *Monotropeæ;* and the Cornaceous or Olacaceous degrades through *Santalaceæ* into *Loranthaceæ*.

It is quite probable that our author would deny the degradation in the latter case, judging from some points which he makes when considering whether the group of *Balanophoreæ*, "putting aside any consideration of its relationship with other

orders, and regarding it *per se*, . . . should abstractedly be considered as ranking high, or the contrary." This is an abstraction of which we are hardly capable, — that of determining the rank of an order *per se*. Still our author's ideas are clear and clearly expressed; the comparison is really between these plants and the ideal plant-type. And what is wanting to make the comparison practical is a settled idea as to what constitutes the highest style of plant, and what is the relative importance of deviations from it; questions too large to be entered upon here, if indeed the science is yet ready for their discussion, but which underlie the most important inquiries which good systematic botanists are everywhere tentatively prosecuting. Assuming that the conventional definition of perfection in use among zoölogists is applicable to the vegetable kingdom, and which argues that a high degree of specification of organs and morphological differentiation of them for the performance of the highest functions indicate a high rank, Dr. Hooker ingeniously argues that "*Balanophoreæ* may in some respects be considered to hold a very high one;" and the points are presented under seven heads. Now we will not deny that the principles are logically applied in the present case, nor that the considerations of the kind are perhaps as applicable to the vegetable as to the animal kingdom. But we should *a priori* expect that principles of fundamental importance in the latter could have no sound application to the former; that even such as relate to functions common to the two, or to structures analogous, would require to be based each upon its own ground. As to morphology, and as to what constitutes perfection of type, we should look to the fundamental differences rather than to the resemblances of the two for our starting-point.

Plants for obvious reasons are constructed on the principle of extension of surface. Concentration or consolidation, wherever it occurs in the vegetable kingdom, is a special provision against some peculiar danger. Animals, on the contrary, are formed on the principle of restriction of surface. As if to withdraw them as much as practicable from the direct action of the external world, their shape is compact, their extent as

individuals strictly limited, the external organs by which they take their sustenance comparatively few and small, while the most essential organs are safely sheltered within. Consolidation of organs and even their restriction in number, accordingly, are not likely to be indications of high rank in the vegetable kingdom. Not the latter, because the object of the plant in vegetation is attained by the indefinite repetition of the same organs; nor the former, for the type of the plant is realized only in the distinct elimination of leaves from the axis. A Melon-Cactus and a Cuscuta are low forms of plants as to vegetation. As it is a fundamental character of plants that their organs of reproduction are only specialized organs of vegetation; as the higher great divisions of plants are those in which the leaf-type is most apparent throughout; as the perfect accomplishment of the end in view — the production, protection, and nourishment of the embryo even of the highest or most developed kind — does not require the confluence of homogeneous parts, why should such confluence be regarded as indicating higher rank, merely because the type is more disguised in such cases? We see no sufficient ground for ranking a monopetalous plant higher than a polypetalous one on that account; and still less for regarding a Loranthus or a Viscum as the highest style of plant. On the contrary, we incline to look upon the consolidation of heterogeneous parts in the blossom not as high specialization at all, but as want of development, *i. e.* imperfect elimination; and in this light those who maintain an inferior ovary to be one immersed in a receptacle, must needs regard it.

Again, suppression or abortion of organs that belong to the type of the blossom cannot be considered as other than an imperfection, although the loss of the corolla is no great matter, and the abortion of one of the sexes little more. Still hermaphroditism is plainly in the type of the highest style of plant; while the opposite is the case in the animal kingdom. But we cannot here enter further into the discussion of this class of questions. No one feels more deeply than our author the want of fixed and philosophical principles for the subordination of characters and the study of affinities in plants;

and no botanist of his age is more competent, or so well placed and furnished for the investigation of this problem, to which we invite him as to a task worthy of his powers.

As to the rank of *Balanophoreæ*, if our author has demonstrated anything, it is that they belong to the highest class of plants, but that they are probably the most degraded members of it.

BOUSSINGAULT ON THE INFLUENCE OF NITRATES ON THE PRODUCTION OF VEGETABLE MATTER.

Several years ago Boussingault demonstrated, in the clearest way, that plants are incapable of assimilating the free nitrogen of the atmosphere. Two years ago, in a paper communicated to the French Academy of Sciences, he showed that nitrates eminently favor vegetation. He now shows,[1] by decisive experiments, —

(1) That the amount even of ternary vegetable matter produced by a plant depends absolutely upon the supply of assimilable nitrogen (ammonia and nitrates). A plant, such as a sunflower, with a rather large seed, may grow in a soil of recently calcined brick, watered with pure water, so far as even to complete itself with a blossom; but it will only have trebled or quadrupled the amount of vegetable matter it had to begin with in the seed. In the experiments, the seeds weighing 0.107 grams, in three months of vegetation formed plants which when dried weighed only 0.392 grams, — a little more than trebling their weight. The carbon they had acquired from the decomposition of carbonic acid of the air was only 0.114 grams; the nitrogen they had assimilated from the air in three months was only 0.0025 grams.

(2) Phosphate of lime, alkaline salts, and earthy matters

[1] *Researches upon the Influence which assimilable Nitrogen in manures exerts upon the production of Vegetable Matter; and* (2) *Upon the Quantity of Nitrates contained in the Soil and in Water of various kinds.* J. B. J. D. Boussingault. *Annales des Sciences Naturelles*, 4 ser., vii., No. 1, 1857. (American Journal of Science and Arts, 2 ser., xxv. 120.)

indispensable to the constitution of plants exert no appreciable action upon vegetation, except when accompanied by matters capable of furnishing assimilable nitrogen. Two plants of the same kind, grown under the same conditions as above, but with the perfectly sterile soil adequately supplied with phosphate of lime, alkali in the form of bicarbonate of potash, and silex from the ashes of grasses, resulted in only 0.498 grams of dried vegetable matter, from seeds weighing 0.107 grams; and had acquired only 0.0027 grams of nitrogen beyond what was in the seeds.

(3) But nitrate of potash furnishing assimilable nitrogen, associated with phosphate of lime and silicate of potash, forms a complete manure, and suffices for the full development of vegetation. Parallel experiments with nitrate in place of bicarbonate of potash resulted in the vigorous growth of the Sunflower plants, and the formation of 21.248 grams of organic matter, from seeds weighing as before only 0.107. This 21.111 grams of new vegetable matter, produced in three months of vegetation, contained 8.444 of carbon derived from the carbonic acid of the air and 0.1666 grams of nitrogen. The 1.4 grams of nitrate of potash supplied to the soil contained 0.1969 grams of nitrogen, leaving a balance of 0.0303, nearly all of which was found unappropriated in the soil.

Finally Boussingault made a neat series of comparative experiments, introducing into calcined sand the same amount of phosphate of lime and carbonate of potash, but different proportions of nitrate of soda, or in other words of assimilable nitrogen, and watering with water free from ammonia but containing a quarter of its volume of carbonic acid. The soil was divided among four pots, each having two seeds of Sunflower (*H. argophyllus* was the species used in all the experiments); the pot

No. 1	received of	nitrate of soda	. . . . .	0.00	grams.
No. 2	"	"	"	0.02	"
No. 3	"	"	"	0.04	"
No. 4	"	"	"	0.16	"

The results of fifty days' vegetation are given in the rate

of growth, size and number of leaves, weight of the product, etc. : —

No. 1 made of new vegetable matter	. . .	0.397	grams.
No. 2	"	"	"	. . . 0.720 "
No. 3	"	"	"	. . . 1.130 "
No. 4	"	"	"	. . . 3.280 "

In No. 2 so little as three milligrams of assimilable nitrogen introduced into the soil enabled the plant to double the amount of organic matter. The proportion of the weight of the seeds to that of the plant formed was in

No. 1, as	1 :	4.6 grams.
No. 2, as	1 :	7.6
No. 3, as	1 :	11.3
No. 4, as	1 :	30.8

In no case did the nitrogen acquired by the plant exceed that of the nitrate added to the soil.

In the experiments where no nitrate was added to the soil the two or three milligrams of nitrogen acquired by the plants during three months of vegetation came in all probability from ammoniacal vapors and nitrates existing or formed in the atmosphere. To establish their presence, Boussingault arranged an apparatus which detected the production of some nitrates. And, in exposing to the air 500 grams of calcined sand, which had 10 grams of oxalic acid mixed with it, in a glass vessel with an open surface equal to that of one of the flower-pots used in the above experiments, the sand took 0.0013 grams of nitrogen from the air, of which a part was certainly ammonia.

The object of the researches of which a summary is given in the second paper was, to determine the quantity of nitrates contained, at a given moment, in one *hectare* of cultivated ground, one of meadow, one of the forest soil, and in one *metre* of river or spring water. The quantity in the soil was of course found to vary extremely with the extremes of wet or dry weather. Garden soil, highly manured every autumn, contained on the 9th of August, 1856, after fourteen dry and warm days, 316.5 grams of nitre in a cubic litre of soil. On the 29th of the month, after twenty rainy days, the same quan-

tity of the same soil contained only 13 grams of nitre. The greater part had been dissolved out of the superficial soil.

Some specimens of forest-soil, in a state of nature, furnished no indication of nitrates; others gave 0.7 and 3.27 grams of nitre to the cubic metre.

The soil of meadows and pastures afforded from 1 to 11 grams of nitre to the cubic metre. Nineteen specimens of good cultivated land gave, four of them none; others from 0.8 to 1.33; the richer ones from 10.4 to 14.4, and one fallow, of exceptional richness, as much as 108 grams of nitre to the cubic metre. To the latter much calcareous matter has been added.

The soil of the conservatory, from which the nitrates would not be washed away by rains, contained 89, or 161, and some rather deep soil 185 grams of nitre to the cubic metre.

The sources of the nitre are not difficult to understand when we reflect that a manured soil, especially a calcareous one, is just in the condition of an artificial nitre-bed. The ultimate result of the decomposition of ordinary manure is a residuum of alkaline and earthy salts, phosphates, and nitrates, the latter, with the ammonia furnishing the assimilable nitrogen, all-essential to productive vegetation. In incorporating with the soil undecomposed manure, instead of the ultimate results of decomposition, less loss is suffered from prolonged rains wasting out the formed nitrates.

The soluble matters washed out of the soil are to be sought in the water. River and spring waters therefore act as manure by the silex and alkali, the organic matter, and the nitrates which they hold. The spring waters, poorest in nitre of those examined, contained from 0.03 to 0.14 milligrams of nitre to the *litre;* the richer ones from 11 to 14 grams in the cubic metre.

As to the river-water, the Vesle in Champagne held 12 grams, the Seine at Paris 9 grams the cubic metre. These were the richest. The Seine at Paris carries to the sea, in times of low water, 58,000 kilograms, in times of high water, 194,000 kilograms, of nitre every twenty-four hours. What enormous amounts of nitre must be carried into the sea by the

Mississippi, the Amazon, and by every great continental river; and how active, beyond all ordinary conception, must the process of nitrification be all over the land; and how vast the supply of assimilable nitrogen for the use of vegetation!

BENTHAM'S HAND-BOOK OF THE BRITISH FLORA.

One of the best systematic botanists — of the soundest judgment and the largest experience, both in European and exotic botany — has deemed it no unfit employment of a portion of his valuable time to prepare a volume [1] by which beginners, having no previous acquaintance with the science, may learn to know most advantageously and readily, the wild flowers and plants of his native land. The result is a genuine popular Flora, and a clear proof that the plants of a limited country may be described, by one who understands them thoroughly, in comparatively simple language, without any sacrifice of scientific accuracy, or of scientific interest. No really good work of this kind was ever made by a compiler; and no one who has not essayed the task, can comprehend how thoroughly faithful writing for beginners brings one's knowledge to the proof.

The characteristic features of the work before us are: 1. The full use of analytical keys, after the mode of De Candolle's "Flora Française," leading easily not only to the order and the genus, but also to the species of the plant in hand. These keys, or analyses, are here made to supersede specific characters as such, neat and free in descriptions, longer or shorter according to circumstances, occupying their place. But generic characters are given with considerable fullness. 2. The exclusion of all technical terms which were not required for the purpose in view, and "the omission, in numerous instances, of microscopical, anatomical, or theoretical

[1] *Hand-book of the British Flora; a Description of the Flowering Plants and Ferns indigenous to, or naturalized in, the British Isles: for the use of Beginners and Amateurs.* By George Bentham. London, 1858. (American Journal of Science and Arts, 2 ser., xxvi. 413.)

characters, often of the greatest importance in scientific botany, but useless to the mere amateur." 3. The descriptions are original, and have been drawn up from British specimens in the first instance, and afterwards compared with the characters given in the standard Floras, and verified upon continental specimens from various parts of the geographical range of the species. As a describer of species (which is something very different from a describer of specimens), Mr. Bentham has no superior. 4. The geographical range of each species, at least its European range, is carefully specified; then the British stations are given in general terms, the object being to state where the plant is likely to be found, rather than to indicate the precise spot where it has been gathered. 5. The judicious limitation of species, and the reduction of a crowd of nominal or "critical" species to their supposed types, with a thoroughness which only a botanist of Mr. Bentham's great experience and authority could well venture upon. The following extract from the preface will explain his views: —

"Taking into account the omission of all plants erroneously indicated as British, it will still, no doubt, be a matter of astonishment that, whilst the last edition of Hooker and Arnott's Flora contains 1571 species, and that of Babington's Manual as many as 1708 (exclusive of Chara), the number, in the present work, is reduced to 1285. This is not owing to any real difference of opinion as to the richness and diversity of our vegetable productions, but is occasioned by a different appreciation of the value of the species themselves. The author has long been persuaded that the views originally entertained by Linnæus, of what really constitutes a species, were far more correct than the limited sense to which many modern botanists seem inclined to restrict the term; and that in most cases where that great master had good means of observation, he succeeded admirably in the practical application of his principles. At any rate, if those minute distinctions by which the innumerable varieties of Brambles, of Roses, of Hawk-weeds, or of Willows, have of late years been characterized, are really more constant and more important than the author's experience has led him to conclude, they cannot

be understood without a more complete acquaintance with trifling, vague, and sometimes theoretical characters, than he has himself been able to attain, or than can ever be expected from the mere amateur. . . . The species are limited according to what are conceived to have been the original principles of Linnæus: and the author, in submitting his views to the judgment of the scientific world, trusts that they will not be attributed to hasty generalizations, or conjectural theories, but that they will be generally recognized as founded on personal observation of living plants, made during many years' residence on the continent, as well as in this country, and on repeated comparison of specimens collected from the most varied and distant points of the geographical areas of the several species."

6. Popular names are employed and reduced to a system in accordance with the principles of botanical nomenclature. "An attempt has, on the present occasion, been made to give prominence to a series of English names to the British plants, rendering them as far as possible consistent with the recognized principles of systematic nomenclature, so essential for the study of plants. It was at first intended merely to have adopted those which are appended to all the genera and species in Hooker and Arnott's Flora; but the first attempts to apply them practically, gave evidence that they had never been framed with a view to being used by botanists, or amateurs, in the place of the Latin ones. It will be observed that there is among them a continual confusion between popular, trivial, and generic names; between epithets and specific names; between substantives and adjectives; that on frequent occasions one name is applied to several genera, or several names to one genus; that the number of words forming the name of a plant varies from one to five, instead of being constantly two; and that some of the names put forward as English, are very local, almost unknown or obsolete, and no easier to learn than the more useful Latin ones they represent. It became necessary, therefore, thoroughly to revise the whole system, and to recast it upon the Linnæan principles universally adopted for the Latin botanical names. . . . The full

statement of the principles which have induced the rejection of certain names, and the substitution of others, and the details of their application to individual cases, . . . are given at length in a paper prepared by the author, to be laid before the Linnæan Society on the publication of this Flora."

Criticism may well be deferred until this paper comes to hand. Of the propriety of an English nomenclature of some kind in a Flora where a great part of the plants have well-known vernacular names, there is no room for doubt; and if used at all, it is desirable that these names should be reduced to a systematic form. This is readily done for perhaps half of the common plants of the British Flora; but for the rest, the difficulties are various and much greater than one would imagine before making the attempt. The present undertaking must be deemed a decided success. What imperfections it has, are on the safer side. We should have inclined to a larger use of the vernacular for the generic names; and where they were inapplicable to the whole genera, to apply them to subgenera, *e. g.*, Apple and Pear, Gooseberry and Currant. As these are real and universal English generic names, they ought, if possible, to be given as such. Still we appreciate the reasons which appear to have compelled the adoption of Pyrus and Ribes as English names, though English they never can become. Our author is strongly disposed throughout to make the Latin name do duty as an English one, doubtless supposing that they may become popular appellations in time, as Geranium and Aster have done. Sometimes he adopts the Latin word entire; sometimes he truncates or anglicizes the termination. Happy instances of the latter sort are: —

Trigonel, from Trigonella.

Limosel, from Limosella.

Corydal, from Corydalis.

Corrigiole, from Corrigiola.

Chrysosplene, from Chrysosplenium; but why not Goldenspleen?

Samole, from Samolus; but why not Brookweed?

Limnanth, from Limnanthemum.

Scleranth, from Scleranthus; but why not Knawel?

Osmund, from Osmunda.

Myriophyll, from Myriophyllum; but why not Milfoil?

Matricary, for Matricaria.

Eupatory, for Eupatorium, etc.

Those names which are not at all to our tastes are: —

Cerast, for Cerastium; but if such a word must be coined, why not Holost, for Holosteum on the preceding page?

Doronic, for Doronicum.

Onopord, for Onopordon; why not Cotton-Thistle?

Polycarp (newly martyred), for Polycarpon.

Myosote, for Myosotis; in place of Forget-me-not.

Capsell, for Capsella; in the place of the vernacular Shepherd's-purse.

The best coinage of an English name is Rockcist for Helianthemum.

An Introduction of thirty-six pages teaches the elements of botany to beginners, and explains the technical terms used in the flora, and many besides. The definitions of perigynous and epigynous, however conformable to etymology, are not the quite usual ones, and are not adhered to in the work itself. We were not aware that "in general the word ovary is used to designate all the ovaries of a flower," unless when united into one body, and are glad to observe that the author does not use the word in this way in the body of the work, one or two instances excepted. We always supposed the word to be an exact synonym of the Linnæan germen. And if we may not use it, as botanists always have done, for the ovule-bearing portion of the pistil, whether simple or compound (reserving carpel for the simple of elementary pistil, whether separate or combined), then a new word must needs be coined for this very purpose. To mistake the radicle of the embryo for the root, is common to all English botanists. The short sections upon classification and the examination and determination of plants, are full of practical wisdom.

VILMORIN'S IMPROVEMENT OF CULTIVATED PLANTS.

THIS very interesting pamphlet[1] is a collection and reprint of several of Louis Vilmorin's important communications to the Central Agricultural Society of France and to the Academy of Sciences: to which is prefixed a French translation of a memoir upon the Amelioration of the Wild Carrot, contributed by his venerable father to the Transactions of the London Horticultural Society (but not before published in the vernacular of the author), which memoir, as the younger Vilmorin informs us, was the point of departure for his own investigations in this field, and even contains the germ of most of the ideas which he has since developed upon the theory of the amelioration of the plants from the seed. These papers claim the attention of the philosophical naturalist, no less than of the practical horticulturalist.

Most of our esculents are deviations from the natural state of the species, which have arisen under the care and labor of man in very early times. New varieties of these cultivated races are originated almost every year, indeed; but between these particular varieties, the differences, however well marked, are not to be compared for importance with those changes which the wild plant has generally undergone in assuming the esculent state. In this amelioration or alteration, as in other cases, *c'est la première pas qui coûte.* For the altered race, once originated, has more stability than the wild stock; it accordingly tends not only to degenerate (as the cultivator would term it) towards its original and less useful state, but also to sport into new deviations, in various directions, with a freedom and facility not manifested by its wild ancestors. This explains the readiness with which we continually obtain new varieties of those esculent plants which have been a long time in cultivation, while a newly-introduced

[1] *Notice sur l'Amelioration des Plantes par le Semis et Considerations sur l'Hérédité dans les Vegetaux.* Par M. Louis Vilmorin. Paris, 1859. (American Journal of Science and Arts, 2 ser., xxvii. 440.)

plant exhibits little flexibility. To detect the earliest indications of sporting, and to select for the parents of the new race those individuals which begin to vary in the requisite direction, is the part of the scientific cultivator. In this way, the elder Vilmorin succeeded in producing the esculent carrot from the wild stock in the course of three generations, — no addition to our resources, indeed, but significant of what may be done by art directed by science. By adopting and skillfully applying these principles, the younger Vilmorin has conferred a benefit upon France which (if she will continue to make sugar from the beet) may almost be compared with that of causing two blades of grass to grow where only one grew before, having, so to say, created a race of beets containing twice as much sugar as their ancestors, and indicated the practicability of its perpetuation. The mode of procedure, and the ingenious methods he contrived for rapidly selecting the most saccharine out of a whole crop of beets, as seed-bearers for the next season, are detailed in these papers.

Once originated, and established by selection and segregation for a few generations, the race becomes fixed and perpetuable in cultivation, with proper care against intermixture, in virtue of the most fundamental of organic laws, namely, that the offspring shall inherit the characteristics of the parent, — of which law that of the general permanence of species is one of the consequences. The desideratum in the production of a race is, how to initiate the deviation. The divellent force, or idiosyncrasy, the source of that "infinite variety in unity which characterizes the works of the Creator," though ever active in all organisms, is commonly limited in its practical results to the production of those slighter differences which ensure that no two descendants of the same parent shall be just alike, being overborne by that opposite or centripetal force, whatever it be, which ensures the particular resemblance of offspring to parents. Now the latter force, as Mr. Louis Vilmorin has well remarked, is really an aggregation of forces, composed of the individual attraction of a series of ancestors, which we may regard as the attraction of the type of the species, and which we perceive is generally all-

powerful. There is also the attraction or influence of the immediate parent, less powerful than the aggregate of the ancestry, but more close, which ever tends to impress upon the offspring all the parental peculiarities. So, when the parent has no salient individual characteristics, both the longer and the shorter lines of force are parallel, and combine to produce the same result. But whenever the immediate parent deviates from the type, its influence upon its offspring is no longer parallel with that of the ancestry; so the tendency of the offspring to vary no longer radiates around the type of the species as a centre, but around some point upon the line which represents the amount of its deviation from the type. Left to themselves, as Mr. Vilmorin proceeds to remark, such varieties mostly perish in the vast number of individuals which annually disappear, — or else, we may add, are obliterated in the next generation through cross-fertilization by pollen of the surrounding individuals of the typical sort, — whence results the general fixity of species in Nature. But under man's protecting care they are preserved and multiplied, perhaps still further modified, and the better sorts fixed by selection and segregation.

Keeping these principles in view, Mr. Vilmorin concluded that, in order to obtain varieties of any particular sort, his first endeavor should be to elicit variation in any direction whatever; that is, he selected his seed simply from those individuals which differed most from the type of the species, however unlike the state it was desired to originate. Repeating this in the second, third, and the succeeding generations, the resulting plants were found to have a tendency to vary widely, as was anticipated; being loosed, as it were, from the ancestral influence, which no longer acted upon a straight and continuous line, but upon one broken and interrupted by the opposing action of the immediate parents and grandparents. Thus confused by the contrariety of its inherited tendencies, it is the more free to sport in various ways; and we have only to select those variations which manifest the qualities desired, as the progenitors of the new race, and to develop and fix the product by selection upon the same principle continued for several generations.

It is in this way that Mr. Vilmorin supposes cross-fertilization to operate in the production of new varieties; and even in the crossing of two distinct species, the result, he thinks, is rarely, if ever, the production of a fertile hybrid, but of an offspring which, thus powerfully impressed by the strange fertilization and rendered productive by the pollen of its own female parent, is then most likely to give origin to a new race.

We cannot follow out this interesting but rather recondite subject in a brief article like this. But we are naturally led to inquire whether the history of those plants with which man has had most to do, and the study of the laws which regulate the production and perpetuation of domesticated races, may not throw some light upon the production of varieties in Nature; and whether races may not have naturally originated, occasionally, under circumstances equivalent to artificial selection and segregation. Some recent attempts which have been made in this direction we may hope to notice upon another occasion.

THE BUFFALO-GRASS.

The Buffalo-Grass,[1] so abundant and so widely diffused over the broad, arid region which separates our Pacific from our Atlantic possessions, is one of the humblest plants of its order, rising only a few inches above the surface of the soil; but at the same time it is one of the most important and useful, since it forms the principal subsistence of the buffalo for a part of the year, and no less so of the cattle of the emigrant. The botanical history of this little grass, now happily completed by Dr. Engelmann, is remarkable. Nuttall first named and described it nearly thirty years ago; and, while he referred it to Sesleria, suspected it to be *sui generis*, and threw

[1] *Two new Genera of Diœcious Grasses of the United States.* By George Engelmann, M. D. Extr. from the Transactions of the Academy of Natural Sciences of St. Louis, i. p. 431; with three plates. 1859. (American Journal of Science and Arts, 2 ser., xxviii. 439.)

out a happy conjecture as to its natural relationship. Torrey figured it twelve years ago, and also announced its affinity to the *Chlorideæ;* he at the same time discovered its dicœcious character, and showed that only the male plant was known. At length Dr. Engelmann has detected the female plant in a rather rare grass, the *Anthephora axilliflora* of Steudel, which is so unlike the common Buffalo-Grass that it naturally had been referred to a widely different tribe. Struck by the similarity of their foliage and stoloniferous growth, as they occurred together in a collection made by his brother, Dr. Engelmann shrewdly suspected the relationship, and finally set the question at rest by finding a male Buffalo-Grass which happened to bear a stalk of female flowers from the same root-stock; and these flowers were those of the so-called Anthephora. So different are the two that nothing short of this ocular proof would have been convincing. It hardly need be said that the male plant is not a Sesleria, nor the female an Anthephora; although they severally resemble these genera, or at least the female spikelets have a very great external resemblance to the Paniceous genus Anthephora. So that Dr. Engelmann, having to characterize this new generic type, very naturally named it Búchloë (shorter and more euphonious than Bubalóchloë), *i. e.* Buffalo-Grass; and he retained the specific appellation of Dactyloides, although the male plant is not much like a Dactylis, and the female wholly unlike. Very glad we are to see the genus established under so appropriate a name, — the more so as it has narrowly escaped a different fate. That is to say, two inchoate attempts seem to have been made to found a genus upon the male sex. First, in Sir William Hooker's enumeration of the plants of Geyer's western collection we find "*Calanthera dactyloides.* Kth. — Nutt. Sesleria, Nutt. Gen. i. p. 65." But neither Kunth nor any other author has described a genus Calanthera. We have a suspicion that the "Kth." is a slip of the pen, and that the name is really Nuttall's, given by him to a specimen in the Hookerian herbarium. But if this be so, the manuscript name (which, moreover, is destitute of any particular significance) can by no means now supersede Engelmann's published

one; though we might have been constrained by courtesy to adopt it, if this suspicion had occurred to him, and he had been able to confirm it. Again, in the corrections at the close of the "Plantæ Hartwegianæ," Mr. Bentham applies the name of "*Lasiostega humilis*, Rupprecht (ined.)" to No. 250, which he had before called a Triodia. The plant is undoubtedly a male Buffalo-Grass. But no genus Lasiostega is found to be published, nor has this name any appropriateness as applied to the plant in question.

It is curious to remark that the male plant, being more proliferous by stolons than the female, has nearly displaced the latter, or has (so far as known) attained a wider geographical range as well as a far greater abundance. Probably, in accordance with a general law, the tendency to barrenness from seed which accompanies copious multiplication by offshoots, has also assisted in the production of this result, — a state of things quite contrary to the genius of that polygamous community which has effected a lodgment in the region of Buffalo-Grass.

Dr. Engelmann's second genus, Monanthochloë, is founded upon a singular, exceedingly stoloniferous, littoral grass with leaves scarcely half an inch long, with solitary sessile spikelets, which has long been known to occur on the coast of Texas and Florida (collected by Berlandier, Drummond, and Blodgett), but has never been studied until now. In fact, it has been thought to be something abnormal, on account of its showing as its most interesting feature, a regular transition from the foliage to the paleæ of the flowers. Dr. Engelmann notes that the glumes are wanting (perhaps represented by ordinary leaves of the axis of which the spikelet is a direct continuation), the uppermost leaf representing the lowest palea of the spikelet. The latter consists of from three to five flowers, of which the lowest flower and sometimes the next are neutral or rudimentary, from one to three succeeding ones are staminiferous or pistilliferous, according to the sex, and the uppermost is also reduced to a rudiment. In the hands of agrostologists such a grass as this will be likely further to elucidate the floral structure of the order, the

theory of which is by no means settled yet. Dr. Engelmann's three excellent plates, displaying all the details of the flowers, will facilitate this investigation.

The youthful Academy of Natural Sciences of St. Louis is well inaugurating its public career by publications of such character as this paper, and the more elaborate "Monograph of Cuscuta" by the same author, which is now in press.

THE TREES OF NORTH CAROLINA.

We have turned over the pages of this popular exposition[1] with much interest, and gleaned some valuable information. "Botanists will of course find fault with it," says the author, who we well know could write scientifically and profoundly enough, if he so pleased, but who has here come down to the level of his most unlearned readers, discoursed separately of trees, shrubs, and vines, and classified these in a fashion which might well shock the susceptibilities of a stickler for technical nomenclature and natural system in botany. Now, we are not shocked at all; indeed we quite enjoy a glimpse of Flora en deshabille and slip-shod, and are well aware how much easier it is, and how much better in such cases, to fit your book to its proper readers than to fit the readers to it. The fault we should find is not with the plan of this Report but with the quantity. We could wish for more of it, for a volume as large at least as Mr. Emerson's Report on the Trees and Shrubs of Massachusetts. We quite like to see the popular names put foremost, but would suggest that the botanist who does this should lead as well as follow the indigenous nomenclature, so far as to correct absurd or incongruous local names and introduce right or fitting ones as far as practicable. For instance, "Virgin's Bower" is not a proper name for *Wistaria frutescens*, and is rightly applied to

[1] *Geological and Natural History Survey of North Carolina.* Part III. Botany. The Woody Plants of North Carolina. By M. A. Curtis. Raleigh, 1860. (American Journal of Science and Arts, 2 ser., xxx. 275.)

Clematis Virginiana over the leaf. (We venture to add, in passing, *C. Viorna* to the list, having gathered it in Ashe County.) And although the people alongshore call Baccharis by the name of the English annual weed, "Groundsel," it were better to write it "Groundsel-tree." "Yellow-wood" is the name of Cladrastis, rather than of Symplocos, which the Carolinians call "Horse-Sugar." Dr. Curtis can coin a name upon occasion; for surely nobody in Carolina knows *Menziesia globularis* as False Heath, nor has it any scientific claim to this appellation. While in critical mood we may express a strong dissent from the proposition that *Rhododendron punctatum* is too inferior to the other two species "to attract or deserve much attention." With us, it is surpassingly beautiful in cultivation, none the less so because its habit is so different, having light and pendent branches, when well grown forming broad and thick masses, and loaded with its handsome rose-colored blossoms. While *Leucothoë Catesbœi* is called "a very pretty shrub," the far handsomer *Andromeda floribunda*, so much prized by our nurserymen, gets no commendation. *Magnolia Fraseri* may not only be "cultivated in the open air near Philadelphia," but is perfectly hardy near Boston, and the earliest to blossom; but we never noticed the fragrance of the flowers. On the other hand, as it is a native so far south as Florida, it might thrive in plantations anywhere in North Carolina. The flowers of *M. cordata* are described as if larger than those of *M. Fraseri*, instead of the contrary; we could hardly say much for their beauty, except in comparison to those of the common Cucumber-tree. *Prunus Virginiana* is omitted; yet surely it is not wanting in North Carolina. And it is almost an excess of conscientiousness to leave out Cladrastis, the handsomest tree of the country, all things considered, when it is known to grow only a few rods over the Tennessee line.

On the other hand, we are disposed to doubt if the genuine White Spruce (*Abies alba*) occurs in North Carolina. At length we know this tree, but only in Canada and parts adjacent. It is more, instead of less, northern in its range than *A. nigra*. But since President Wheeler has pretty

nearly determined the existence of *A. Fraseri* on the Green Mountains in Vermont, we could not deny that *A. alba* grows with the latter on the high mountains of North Carolina. We make our little criticism freely, — as we know the excellent author would wish, — for we think it likely that this part of the Report will pass to a second edition, — when we hope it will be largely augmented.

BENTHAM'S FLORA OF HONGKONG.

The present work[1] is the third of the series of British Colonial Floras, upon a new and simple plan, compact in form, written in English throughout, authorized and supported by the British Government. The Colonial department pays a very moderate recompense to the authors, and turns the work over to a publisher upon such terms as to render the volume generally accessible to working botanists and colonists. This is a much wiser as well as vastly more economical plan of government patronage to scientific publication than that adopted in this country, one which secures that the publications are just what is wanted and that they reach the hands which are to use them, and not others, — one which, when our present task is done and we again cultivate the arts of peace, we might profitably adopt. The present work is by a master-hand; for Mr. Bentham is one of the most experienced, industrious, and judicious of systematic botanists. The island of Hongkong has an area of scarcely thirty square miles, its general aspect is bleak and barren; yet it has already yielded about a thousand phænogamous species. "At a first glance," as the author observes, "one is struck with the very large total amount of species crowded upon so small an island, which all navigators depict as apparently so bleak and bare;

[1] *Flora Hongkongensis*; a Description of the Flowering Plants and Ferns of the Island of Hongkong, by George Bentham. With a Map of the Island. Published under the Authority of Her Majesty's Secretary of State for the Colonies. London, 1861. (American Journal of Science and Arts, 2 ser., xxxii. 124.)

— with the tropical character of the great majority of species, when botanists agree in representing the general aspect (derived from the majority of individuals) to present the features of a much more northern latitude; — with the large proportion of arborescent and shrubby species, on a rocky mass where the woods are limited to a few ravines, or short narrow valleys half monopolized by cultivation; — and with the very great diversity in the species themselves, the proportion of orders and genera to species, and the comparative number of monotypic genera, being far greater in the Hongkong Flora than in any other Flora of similar extent known to me. The very large number of endemic species — of species known to us only from the island — is probably occasioned by our ignorance, already alluded to, of the vegetation of continental south China."

A fitting acknowledgment is given for the important contribution to this Flora furnished by the botanical collection (of above 500 species) made by Charles Wright, as botanist of the U. S. North Pacific Exploring Expedition under Captains Ringgold and Rodgers, duplicates of which were obligingly and most properly furnished by direction of the Commander and the enlightened Secretary of the Smithsonian Institution.

In aid of the colonial botanists or amateurs who may use this Flora, the author has prefixed (with some minor alterations) the admirable brief outlines of Botany and Glossary prepared for his popular British Flora.

In these Outlines the subject is regarded, not from the morphological or the physiological, but from the descriptive point of view. It opens with a statement of the nature and design of a Flora, and of what a botanical description ought to be.

"These descriptions should be clear, concise, accurate, and characteristic, so that each one should be readily adapted to the plant it relates to, and to no other; they should be as nearly as possible arranged under natural divisions, so as to facilitate the comparison of each plant with those nearest allied to it; and they should be accompanied by an artificial key or index, by means of which the student may be guided

step by step in the observation of such peculiarities, or characters, in his plant as may lead him, with the least delay, to the individual description belonging to it.

"For descriptions to be clear and readily intelligible, they should be expressed as much as possible in ordinary well-established language. But, for the purpose of accuracy, it is necessary not only to give a more precise technical meaning to many terms used more or less vaguely in common conversation, but also to introduce purely technical names for such parts of plants or forms as are of little importance except to the botanist. In the present chapter it is proposed to define such technical or technically limited terms as are made use of in these Floras.

"At the same time mathematical accuracy must not be expected. The forms and appearances assumed by plants and their parts are infinite. Names cannot be invented for all; those even that have been proposed are too numerous for ordinary memories. Many are derived from supposed resemblances to well-known forms and objects. These resemblances are differently appreciated by different persons; and the same term is not only differently applied by two different botanists, but it frequently happens that the same writer is led on different occasions to give somewhat different meanings to the same word. The botanist's endeavors should always be, on the one hand to make as near an approach to precision as circumstances will allow, and on the other hand to avoid that prolixity of detail and overloading with technical terms which tends rather to confusion than to clearness. In this he will be more or less successful. The aptness of a botanical description, like the beauty of a work of imagination, will always vary with the style and genius of the author."

These Outlines are throughout so well sketched, and so worthy to be regarded as of standard authority, that we must still venture a criticism or two, looking to their possible improvement.

In the first place, referring to paragraphs 8 and 88, we must dissent from the proposition that the subject of *homology* does not belong to "*morphology* in the proper sense of

the term;" — unless, indeed, morphology relates simply to form in the lowest sense, to mere shape, arbitrarily viewed, — which would belittle the subject down to mere terminology, and empty that of all scientific interest. If the comparison even of a perfoliate or clasping with a cordate leaf, or of membranaceous or coriaceous with thickened leaves, such as those of a Houseleek, a Mesembryanthemum, and an Aloë, falls within the province of morphology, surely so also must the comparison of an ordinary leaf with a cotyledon, with a bulb-scale, a bud-scale, and no less with a sepal, a petal, a carpel, etc. In the latter we merely trace morphological relations of the very same kind somewhat further and higher. The relation of a leaf as foliage to the scale of a bud, or to the thorn of a Barberry, is clearly of the same category as its relation to a sepal or a petal, — the latter, as we regard it, bringing in no new idea, and requiring no new point of view.

Next, *Quincuncial* imbrication is defined by Mr. Bentham to be that arrangement in which "one petal is outside, an adjoining one wholly inside, the three others intermediate and overlapping on one side." But why give this name to a mixed form, to that which is merely convolute æstivation deranged by one of the five petals getting both edges under? And why change the uniform usage from De Candolle's "Théorie Elémentaire," if not earlier, down to the present time, which defines the quincuncial mode as having two members exterior, two interior, and one with one edge overlapping its neighbor and the other overlapped; an arrangement which especially merits a distinguishing name, since it is the normal imbrication in a pentamerous perianth, answering as it does to two fifths phyllotaxis. So that current usage and reason tell against the innovation.

In the third place, we are equally inclined to demur to the proposed modifications of the sense of the terms *perigynous* and *epigynous* (paragraph 140), Mr. Bentham restricting the former to those cases in which the petals, etc., are adnate to a perfectly free calyx, as in the Cherry, and applying the latter in cases where the calyx, equally bearing the petals, etc., is adnate even merely to the base of the ovary, if only the

adhesion reaches above the level of the insertion of the lowest ovule; which would make most Saxifrages epigynous. Besides the etymological objections, and the inconvenience of a change, the new definitions seem to us to be at least as ambiguous as the old in practice; and it is not surprising that they are not uniformly adopted in the Hongkong Flora itself.

Finally, as to paragraph 166, we are not much better satisfied with the definition that the radicle is the "base of the future root," than with the original statement that it is "the future root." To us nothing in botany is clearer, or more patent to observation during germination, than that while the radicle is, if you please, "the base of the future root" inasmuch as it is that from which the root proceeds, it is itself the first internode of the stem. This view, to which morphological considerations and observation of the development long since brought us, appears to be generally adopted by the French and German botanists, but not by the English. If the radicle universally failed to elongate, as in Monocotyledons, and in the Pea, Oak and others with hypogæous germination, this organ might be deemed to be merely the base of the future root; but its more usual elongation, in the manner of any other internode, plainly reveals the cauline nature which analogy would also assign to it.

The chapter on Vegetable Anatomy and Physiology is new, is very condensed, and considering that it deals with matters to which Mr. Bentham has never specially attended, is remarkably good and accurate. We merely observe in passing, of paragraphs 195, 197, that the distinction between exogenous and endogenous stems is as obvious during the first season, and even at its beginning, as ever afterward, and it is then that the purely systematic botanist will more commonly have occasion to examine the structure in this regard; of § 198[5], that "the liber or inner bark" is by no means always "formed of bast cells;" of § 200, that we cannot accept the statement that "in the leaf the structure of the petioles and principal ribs or veins is the same as that of the young branches of which they are ramifications," at least in any sense in which

the sentence would be understood by the learner. Paragraph 207, that roots grow in length at the extremities, "in proportion as they find the requisite nutriment," might imply the popular fallacy that they grow directly by means of what they take in from the soil, which surely they do not, unless they live in the manner of *Fungi*. To say that the starch, etc. in a tuber or in a seed "appears to be a store of nourishment" for the early growth of the buds or the embryo, is a remarkably over-cautious statement (how could these grow without some store of elaborated matter to feed upon?); nor does the consideration that similar accumulations in the pericarps of many fruits "perish long before germination," and so do not nourish the embryo, afford to us any presumption to the contrary, even if we could not conceive — as we readily can — of other final causes, some of them important to the continuance of the species thereby subserved.

The fourth chapter, on the Collection, Preservation, and Determination of Plants, and upon Aberrations from the ordinary type or appearance, is most excellent.

DR. HOOKER'S DISTRIBUTION OF ARCTIC PLANTS.

The immediate subjects of the treatise[1] are the Arctic plants, of every phænogamous species known to occur spontaneously anywhere within the Arctic circle; the geographical distribution of which, so far as known, is carefully indicated: 1. Within the Arctic region, under the several divisions — Europe, Asia, western America (Behring's Straits to the Mackenzie River), eastern America (Mackenzie River to Baffin's Bay), and arctic Greenland. 2. Without this circle, and under the general divisions of north and central European and north Asiatic Distribution, with three longitudinal subdivisions; American Distribution, with appropriate subdivisions;

[1] *Outlines of the Distribution of Arctic Plants.* By Joseph D. Hooker. Extr. *Transactions Linnæan Society*, of London. Vol. xxiii. pp. 251–348. 1861. (American Journal of Science and Arts, 2 ser., xxxiv. 144.)

south European and African Distribution; central and south Asiatic Distribution. The theory upon which the facts are collocated and discussed, and which they are thought strongly to confirm, is that of Edward Forbes, which was completed, if not indeed originated, by Darwin: — "first, that the existing Scandinavian flora is of great antiquity, and that previous to the glacial epoch it was more uniformly distributed over the Polar Zone than it is now; secondly, that during the advent of the glacial period this Scandinavian vegetation was driven southward in every longitude, and even across the tropics into the south temperate zone; and that, on the succeeding warmth of the present epoch, those species that survived both ascended the mountains of the warmer zones, and also returned northward, accompanied by aborigines of the countries they had invaded during their southern migration. Mr. Darwin shows how aptly such an explanation meets the difficulty of accounting for the restriction of so many American and Asiatic arctic types to their own peculiar longitudinal zones, and for what is a far greater difficulty, the representation of the same arctic genera by closely allied species in different longitudes. . . . Mr. Darwin's hypothesis accounts for many varieties of one plant being found in various alpine and arctic regions of the globe, by the competition into which their common ancestor was brought with the aborigines of the countries it invaded. Different races survived the struggle for life in different longitudes; and these races again, afterwards converging on the zone from which their ancestor started, present there a plexus of closely allied but more or less distinct varieties, or even species, whose geographical limits overlap, and whose members, very probably, occasionally breed together." A further advantage claimed for this hypothesis is, that it explains a fact brought out by Dr. Hooker in a former publication, namely: "that the Scandinavian flora is present in every latitude of the globe, and is the only one that is so."

Moreover, Dr. Hooker discovers in the flora of Greenland a state of things explicable upon this hypothesis, but hardly by any other, namely: its almost complete identity with that of

Lapland; its general paucity, as well as its poverty in peculiar species; the rarity of American species there; the fewness of temperate plants in temperate Greenland; and the presence of a few of the rarest Greenland and Scandinavian species in enormously remote alpine localities of west America and the United States. Our author reasons thus: "If it be granted that the polar area was once occupied by the Scandinavian flora, and that the cold of the glacial epoch did drive this vegetation southwards, it is evident that the Greenland individuals, from being confined to a peninsula, would have been exposed to very different conditions from those of the great continents. In Greenland many species would, as it were, be driven into the sea, that is, exterminated; and the survivors would be confined to the southern portion of the peninsula, and, not being there brought into competition with other types, there could be no struggle for life amongst their progeny, and, consequently, no selection of better adapted varieties. On the return of heat, survivors would simply travel northwards, unaccompanied by the plants of any other country."

The rustic denizens of Greenland, huddled upon the point of the peninsula during the long glacial cold, have never enjoyed the advantages of foreign travel; those of the adjacent continents on either side have "seen the world," and gained much improvement and diversity thereby. Considering the present frigid climate of Greenland, the isotherm of 32° just impinging upon its southern point, its moderate summer and low autumnal temperature, we should rather have supposed the complete extermination of the Greenland ante-glacial flora; and have referred the Scandinavian character of the existing flora (all but eleven of the 207 arctic species, and almost all those of temperate Greenland, being European plants) directly to subsequent immigration from the eastern continent. Several geographical considerations, and the course of the currents, which Dr. Hooker brings to view on p. 270, would go far towards explaining why Greenland should have been re-peopled from the Old rather than from the New World; while the list (on pp. 272, 273) of upwards of 230

Arctic-European species, which are all likewise American plants, but are remarkable for their absence from Greenland, would indicate no small difficulty in the westward migration, and render it most probable that the diffusion of species from the Old World to the New was eastward through Asia, for the arctic no less than (as has elsewhere been shown) for the temperate plants. Was it that Greenland and the adjacent part of the American continent remained glacial longer than the rest of the zone? And if our northern regions were thus colonized by an ancient Scandinavian flora, this seems to have been in return for a still earlier donation of American plants to Europe, to which a very few existing but numerous fossil remains bear testimony. Speculative inquiries of this sort are enticing, and the time is approaching in which they may be fruitful.

Indeed, the characteristic features and the immediate interest and importance of the present memoir, as of others of the same general scope and interest, are found in this: 1. That the actual geographical distribution of species is something to be accounted for; 2. That our existing species, or their originals, are far more ancient than was formerly thought, mainly if not wholly antedating the glacial period; and, 3. That they have therefore been subject to grave climatic vicissitudes and changes. There may be many naturalists who still hesitate to accept these propositions, as there are one or two who deny them; but these or similar conclusions have evidently been reached by those botanists, paleontologists, and geologists in general who have most turned their thoughts to such inquiries, and who march foremost in the advancing movement of these sciences. In this position, the author of the present memoir, — prepossessed with Darwin's theory of the diversification of species through natural selection, — having occasion to revise systematically the materials of the arctic flora, is naturally led to compare the new theory with the facts of the case in this regard; to see how far the vicissitudes to which it is all but demonstrated that the plants of the northern hemisphere have long been subjected, and the modifications and extinctions which he thinks must have

ensued under such grave changes and perils, during such lapse of time, may serve to explain the actual distribution of arctic species and the remarkable dispersion of many of them. That the enquiry is a legitimate and a hopeful one we must all agree, whether we favor Darwinian hypotheses or not. How well it works in the present trial we could not venture to pronounce without a far more critical examination than could now be undertaken. But there are good reasons for the opinion that this is just the ground upon which the elements of the new hypothesis figure to the best advantage.

The mass of facts, so patiently and skillfully collected and digested in this essay, have a high and positive value, irrespective of all theoretical views. We cannot undertake to offer an abstract, but may note here and there a point of interest. The flowering plants which have been collected within the arctic circle number 762, namely, 214 Monocotyledons and 548 Dicotyledons. They occupy a circumpolar belt of 10° to 14° of latitude. The only abrupt change in the vegetation anywhere along this belt is at Baffin's Bay, the opposite shores of which present, as has been already intimated, an almost purely European flora on the east coast, but a large admixture of purely American species on the west.

"Regarded as a whole, the arctic flora is decidedly Scandinavian; for Arctic Scandinavia, or Lapland, though a very small tract of land, contains by far the richest arctic flora, amounting to three fourths of the whole." This would not be very surprising, since this is much the least frigid portion of the zone, and has the highest summer temperature; but "upwards of three-fifths of the species, and almost all the genera of Arctic Asia and America, are likewise Lapponian;" so that the Scandinavian character pervades the whole.

In the section on the local distribution of plants within the arctic circle, Dr. Hooker shows that there is no close relation discoverable between the isothermal lines (whether annual or monthly) and the amount of vegetation, beyond the general fact that the scantiness of the Siberian flora is associated with a

great southern bend in Asia, and its richness in Lapland, with an equally great northern bend there, of the annual isotherm of 32°. Yet "the same isotherm bends northwards in passing from eastern America to Greenland, the vegetation of which is the scantier of the two; and it passed to the northward of Iceland, which is much poorer in species than those parts of Lapland to the southward of which it passes." A glance at the supposed former state of things would suggest the explanation of all that is anomalous here.

"The June isothermals, as indicating the most effective temperatures in the arctic regions (when all vegetation is torpid for nine months, and excessively stimulated during the three others), might have been expected to indicate better the positions of the most luxuriant vegetation. But neither is this the case; for the June isothermal of 41°, which lies within the arctic zone in Asia, where the vegetation is scanty in the extreme, descends to lat. 54° in the meridian of Behring's Straits, where the flora is comparatively luxuriant." The aridity of the former and the humidity of the latter district here offers an obvious explanation; also the great severity of the winter in the former, and its mildness in the latter. And Great Britain, in which a far greater diversity of species are capable of surviving without protection than in the eastern United States under the same annual isotherms, indicates the advantage of a mean over an extreme climate in this respect, if only there be a certain amount of summer heat. For lack of that, doubtless, very many of the introduced denizens of Britain would soon disappear, if deprived of human care.

"The northern limit to which vegetation extends varies in every longitude; the extreme is still unknown; it may, indeed, reach to the pole itself. Phænogamic plants, however, are probably nowhere found far north of lat. 81°. Seventy flowering plants are found in Spitzbergen; and Sabine and Ross collected nine on Walden Island, towards its northern extreme, but none on Ross's Islet, fifteen miles further to the north.

"*Saxifraga oppositifolia* is probably the most ubiquitous, and may be considered the commonest and most arctic flower-

ing plant." There are only eight or nine phænogamous species peculiar to the arctic zone, and only one peculiar genus, namely, the grass Pleuropogon.[1] Of the 762 found south of the circle, all but 150 have advanced beyond lat. 40° N. in some part of the world; about 50 of them are identified as natives of the mountainous regions of the tropics, and 105 as inhabiting the south temperate zone.

"The proportion of species which have migrated southward in the Old and New World also bear a fair relation to the facilities for migration presented by the different continents." The tables given to illustrate this "present in a very striking point of view the fact of the Scandinavian flora being the most widely distributed over the world. The Mediterranean, south African, Malayan, Australian, and all the floras of the New World, have narrow ranges compared with the Scandinavian, and none of them form a prominent feature in any other continent than their own. But the Scandinavian not only girdles the globe in the arctic circle, and dominates over all others in the north temperate zone of the Old World, but intrudes conspicuously into every other temperate flora, whether in the northern or southern hemisphere, or on the Alps of tropical countries. . . . In one respect this migration is most direct in the American meridian, where more arctic species reach the highest southern latitudes. This I have accounted for ('Flora Antarctica,' p. 230) by the continuous chain of the Andes having favored their southern dispersion."

In presenting the actual number of arctic species, and in delineating their geographical ranges, the question, what are to be regarded as species, becomes all-important. As to this, it does not so much matter what scale is adopted, as to know clearly what the adopted scale is. Here we are not left in

[1] *Douglasia* is mentioned in another place (p. 269) as an absolutely peculiar arctic or arctic-alpine genus of eastern America. But we have considered this genus as identical with Gregoria, of Duby. It would appear as if these two genera were established in the same year, since Lindley himself, in the "Botanical Register," refers to Brande's Journal for January, 1828, for his original article. But this article will be found in the volume of that Journal for 1827; so that the name Douglasia is to be adopted, if the genus is sufficiently distinct from Androsace.

doubt. Taking European botanists by number, we are confident that nine out of ten would have enlarged the list of 762 phænogamous arctic species to 800 or more, and would not have recognized a goodly number of the synonyms adduced, thereby considerably affecting the assigned ranges, especially into temperate and austral latitudes. In this regard we should side with Dr. Hooker on the whole, but with differences and with questionings — with halting steps following his bold and free movement, but probably arriving at the same goal at length. Indeed, we freely receive the view which Dr. Hooker presents as appropriate to his particular purpose, and as the most useful expression of our knowledge of the relationships of the plants in question, when collocated in reference to the ideas upon which this memoir is based. That is: "if, with many botanists, we consider these closely allied varieties and species as derived by variation and natural selection from one parent form at a comparatively modern epoch, we may with advantage, for certain purposes, regard the aggregate distribution of such very closely allied species as that of one plant." "An empirical grouping of allied plants, for the purposes of distribution, may thus lead to a practical solution of difficulties in the classification and synonymy of species. My thus grouping names must not be regarded as a committal of myself to the opinion that the plants thus grouped are not to be held as distinct species. . . . My main object is to show the affinities of the polar plants, and I can best do this by keeping the specific idea comprehensive." And further: "I wish it then to be clearly understood, that the catalogue here appended is intended to include every species hitherto found within the arctic circle, together with those most closely allied forms which I believe to have branched off from one common parent within a comparatively recent geological epoch, and that immediately previous to the glacial period or since then" (p. 279). All we could ask more would be some distinction (typographical or other), to mark, 1, undoubted and complete synonyms; 2, mere variations or states, local or otherwise, or undoubted varieties; 3, such as, theory apart, would claim to be regarded as distinct but

closely related species. For example: to take one order, while *Rhinanthus minor* may well be considered as "not a sufficiently constant form to rank as a race even," while *Limosella tenuifolia* could rank for no more than a race, and while *Castilleia septentrionalis* and *C. pallida*, we are now convinced, however distinct in this single character, differ only (and inconstantly) in the relative development of the galea, we think it likely that *Pedicularis lanata*, Willd., does not rightfully merge in *P. hirsuta* this side of the glacial period, although it perhaps may into *P. Langsdorffii*, and that into *P. Sudetica*. But this is no place for criticisms upon the limitation of species, upon which the opinions of botanists will so greatly depend upon the amount of their materials, and upon which the best considered opinions must be subject to frequent revisal. Nor does the value of the present memoir at all depend upon the settlement of such points. To the philosophical naturalist, as to the archæologist, just now the most interesting and pregnant epoch of the world's natural history is that immediately antecedent to the present, that near past from which the present has proceeded, and upon which so much light, from very diverse sources, is now being concentrated: towards its elucidation the memoir we have been considering is a very valuable contribution.

ALPHONSE DE CANDOLLE ON THE VARIATION AND DISTRIBUTION OF SPECIES.

This is the title of a paper [1] by Monsieur De Candolle growing out of his study of the Oaks. It was published in the November number of the "Bibliothèque Universelle," and separately issued as a pamphlet. A less inspiring task could hardly be assigned to a botanist than the systematic elaboration of the genus Quercus and its allies. The vast materials assembled under De Candolle's hands, while disheartening

[1] *Etude sur l'Espèce, à l'occasion d'une Revision de la Famille des Cupulifers.* Par M. Alphonse De Candolle. (American Journal of Science and Arts, 2 ser., xxxv. 431.)

for their bulk, offered small hope of novelty. The subject was both extremely trite and extremely difficult. Happily it occurred to De Candolle that an interest might be imparted to an onerous undertaking, and a work of necessity be turned to good account for science, by studying the Oaks in view of the question of Species.

What this term Species means, or should mean, in natural history, what the limits of species, *inter se* or chronologically, or in geographical distribution, their modifications, actual or probable, their origin, and their destiny, — these are questions which surge up from time to time; and now and then in the progress of science they come to assume a new and hopeful interest. Botany and Zoölogy, Geology, and what our author, feeling the want of a new term, proposes to name *Epiontology*,[1] all lead up to and converge into this class of questions, while recent theories shape and point the discussion. So we look with eager interest to see what light the study of Oaks, by a very careful, experienced, and conservative botanist, particularly conversant with the geographical relation of plants, may throw upon the subject.

The course of investigation in this instance does not differ from that ordinarily pursued by working botanists; nor, indeed, are the theoretical conclusions other than those to which a similar study of other orders might not have equally led. The Oaks afford a very good occasion for the discussion of questions which press upon our attention, and perhaps they offer peculiarly good materials on account of the number of fossil species.

Preconceived notions about species being laid aside, the

[1] A name which, at the close of his article, De Candolle proposes for *the study of the succession of organized beings*, to comprehend, therefore, palæontology and everything included under what is called geographical botany and geographical zoölogy, — the whole forming a science parallel to geology, — the latter devoted to the history of unorganized bodies, the former, to that of organized beings, as respects origin, distribution, and succession. We are not satisfied with the word, notwithstanding the precedent of *palæontology;* since *ontology*, the science of being, has an established meaning as referring to mental existence, — *i. e.*, is a synonym or a department of metaphysics.

specimens in hand were distributed, according to their obvious resemblances, into groups of apparently identical or nearly identical forms, which were severally examined and compared. Where specimens were few, as from countries little explored, the work was easy, but the conclusions, as will be seen, of small value. The fewer the materials, the smaller the likelihood of forms intermediate between any two, and — what does not appear being treated upon the old law-maxim as non-existent — species are readily enough defined. Where, however, specimens abound, as in the case of the Oaks of Europe, of the Orient, and of the United States, of which the specimens amounted to hundreds, collected at different ages, in varied localities, by botanists of all sorts of views and predilections, — here alone were data fit to draw useful conclusions from. Here, as De Candolle remarks, he had every advantage, being furnished with materials more complete than any one person could have procured from his own herborizations, more varied than if he had observed a hundred times over the same forms in the same district, and more impartial than if they had all been amassed by one person with his own ideas or predispositions. So that vast herbaria, into which contributions from every source have flowed for years, furnish the best possible data — at least are far better than any practicable amount of personal herborization — for the comparative study of related forms occurring over wide tracts of territory. But as the materials increase, so do the difficulties. Forms, which appeared totally distinct, approach or blend through intermediate gradations; characters, stable in a limited number of instances or in a limited district, prove unstable occasionally, or when observed over a wider area; and the practical question is forced upon the investigator, — what here is probably fixed and specific, and what is variant, pertaining to individual, variety, or race?

In the examination of these rich materials, certain characters were found to vary upon the same branch, or upon the same tree, sometimes according to age or development, sometimes irrespective of such relations or of any assignable reasons. Such characters, of course, are not specific, although

many of them are such as would have been expected to be constant in the same species, and are such as generally enter into specific definitions. Variations of this sort, De Candolle, with his usual painstaking, classifies and tabulates, and even expresses numerically their frequency in certain species. The results are brought well to view in a systematic enumeration: —

(1) Of characters which frequently vary upon the same branch: over a dozen such are mentioned.

(2) Of those which sometimes vary upon the same branch: a smaller number of these are mentioned.

(3) Those so rare that they might be called monstrosities.

Then he enumerates characters, ten in number, which he has never found to vary on the same branch, and which therefore may better claim to be employed as specific. But, as among them he includes the duration of the leaves, the size of the cupule, and the form and size of its scales, which are by no means wholly uniform in different trees of the same species, even these characters must be taken with allowance. In fact, having first brought together, as groups of the lowest order, those forms which varied upon the same stock, he next had to combine similarly various forms which, though not found associated upon the same branch, were thoroughly blended by intermediate degrees.

"The lower groups (varieties or races) being thus constituted, I have given the rank of *species* to the groups next above these, which differ in other respects, *i. e.*, either in characters which were not found united upon certain individuals, or in those which do not show transitions from one individual to another. For the Oaks of regions sufficiently known, the species thus formed rest upon satisfactory bases, of which the proof can be furnished. It is quite otherwise with those which are represented in our herbaria by single or few specimens. These are *provisional species*, — species which may hereafter fall to the rank of simple varieties. I have not been inclined to prejudge such questions; indeed, in this regard, I am not disposed to follow those authors whose tendency is, as they say, to reunite species. I never reunite them without proof in each partic-

ular case; while the botanists to whom I refer do so on the ground of analogous variations or transitions occurring in the same genus or in the same family. For example, resting on the fact that *Quercus Ilex*, *Q. coccifera*, *Q. acutifolia*, etc., have the leaves sometimes entire and sometimes toothed upon the same branch, or present transitions from one tree to another, I might readily have united my *Q. Tlapuxahuensis* to *Q. Sartorii* of Liebmann, since these two differ only in their entire or their toothed leaves. From the fact that the length of the peduncle varies in *Q. Robur* and many other Oaks, I might have combined *Q. Seemannii*, Liebm., with *Q. salicifolia*, Née. I have not admitted these inductions, but have demanded visible proof in each particular case. Many species are thus left as provisional; but in proceeding thus, the progress of the science will be more regular, and the synonymy less dependent upon the caprice or the theoretical opinions of each author."

This is safe and to a certain degree judicious, no doubt, as respects published species. Once admitted, they may stand until they are put down by evidence, direct or circumstantial. Surely a species may rightfully be condemned on good circumstantial evidence. But what course does De Candolle pursue in the case — of every-day occurrences to most working botanists having to elaborate collections from countries not so well explored as Europe — when the forms in question, or one of the two, are as yet unnamed? Does he introduce as a new species every form which he cannot connect by ocular proof with a near relative, from which it differs only in particulars which he sees are inconstant in better known species of the same group? We suppose not. But if so, little improvement for the future upon the state of things revealed in the following paragraph can be expected.

"In the actual state of our knowledge, after having seen nearly all the original specimens, and in some species as many as 200 representatives from different localities, I estimate that, out of the 300 species of *Cupuliferæ* which will be enumerated in the 'Prodromus,' two thirds at least are *provisional* species. In general, when we consider what a multitude of species were described from a single specimen, or from the forms of a single locality, of a single country, or are badly described, it is difficult to believe that above one third of the actual species in botanical works will remain unchanged."

Such being the results of the want of adequate knowledge, how is it likely to be when our knowledge is largely increased? The judgment of so practised a botanist as De Candolle is important in this regard, and it accords with that of other botanists of equal experience.

"They are mistaken," he pointedly asserts, "who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a feeble minority. This seemed to be true, so long as a genus was imperfectly known, and its species were founded upon few specimens, that is to say, were provisional. Just as we come to know them better, intermediate forms flow in, and doubts as to specific limits augment."

De Candolle insists, indeed, in this connection, that the higher the rank of the groups, the more definite their limitation, or, in other terms, the fewer the ambiguous or doubtful forms; that genera are more strictly limited than species, tribes than genera, orders than tribes, etc. We are not convinced of this. Often where it has appeared to be so, advancing discovery has brought intermediate forms to light, perplexing to the systematist. "They are mistaken," we think more than one systematic botanist will say, "who repeat that the greater part of our natural orders and tribes are absolutely limited," however we may agree that we will limit them. Provisional genera we suppose are proportionally hardly less common than provisional species; and hundreds of genera are kept up on considerations of general propriety or general convenience, although well known to shade off into adjacent ones by complete gradations. Somewhat of this greater fixity of higher groups, therefore, is rather apparent than real. On the other hand, that varieties should be less definite than species, follows from the very terms employed. They are ranked as varieties, rather than species, just because of their less definiteness.

Singular as it may appear, we have heard it denied that spontaneous varieties occur. De Candolle makes the important announcement that, in the Oak genus, the best known species are just those which present the greatest number of

spontaneous varieties and sub-varieties. The maximum is found in *Q. Robur*, with twenty-eight varieties, all spontaneous. Of *Q. Lusitanica* eleven varieties are enumerated, of *Q. Calliprinos* ten, of *Q. coccifera* eight, etc. And he significantly adds that "these very species which offer such numerous modifications are themselves ordinarily surrounded by other forms, provisionally called species, because of the absence of known transitions or variations, but to which some of these will probably have to be joined hereafter." The inference is natural, if not inevitable, that the difference between such species and such varieties is only one of degree, either as to amount of divergence, or of hereditary fixity, or as to the frequency or rarity, at the present time, of intermediate forms.

This brings us to the second section of De Candolle's article, in which he passes on, from the observation of the present forms and affinities of Cupuliferous plants, to the consideration of their probable history and origin. Suffice it to say, that he frankly accepts the inferences derived from the whole course of observation, and even contemplates with satisfaction a probable historical connection between congeneric species. He accepts and, by various considerations drawn from the geographical distribution of European *Cupuliferæ*, fortifies the conclusion — long ago arrived at by Edward Forbes — that the present species, and even some of their varieties, date back to about the close of the Tertiary epoch, since which time they have been subject to frequent and great changes of habitation or limitation, but without appreciable change of specific form or character; that is, without profounder changes than those within which a species at the present time is known to vary. Moreover, he is careful to state that he is far from concluding that the time of the appearance of a species in Europe at all indicates the time of its origin. Looking back still further into the Tertiary epoch, of which the vegetable remains indicate many analogous, but few, if any, identical forms, he concludes, with Heer and others, that specific changes of form, as well as changes of station, are to be presumed. And finally, that "the theory

of a succession of forms through the deviation of anterior forms is the most natural hypothesis, and the most accordant with the known facts in palæontology, geographical botany and zoölogy, of anatomical structure and classification: but direct proof of it is wanting, and moreover, if true, it must have taken place very slowly; so slowly indeed, that its effects are discernible only after a lapse of time far longer than our historic epoch."

In contemplating the present state of the species of *Cupuliferæ* in Europe, De Candolle comes to the conclusion that, while the Beech is increasing, and extending its limits southward and westward (at the expense of *Coniferæ* and Birches), the Common Oak, to some extent, and the Turkey Oak decidedly, are diminishing and retreating, and this wholly irrespective of man's agency. This is inferred of the Turkey Oak from the great gaps found in its present geographical area, which are otherwise inexplicable, and which he regards as plain indications of a partial extinction. Community of descent of all the individuals of species is of course implied in these and all similar reasonings.

An obvious result of such partial extinction is clearly enough brought to view. The European Oaks (like the American species) greatly tend to vary, — that is, they manifest an active disposition to produce new forms. Every form tends to become hereditary, and so to pass from the state of mere variation to that of race; and of these competing incipient races some only will survive. *Quercus Robur* offers a familiar illustration of the manner in which one form may in the course of time become separated into two or more distinct ones.

To Linnæus this Common Oak of Europe was all of one species. But of late years the greater number of European botanists have regarded it as including three species, *Q. pedunculata*, *Q. sessiliflora*, and *Q. pubescens*. De Candolle looks with satisfaction to the independent conclusion which he reached from a long and patient study of the forms (and which Webb, Gay, Bentham and others had equally reached), that the view of Linnæus was correct, inasmuch as it goes to

show that the idea and the practical application of the term species have remained unchanged during the century which has elapsed since the publication of the "Species Plantarum." But the idea remaining unchanged, the facts might appear under a different aspect, and the conclusion be different, under a slight and very supposable change of circumstances. Of the twenty-eight spontaneous varieties of *Q. Robur*, which De Candolle recognizes, all but six, he remarks, fall naturally under the three sub-species, *pedunculata*, *sessiliflora*, and *pubescens*, and are therefore forms grouped around these as centres; and, moreover, the few connecting forms are by no means the most common. Were these to die out, it is clear that the three forms which have already been so frequently taken for species, would be what the group of four or five provisionally admitted species which closely surround *Q. Robur* (see p. 435) now are. The best example of such a case, as having in all probability occurred, through geographical segregation and partial extinction, is that of the Cedar, thus separated into the Deodar, the Lebanon, and the Atlantic Cedars, — a case admirably worked out by Dr. Hooker two or three years ago.[1]

A special advantage of the *Cupuliferæ* for determining the probable antiquity of existing species in Europe, De Candolle finds in the size and character of their fruits. However it may be with other plants (and he comes to the conclusion generally that marine currents and all other means of distant transport have played only a very small part in the actual dispersion of species), the transport of acorns and chestnuts by natural causes across an arm of the sea in a condition to germinate, and much more the spontaneous establishment of a forest of Oaks or Chestnuts in this way, De Candolle conceives to be fairly impossible in itself, and contrary to all experience. From such considerations, *i. e.*, from the actual dispersion of the existing species, with occasional aid from Post-tertiary deposits, it is thought to be shown that the principal *Cupuliferæ* of the Old World attained their actual extension before the present separation of Sicily, Sardinia and Corsica, or of Britain, from the European continent.

[1] Natural History Review, Jan., 1862.

This view once adopted, and this course once entered upon, has to be pursued farther. *Quercus Robur* of Europe with its bevy of admitted derivatives, and its attending species only provisionally admitted to that rank, is very closely related to certain species of eastern Asia, and of Oregon and California, — so closely that "a view of the specimens by no means forbids the idea that they have all originated from *Q. Robur*, or have originated, with the latter, from one or more preceding forms so like the present ones that a naturalist could hardly know whether to call them species or varieties." Moreover, there are fossil leaves from diluvian deposits in Italy, figured by Gaudin, which are hardly distinguishable from those of *Q. Robur* on the one hand, and from those of *Q. Douglasii*, etc., of California on the other. No such leaves are found in any Tertiary deposit in Europe; but such are found of that age, it appears, in northwest America, where their remote descendants still flourish. So that the probable genealogy of *Q. Robur*, traceable in Europe up to the commencement of the present epoch, looks eastward and far into the past on far distant shores.

Q. Ilex, the Evergreen Oak of southern Europe and northern Africa, reveals a similar archæology; but its presence in Algeria leads De Candolle to regard it as a much more ancient denizen of Europe than *Q. Robur;* and a Tertiary Oak, *Q. ilicoides*, from a very old Miocene bed in Switzerland, is thought to be one of its ancestral forms. This high antiquity once established, it follows almost of course that the very nearly related species in central Asia, in Japan, in California, and even our own Live Oak with its Mexican relatives, may probably enough be regarded as early offshoots from the same stock with *Q. Ilex*.

In brief, — not to continue these abstracts and remarks, and without reference to Darwin's particular theory (which De Candolle at the close very fairly considers), — if existing species, or many of them, are as ancient as they are now generally thought to be, and were subject to the physical and geographical changes (among them the coming and the going of the Glacial epoch) which this antiquity implies; if in

former times they were as liable to variation as they now are; and if the individuals of the same species may claim a common local origin, — then we cannot wonder that "the theory of a succession of forms by deviations of anterior forms" should be regarded as "the most natural hypothesis," nor at the general advance made towards its acceptance in some form or other.

The question being, not, how plants and animals originated, but, how came the existing animals and plants to be just where they are and what they are; it is plain that naturalists interested in such inquiries are mostly looking for the answer in one direction. The general drift of opinion, or at least of expectation, is exemplified by this essay of De Candolle; and the set and force of the current are seen by noticing how it carries along naturalists of widely different views and prepossessions, — some faster and farther than others, — but all in one way. The tendency is, we may say, to extend the law of continuity, or something analogous to it, from inorganic to organic nature, and in the latter to connect the present with the past in some sort of material connection. The generalization may indeed be expressed so as not to assert that the connection is genetic, as in Mr. Wallace's formula: "Every species has come into existence coincident both in time and space with prec̈isting closely allied species." Edward Forbes, who may be called the originator of this whole line of inquiry, long ago expressed a similar view. But the only material sequence we know, or can clearly conceive, in plants and animals, is that from parent to progeny; and, as De Candolle implies, the origin of species and that of races can hardly be much unlike, nor governed by other than the same laws, whatever these may be.

The progress of opinion upon this subject in one generation is not badly represented by that of De Candolle himself, who is by no means prone to adopt new views without much consideration. In an elementary treatise published in the year 1835, he adopted, and, if we rightly remember, vigorously maintained, Schouw's idea of the double or multiple origin of species, at least of some species, — a view which has been

carried out to its ultimate development only perhaps by Agassiz, in the denial of any necessary genetic connection among the individuals of the same species, or of any original localization more restricted than the area now occupied by the species. But in 1855, in his "Géographie Botanique," the multiple hypothesis, although in principle not abandoned, is seen to lose its point, in view of the probable high antiquity of existing species. The actual vegetation of the world being now regarded as a continuation, through numerous geological, geographical, and more recently historical, changes, of anterior vegetations, the actual distribution of plants is seen to be a consequence of preceding conditions and geological considerations, and these alone may be expected to explain all the facts, many of them so curious and extraordinary, of the actual geographical distribution of the species. In the present essay, not only the distribution but the origin of congeneric species is regarded as something derivative; whether derived by slow and very gradual changes in the course of ages, according to Darwin, or by a sudden, inexplicable change of their Tertiary ancestors, as conceived by Heer, De Candolle hazards no opinion. It may, however, be inferred that he looks upon "natural selection" (which he rather underrates) as a real, but insufficient, cause; while some curious remarks (pp. 57, 58), upon the number of monstrosities annually produced, and the possibility of their enduring, may be regarded as favorable to Heer's view.

As an index to the progress of opinion in the direction referred to, it will be interesting to compare Sir Charles Lyell's well-known chapters of twenty or thirty years ago, in which the permanence of species was ably maintained, with his treatment of the same subject in a work just issued in England, which, however, has not yet reached us.

A belief in the derivation of species may be maintained along with a conviction of great persistence of specific characters. This is the idea of the excellent Swiss vegetable palæontologist Heer, who imagines a sudden change of specific type at certain periods, and perhaps is that of Pictet. Falconer adheres to somewhat similar views in his elaborate

paper on Elephants, living and fossil, in the "Natural History Review" for January last. Noting that "there is clear evidence of the true Mammoth having existed in America long after the period of the northern drift, when the surface of the country had settled down into its present form," and also in Europe so late as to have been a contemporary of the Irish Elk, and on the other hand that it existed in England so far back as before the deposition of the boulder Clay; also that four well-defined species of fossil Elephant are known to have existed in Europe; that "a vast number of the remains of three of these species have been exhumed over a large area in Europe; and, even in the geological sense, an enormous interval of time has elapsed between the formation of the most ancient and the most recent of these deposits, quite sufficient to test the persistence of specific characters in an Elephant," he presents the question: "Do then the successive Elephants occurring in these strata show any signs of a passage from the older form into the newer?"

To which the reply is: "If there is one fact which is impressed on the conviction of the observer with more force than any other, it is the persistence and uniformity of the characters of the molar teeth in the earliest known Mammoth and his most modern successor. . . . Assuming the observation to be correct, what strong proof does it not afford of the persistence and constancy, throughout vast intervals of time, of the distinctive characters of those organs which are most concerned in the existence and habits of the species? If we cast a glance back on the long vista of physical changes which our planet has undergone since the Neozoic Epoch, we can nowhere detect signs of a revolution more sudden and pronounced, or more important in its results, than the intercalation and sudden disappearance of the glacial period. Yet the 'dicyclotherian' Mammoth lived before it, and passed through the ordeal of all the hard extremities it involved, bearing his organs of locomotion and digestion all but unchanged. Taking the group of four European fossil species above enumerated, do they show any signs in the successive deposits of a transition from the one form into the other? Here again

the result of my observation, in so far as it has extended over the European area, is, that the specific characters of the molars are constant in each, within a moderate range of variation, and that we nowhere meet with intermediate forms." . . . Dr. Falconer continues (p. 80) : —

"The inferences which I draw from these facts are not opposed to one of the leading propositions of Darwin's theory. With him, I have no faith in the opinion that the Mammoth and other extinct Elephants made their appearance suddenly, after the type in which their fossil remains are presented to us. The most rational view seems to be, that they are in some shape the modified descendants of earlier progenitors. But if the asserted facts be correct, they seem clearly to indicate that the older elephants of Europe, such as *E. meridionalis* and *E. antiquus*, were not the stocks from which the later species, *E. primigenius* and *E. Africanus*, sprung, and that we must look elsewhere for their origin. The nearest affinity, and that a very close one, of the European *E. meridionalis* is with the Miocene *E. planifrons* of India; and of *E. primigenius*, with the existing India species.

"Another reflexion is equally strong in my mind, — that the means which have been adduced to explain the origin of the species by 'Natural Selection,' or a process of variation from external influences, are inadequate to account for the phenomena. The law of phyllotaxis, which governs the evolution of leaves around the axis of a plant, is as nearly constant in its manifestation as any of the physical laws connected with the material world. Each instance, however different from another, can be shown to be a term of some series of continued fractions. When this is coupled with the geometrical law governing the evolution of form, so manifest in some departments of the animal kingdom, *e. g.*, the spiral shells of the Mollusca, it is difficult to believe that there is not, in nature, a deeper-seated and innate principle, to the operation of which Natural Selection is merely an adjunct. The whole range of the Mammalia, fossil and recent, cannot furnish a species which has had a wider geographical distribution, and passed through a longer term of time, and through more extreme changes of climatal conditions, than the Mammoth. If species are so unstable, and so susceptible of mutation through such influences, why does that extinct form stand out so signally a monument of stability? By his admirable researches and earnest writings, Darwin has, beyond all his cotemporaries, given

an impulse to the philosophical investigation of the most backward and obscure branch of the biological sciences of his day; he has laid the foundations of a great edifice; but he need not be surprised, if, in the progress of erection, the superstructure is altered by his successors, like the Duomo of Milan from the Roman to a different style of architecture."

Entertaining ourselves the opinion that something more than natural selection is requisite to account for the orderly production and succession of species, we offer two incidental remarks upon the above extract.

First, we find in it — in the phrase "Natural Selection, or a process of variation from external influences" — an example of the very common confusion of two distinct things, namely, variation and natural selection. The former has never yet been shown to have its cause in "external influences," nor to occur at random. As we have elsewhere insisted, if not inexplicable, it has never been explained; all we can yet say is, that plants and animals are prone to vary, and that some conditions favor variation. Perhaps in this Dr. Falconer may yet find what he seeks: for "it is difficult to believe that there is not in [its] nature, a deeper-seated and innate principle, to the operation of which Natural Selection is merely an adjunct." The latter, which is the *ensemble* of the external influences, including the competition of the individuals themselves, picks out certain variations as they arise, but in no proper sense can be said to originate them.

Secondly, although we are not quite sure how Dr. Falconer intends to apply the law of phyllotaxis to illustrate his idea, we fancy that a pertinent illustration may be drawn from it, in this way. There are two species of phyllotaxis, perfectly distinct, and, we suppose, not mathematically reducible the one to the other, — namely, 1, that of alternate leaves, with its varieties; and 2, that of verticillate leaves, of which opposite leaves present the simplest case. That, although generally constant, a change from one variety of alternate phyllotaxis to another should occur on the same axis, or on successive axes, is not surprising, the different sorts being terms of a regular series, — although indeed we have not the

least idea as to how the change from the one to the other comes to pass. But it is interesting, and in this connection perhaps instructive, to remark that, while some dicotyledonous plants hold to the verticillate, *i. e.*, opposite-leaved phyllotaxis throughout, a larger number — through the operation of some deep-seated and innate principle, which we cannot fathom — change abruptly into the other species at the second or third node, and change back again in the flower, or else effect a synthesis of the two species in a manner which is puzzling to understand. Here is a change from one fixed law to another, as unaccountable, if not as great, as from one specific form to another.

An elaborate paper on the vegetation of the Tertiary period in the southeast of France, by Count Gaston de Saporta, published in "Annales des Sciences Naturelles," xvi. pp. 309–344, — which we have not space to analyze, — is worthy of attention from the general inquirer, on account of its analysis of the Tertiary flora into its separate types, Cretaceous, Austral, Tropical, and Boreal, each of which has its separate and different history, — and for the announcement that "the *hiatus*, which, in the idea of most geologists, intervened between the close of the Cretaceous and the beginning of the Tertiary, appears to have had no existence, so far as concerns the vegetation ; that in general it was not by means of a total overthrow, followed by a complete new emission of species, that the flora has been renewed at each successive period ; and that while the plants of southern Europe inherited from the Cretaceous period more or less rapidly disappeared, as also the austral forms, and later the tropical types (except the Laurel, the Myrtle, and the *Chamærops humilis*), the boreal types, coming later, survived all the others, and now compose, either in Europe, or in the north of Asia, or in North America, the basis of the actual arborescent vegetation." Especially "a very considerable number of forms nearly identical with Tertiary forms now exist in America, where they have found, more easily than in our [European] soil, — less vast and less extended southward, — refuge from ulterior revolutions." The extinction of species is attributed to two kinds of causes : the

one material or physical, whether slow or rapid; the other inherent in the nature of organic beings, incessant, but slow, in a manner latent, but somehow assigning to the species, as to the individuals, a limited period of existence, and, in some equally mysterious but wholly natural way, connected with the development of organic types: — "By type meaning a collection of vegetable forms constructed upon the same plan of organization, of which they reproduce the essential lineaments with certain secondary modifications, and which appear to run back to a common point of departure."

In this community of types, no less than in the community of certain existing species, Saporta recognizes a prolonged material union between North America and Europe in former times. Most naturalists and geologists reason in the same way, — some more cautiously than others, — yet perhaps most of them seem not to perceive how far such inferences imply the doctrine of the common origin of related species.

For obvious reasons such doctrines are likely to find more favor with botanists than with zoölogists. But with both the advance in this direction is seen to have been rapid and great; yet to us not unexpected. We note, also, an evident disposition, notwithstanding some endeavors to the contrary, to allow derivative hypotheses to stand or fall upon their own merits, — to have indeed upon philosophical grounds certain presumptions in their favor, — and to be, perhaps, quite as capable of being turned to good account as to bad account in natural theology.[1]

[1] What the Rev. Principal Tulloch remarks in respect to the philosophy of miracles has a pertinent application here. We quote at second hand:

"The stoutest advocates of interference can mean nothing more than that the Supreme Will has so moved the hidden springs of nature that a new issue arises on given circumstances. The ordinary issue is supplanted by a higher issue. The essential facts before us are a certain set of phenomena, and a Higher Will moving them. How moving them? is a question for human definition; the answer to which does not and cannot affect the divine meaning of the change. Yet when we reflect that this Higher Will is everywhere reason and wisdom, it seems a juster as well as a more comprehensive view to regard it as operating by subordination and evolution, rather than by interference or violation."

Among the leading naturalists, indeed, such views — taken in the widest sense — have one and, so far as we are now aware, only one thorough-going and thoroughly consistent opponent, namely, Mr. Agassiz.

Most naturalists take into their very conception of a species, explicitly or by implication, the notion of a material connection resulting from the descent of the individuals composing it from a common stock, of local origin. Mr. Agassiz wholly eliminates community of descent from his idea of species, and even conceives a species to have been as numerous in individuals, and as widespread over space, or as segregated in discontinuous spaces, from the first as at a later period.

The station which it inhabits, therefore, is with other naturalists in nowise essential to the species, and may not have been the region of its origin. In Mr. Agassiz's view the habitat is supposed to mark the origin, and to be a part of the character of the species. The habitat is not merely the place where it is, but a part of what it is.

Most naturalists recognize varieties of species; and many, like De Candolle, have come to conclude that varieties of the highest grade, or races, so far partake of the characteristics of species, and are so far governed by the same laws, that it is often very difficult to draw a clear and certain distinction between the two. Mr. Agassiz will not allow that varieties or races exist in nature, apart from man's agency.

Most naturalists believe that the origin of species is supernatural, their dispersion or particular geographical area, natural, and their extinction, when they disappear, also the result of physical causes. In the view of Mr. Agassiz, if rightly understood, all three are equally independent of physical cause and effect, are equally supernatural.

In comparing preceding periods with the present and with each other, most naturalists and palæontologists now appear to recognize a certain number of species as having survived from one epoch to the next, or even through more than one formation, especially from the Tertiary into the Post-tertiary period, and from that to the present age. Mr. Agassiz is understood to believe in total extinctions and total new crea-

tions at each successive epoch, and even to recognize no existing species as ever contemporary with extinct ones, except in the case of recent exterminations.

These peculiar views, if sustained, will effectually dispose of every form of derivative hypothesis.

Returning for a moment to De Candolle's article, we are disposed to notice his criticism of Linnæus's "definition" of the term species ("Philosophia Botanica," No. 157): "Species tot numeramus quot diversæ formæ in principio sunt creatæ," — which he declares illogical, inapplicable, and the worst that has been propounded. "So, to determine if a form is specific, it is necessary to go back to its origin, which is impossible. A definition by a character which can never be verified is no definition at all."

Now, as Linnæus practically applied the idea of species with a sagacity which has never been surpassed, and rarely equalled, and indeed may be said to have fixed its received meaning in natural history, it may well be inferred that in the phrase above cited he did not so much undertake to frame a logical definition, as to set forth the idea which, in his opinion, lay at the foundation of species. On which basis A. L. Jussieu did construct a logical definition: "nunc rectius definitur perennis individuorum similium successio continuata generatione renascentium." The fundamental idea of species, we would still maintain, is that of a chain, of which genetically-connected individuals are the links. That, in the practical recognition of species, the essential characteristic has to be inferred, is no great objection, — the general fact that like engenders like being an induction from a vast number of instances, and the only assumption being that of the uniformity of nature. The idea of gravitation, that of the atomic constitution of matter, and the like, equally have to be verified inferentially. If we still hold to the idea of Linnæus, and of Agassiz, that existing species were created independently, and essentially all at once at the beginning of the present era, we could not better the propositions of Linnæus and of Jussieu. If, on the other hand, the time has come in which we may accept, with De Candolle, their successive origination, at the com-

mencement of the present era or before, and even by derivation from other forms, then the "in principio" of Linnæus will refer to that time, whenever it was, and his proposition be as sound and wise as ever.

In his "Géographie Botanique" (ii. pp. 1068–1077) De Candolle discusses this subject at length, and in the same interest. Remarking that of the two great facts of species, namely, likeness among the individuals, and genealogical connection, zoölogists have generally preferred the latter,[1] while botanists have been divided in opinion, he pronounces for the former as the essential thing, in the following argumentative statement: —

"Quant à moi, j'ai été conduit, dans ma définition de l'espèce, à mettre décidément la ressemblance au-dessus des caractères de succession. Ce n'est pas seulement à cause des circonstances propres au règne végétal, dont je m'occupe exclusivement; ce n'est pas non plus afin de sortir ma définition des théories et de la rendre le plus possible utile aux naturalistes descripteurs et nomenclateurs, c'est aussi par un motif philosophique. En toute chose il faut aller au fond des questions, quand on le peut. Or, pourquoi la reproduction est-elle possible, habituelle, féconde indéfiniment, entre des êtres organisés que nous dirons de la même espèce? Parce qu'ils se ressemblent et uniquement à cause de cela. Lorsque deux espèces ne peuvent, ou, s'il s'agit d'animaux supérieurs, ne peuvent et ne veulent se croiser, c'est qu'elles sont très differentes. Si l'on obtient des croisements, c'est que les individus sont analogues; si ces croisements donnent des produits féconds, c'est que les individus êtaient plus analogues; si ces produits eux-mêmes sont féconds, c'est que la ressemblance était plus grande; s'ils sont fécond habituellement et indéfiniment, c'est que la ressemblance intérieure et extérieure était très grande. Ainsi le degré de ressemblance est le fond; la reproduction en est seulement la manifestation et la mesure, et il est logique de placer la cause au-dessus de l'effet."

We are not at all convinced. We still hold that genealogical connection, rather than mutual resemblance, is the fundamental thing, — first on the ground of fact, and then from the

[1] Particularly citing Flourens: "La ressemblance n'est qu'une condition secondaire; la condition essentielle est la descendance: ce n'est pas la ressemblance, c'est la succession des individus, qui fait l'espèce."

philosophy of the case. Practically, no botanist can say what amount of dissimilarity is compatible with unity of species; in wild plants it is sometimes very great, in cultivated races, often enormous. De Candolle himself informs us that the different variations which the same Oak tree exhibits are significant indications of a disposition to set up separate varieties, which, becoming hereditary, may constitute a race; he evidently looks upon the extreme forms, say of *Quercus Robur*, as having thus originated; and on this ground, inferred from transitional forms, and not from their mutual resemblance, as we suppose, he includes them in that species. This will be more apparent should the discovery of the transitions, which he leads us to expect, hereafter cause the four provisional species which attend *Q. Robur* to be merged in that species. It may rightly be replied that this conclusion would be arrived at from the likeness step by step in the series of forms; but the cause of the likeness here is obvious. And this brings in our "motif philosophique."

Not to insist that the likeness is after all the variable, not the constant, element, — to learn which is the essential thing, resemblance among the individuals or their genetic connection, we have only to ask which can be the cause of the other.

In hermaphrodite plants (the normal case), and even as the question is ingeniously put by De Candolle in the above extract, the former surely cannot be the cause of the latter, though it may, in case of crossing, offer occasion. But, on the ground of the most fundamental of all things in the constitution of plants and animals, "the fact incapable of farther analysis, that individuals reproduce their like, that characteristics are inheritable," the likeness is a direct natural consequence of the genetic succession, — and it is logical to place the cause above the effect.

We are equally disposed to combat a proposition of De Candolle's about genera, elaborately argued in the "Géographie Botanique," and incidentally re-affirmed in his present article, namely, that genera are more natural than species, and are more correctly distinguished by people in general, as is shown by vernacular names. But we have no space left in which to present some evidence to the contrary.

Here we must abruptly close our long exposition of a paper which, from the scientific position, ability, and impartiality of its author, is likely at this time to produce a marked impression. We would also direct attention to an earlier article in the same important periodical (namely, in the "Bibliothèque Universelle" for May, 1862), on the European Flora and the Configuration of Continents in the Tertiary Epoch, a most interesting abstract of, and commentary on, the introductory part of Heer's "Flora Tertiaria Helvetiæ," as reëdited and translated into the French by Gaudin, with additions by the author.

DR. HOOKER ON WELWITSCHIA.

This is a separate issue, in folio form, of a memoir[1] in the current (24) volume of the Transactions of the Linnæan Society of London, illustrated by fourteen superb and elaborate plates, the expense of which has been mainly defrayed by the Royal Society, from a parliamentary fund placed at its disposal for the promotion of scientific research. By the cooperation of these two learned societies, the fruits of Dr. Hooker's admirable researches are given to the scientific world in a form and manner worthy of them and of the wonderful subject.

A good idea of the vegetable wonder in question is given in the following brief account of its appearance and prominent characters, drawn partly from the descriptions of its discoverer, and partly from specimens sent to England: —

"The Welwitschia is a woody plant, said to attain a century in duration, with an obconic trunk about two feet long, of which a few inches rise above the soil, presenting the appearance of a flat, two-lobed depressed mass, sometimes (according to Dr. Welwitsch) attaining fourteen feet in circumference (!), and looking like a round table. When full grown it is dark brown, hard, and cracked over the whole surface (much like

[1] *On the Welwitschia*, a new genus of *Gnetaceæ*. By Joseph Dalton Hooker. (American Journal of Science and Arts, 2 ser., xxxvi. 434.)

the burnt crust of a loaf of bread) ; the lower portion forms a stout tap-root, buried in the soil, and branching downward at the end. From deep grooves in the circumference of the depressed mass two enormous leaves are given off, each six feet long when full grown, one corresponding to each lobe; these are quite flat, linear, very leathery, and are split to the base into innumerable thongs that lie curling upon the surface of the soil. Its discoverer describes these same two leaves as being present from the earliest condition of the plant, and assures me that they are in fact developed from the two cotyledons of the seed, and are persistent, being replaced by no others. From the circumference of the tabular mass, above, but close to the insertion of the leaves, spring stout, dichotomously branched cymes, nearly a foot high, bearing small erect scarlet cones, which eventually become oblong, and attain the size of those of the common Spruce Fir. The scales of the cones are very closely imbricated, and contain, when young and still very small, solitary flowers, which in some cones are hermaphrodite (structurally but not functionally), in others female. The hermaphrodite flower consists of a perianth of four pieces, six monadelphous stamens, with globose trilocular anthers, surrounding a central ovule, the integument of which is produced into a styliform sigmoid tube, terminated by a discoid apex. The female flower consists of a solitary erect ovule, contained in a compressed utricular perianth. The mature cone is tetragonous, and contains a broadly winged fruit in each scale. Its discoverer observes that the whole plant exudes a resin, and that it is called *Tumbo* by the natives, — whence he suggests that it may bear the generic name of *Tumboa;* but this he withdrew at my suggestion, for reasons which I shall presently give. It inhabits the elevated sandy plateau near Cape Negro (lat. 15° 40′ S.) on the southwest coast of Africa."

Welwitschia mirabilis, Hook. fil., was also detected and made known — indeed the first actual materials, with a drawing of the plant, were sent to England — by Mr. Baines from the Damara country, in lat. 24° or 24° S., about 500 miles south of Cape Negro. Mr. Baines is an artist, and his original

colored sketch of a plant in fruit is reproduced on the first plate of the memoir. It appears as if five-leaved; but probably one of the two original leaves is split in two, and the other into three segments. As might be inferred from the form and structure, the Welwitschia inhabits a dry region. Mr. Monteiro writes to Dr. Hooker: —

" . . . About thirty miles distant from the coast, I passed a plain about three miles across, on which this plant was growing abundantly; that is to say, I saw about thirty specimens on my line of march. The plain was perfectly dry, and bare of other vegetation than the Welwitschia and a little short grass. The ground was a hard quartzose schist. The Welwitschia was generally growing near the little ruts worn in the plain by running water during the rainy season."

And from Damara Land, Mr. Anderson writes that, —

" Rain rarely or never falls where this plant exists. (Yet the night dews are heavy, as other authorities mention.) I have crossed and recrossed Damara Land throughout its entire length and breadth, but only found the plant growing on that desperately arid flat, stretching far and wide about Walvisch Bay."

We are familiar with plants of very diverse orders of Dicotyledons and Monocotyledons which are adapted to arid regions by great restriction of surface. Here a similar plan is adopted by a Gymnosperm; for the resemblance to *Coniferæ* and *Casuarineæ* indicated by Dr. Welwitsch is shown by Dr. Hooker to import a close affinity, the author referring the plant to *Gnetaceæ* near to Ephedra. Its permanently abbreviated ascending axis — of which the greater part consists of the first internode, below the cotyledons — increases in thickness but hardly in length, develops no other than the seminal pair of leaves, above which the disciform bilobate axile portion or " crown," gradually produced, bears year by year only leafless inflorescence.

Hæmanthus equally bears a pair of leaves; but these die off as the season of drought advances, when the plant is reduced to a minimum of surface in its spherical bulb, — which outspreads a new pair of leaves when the rainy season

returns. But in Welwitschia the two leaves are permanent. Wherefore they are firm and coriaceous, and, increasing by basal growth from year to year, the older parts doubtless become inactive at length, while fresh surface below is annually renewed, under the shelter (as Dr. Hooker describes) afforded by the deep grooves which in old plants separate the growth of the hypocotyledonary stock from that of the crown above, and is filled by the tender growing bases of the leaves.

Having given a detailed generic character of Welwitschia and a comparative view of the Gnetaceous genera, now three in number, Dr. Hooker proceeds to describe at length the trunk, leaves, inflorescence, flowers, fertilization, embryogeny, and seeds of this curious subject, — comparing it, in the latter respects, with *Coniferæ* and *Cycadaceæ* on the one hand, and with Santalum and Loranthus on the other, and closing with a general summary of the results.

An abstract or analysis of this most important paper is beyond our present reach and space. But we may refer to some of the special points.

The most obvious peculiarity of Welwitschia is, that "it appears to be the only perennial flowering plant which at no period has other vegetative organs than those proper to the embryo itself, — the main axis being represented by the radicle, which becomes a gigantic caulicle, and develops a root from its base and inflorescences from its plumulary end, and the leaves being the two cotyledons in a very highly developed and specialized condition." It is an excellent case, accordingly, if any such were still needed for showing the nature of the radicle as stem, or ascending axis (not root), — a view which we supposed observation had long ago demonstrated. Dr. Hooker, in a note, refers to this view as expressed by Adr. de Jussieu in his "Cours Elémentaire" (which appeared in 1843 and 1844), and in Gray's Introduction to Botany ("Botanical Text Book"), 1858. But the same view is taken in all the earlier editions of the latter work; even in the first (1842) the radicle is spoken of as the first internode of the stem (p. 28, note); and probably the idea will be found distinctly expressed in works of an earlier date. Dr. Hooker,

in the note referred to, assents to the proposition that "the radicle is rightly regarded as an axis," *i. e.*, an ascending axis, "and not a root," but does not agree that it is an internode. To us, the one implies the other. Conceiving, as we do, the fundamental idea of the morphology of the phænogamous plant to be, that the ascending axis consists of a series of superposed internodes, each crowned by a leaf-bearing point or ring (the node), the first internode must needs be that which is crowned by the first leaf or pair of leaves, the cotyledons; and its whole development confirms this view.

Dr. Hooker notes the curious fact that in Welwitschia flower-buds are occasionally produced on the stock below the insertion of the leaves, that is on the radicle or caulicle itself; and Dr. Masters pointed out to him analogous cases of shoots thus originating, one of which was described by Bernhardi thirty years ago. It is simply the case of adventitious buds; these might seem as likely to occur on the first internode as on any later one.

Welwitschia, having a dicotyledonous embryo, has also essentially an exogenous stem, *i. e.*, "the vascular system is referable to the exogenous plan, but its arrangement into concentric wood wedges is very rude." But the superadded isolated and closed vascular bundles of the stock and root, and especially their losing themselves in the periphery of the stock, are endogenous analogies. So also is the strictly parallel and free venation of the leaves; yet, as there are no cross veinlets, thus favoring the splitting up of the leaf into laciniæ, this looks as much or more towards *Cycadaceæ* and broad-leaved *Coniferæ*.

The total absence of anastomosing veinlets in the leaf, each nerve representing a single and independent vascular axis, extending, in Welwitschia, from the axis of the trunk to the apex of the leaf, causes such leaves as these and those of Dammara, etc., to "resemble more closely a series of parallel uninerved leaves united by cellular tissue, than a foliar expansion of parenchyma traversed by one system of inosculating vessels, and the frequent presence of many linear cotyledons in these plants seems to favor this view, as does the

mixed character of the foliage of Podocarpus, of which some species have uninerved and others many-nerved leaves. The numerous flower-buds along the periphery of the crown also further favor this view." That is, in Welwitschia, where this ingenious surmise carries a plausibility, which it does not when applied to Podocarpus.

The binary symmetry of Welwitschia, beginning with the cotyledons, is carried through the inflorescence up to the decussating pairs of bracts of the cones and the two leaflets in each whorl of the hermaphrodite perianth. But the stamens are six, at first sight a monocotyledonous analogy; yet they may be regarded three sets of two, notwithstanding their monadelphy. The flowers are diœcio-polygamous, *i. e.*, of two sorts, one female, the other structurally hermaphrodite, but the gynæcium sterile, though well-developed, except that no embryo-sac appears. The hermaphrodite cones and their flowers accord in many respects strikingly with the male cones of Ephedra; but the anthers are trilocular, which is remarkable. The simple ellipsoidal pollen is the same in both. In Ephedra the stamens vary from two to eight, and the column is solid, there being no rudiment of a gynæcium.

The female fruitful cones are about three inches long, and bright red when fresh.

The integument of the ovule, as in Gnetum, is prolonged at the summit into a style-like body, thus closely simulating a pistil; and the apex of this styliform tube, which is thin and merely erose in the fertile flower, in the structurally hermaphrodite flower is dilated into a broad papillose disk, exactly imitating a highly developed stigma — a marked instance of a highly developed organ which is functionless; for no pollen has been detected upon it, and no embryo-sac in the nucleus. Here Dr. Hooker speculates upon "the possibility of Welwitschia being the only known representative of an existing or extinct race of plants, in which such a stigma-like organ was really capable of performing the function of a stigma. And, when we see this organ occurring in a hermaphrodite flower, it is easy to suppose that we have in Welwitschia a transition in function, as well as in structure,

between the gymnospermous and angiospermous Dicotyledons; and that the ideal race consisted of hermaphrodite plants, in which the office of the stigma of the carpellary leaf was performed by a stigmatic dilatation of the ovular coat itself."

This assumes that the gymnospermous theory established by Brown is correct (whatever be the nature of the cone-scales, rameal or carpellary, simple or compound), and applicable to the Gnetaceous as well as to the Coniferous type. This view, lately much questioned, Dr. Hooker maintains, and enforces, as respects *Gnetaceæ*, by very convincing and in part wholly original arguments, drawn from his own researches upon the present plant and its allies. We refer to pp. 28 to 31, which we could not readily condense, and have not room to copy. The same is to be said in regard to the resemblances or analogies in gynæcial structure between *Gnetaceæ* and *Loranthaceæ*, etc., — a subject upon which we await expectantly Professor Oliver's investigations. Moreover, as Dr. Hooker remarks upon another page, the decisive or final comparative view of the structures in question cannot be had until the homology of the ovule itself is settled. In cases where the flower is so simplified that the nucleus of an ovule directly terminates the floriferous axis, and is surrounded by few and simple, or peculiarly specialized, investments, the discrimination of these must be difficult enough, and must ultimately depend upon the theory adopted as to the nature of the ovular coats. If these be regarded as foliar (as a rigid application of adopted morphological principles will require), then a complete transition from gymnospermy to angiospermy is probable, and may be expected to be demonstrable.

The fertilization and embryology of Welwitschia have been wonderfully worked out, considering the materials, by Dr. Hooker, and the two most elaborate and valuable plates of the memoir are filled with the details. Suffice it to say, that it appears that the pollen must be brought by insects to the ovule of the female flowers, at an early period, before the nucleus is covered by the ovular coat or by the perianth, and before the former has produced its styliform apex, down

which it would be nearly impossible to convey the grains of pollen which were bodily found on the nucleus, with their tubes there produced. So that, notwithstanding the carpel-like form of the ovule, the impregnation is absolutely gymnospermous. As to embryo-formation also, "there is a general agreement in many most essential particulars with *Cycadaceæ* and *Coniferæ*," especially, and beyond what has already been adverted to, in "the free embryo-sac being filled with endosperm-cells previous to fertilization, the numerous secondary embryo-sacs, the position of the germinal vesicle at the base of these sacs, and in the high development of the long tortuous suspensor." There is an agreement with Angiosperms, however, in several particulars, especially in that of "the germinal vesicle giving rise to one embryo only." And it is concluded that, in special reference to Sentalum and Loranthus, "Welwitschia presents an embryogenic process intermediate between that of Gymnosperms and Angiosperms."

And here we should not omit to mention that its wood differs from that of all known Gymnosperms in wanting the disc-bearing wood-cells!

It will be conceded that Welwitschia is "the most wonderful discovery, in a botanical point of view, that has been brought to light during the present century." Also, that Dr. Hooker has enjoyed (and improved) an opportunity unequalled by any botanist since that which placed the Rafflesia in Mr. Brown's hands.

DARWIN'S MOVEMENTS AND HABITS OF CLIMBING PLANTS.

This is a long paper[1] read before the Linnæan Society in February last. The investigations which it records were made, we believe, during a period when the author's ordinary

[1] *On the Movements and Habits of Climbing Plants.* By Charles Darwin. Journal Linnæan Society of London, ix. London, 1867. (American Journal of Science and Arts, 2 ser., xl. 273; xli. 125.)

scientific labors were interrupted by illness, — as was no less the case with respect to his former papers on Dimorphous and Trimorphous Flowers and his volume on the Fertilization of Orchids by the aid of Insects. Of these works and of the present, — side issues as they are, — it may fairly be said, that they show a genius for biological investigation, and a power of turning common materials and ordinary observations to high scientific account, which, if equalled, have not been surpassed since the days of Hunter and Charles Bell. This will be the opinion equally, we suppose, of those who favor and of those who dislike Mr. Darwin's theory of the gradual transformation of specific forms through natural selection, upon which, indeed, all these collateral researches have a bearing, direct or incidental. In the present case the bearing is obvious. The gradual acquisition by certain plants of advantageous peculiarities is inferred from the gradation of forms and functions. Properties and powers which are latent or feebly developed in most plants are taken advantage of by some, made specially useful, and enhanced from generation to generation. Tendril-bearing plants, the most specialized in structure and the most exquisitely adapted to the end in view, are supposed to have been derived from leaf-climbers, and these in turn from simple twiners.

The author states that he was led to this subject by a brief note, communicated to the American Academy in the summer of 1858 (and reprinted in this Journal), in which the writer of the present notice recorded his observation of the coiling of certain tendrils by a visible movement promptly following an extraneous irritation. Mr. Darwin's observations were more than half completed before he became aware that the spontaneous revolution of the stems and of some tendrils of climbing plants had been observed and recorded almost forty years ago, and nearly at the same time, by Palm and by Von Mohl, and had been the subject of two memoirs by Dutrochet, published more than twenty years ago. But the mode in which the free and growing end of a stem sweeps around seems not to have been previously well made out, having been more or less confounded with the torsion of the axis which

many twining stems, such as those of the Hop and the Morning Glory, are apt to undergo. It is plain to see, however, that many stems which revolve do not twist at all; and those that do never could twist on their axis at every revolution without speedy destruction, — indeed usually do not twist until they have ceased revolving. Every one must have noticed that the growing extremity of a Hop, Convolvulus, or other twiner, when unsupported, hangs over or stretches out horizontally to one side. But it is not so well known that this outstretched portion, while at the proper age, is continually sweeping round, in circles widening as it grows, and always in the same direction, in search of some object round which to twine. The Hop revolves with the sun; the Convolvulus, Bean (Phaseolus), etc., against the sun, that is, in the same directions that they twine. Two or three internodes are usually revolving at the same time. Mr. Darwin observed thirty-seven revolutions in one internode of a Hop, — the first revolution made in about twenty-four hours, the second in nine hours, the third and the following ones up to the eighth in a little over three hours each. "The shoot had now grown $3\frac{1}{2}$ inches in length, and carried at its extremity a young internode an inch in length, which showed slight changes in its curvature. The next or ninth revolution was effected in two hours and thirty minutes. From this time forward the revolutions were easily observed. The thirty-sixth revolution was performed at the usual rate; so was the last or thirty-seventh, but it was not quite completed; for the internode abruptly became upright, and after moving to the centre became motionless. I tied a weight to its upper end, so as slightly to bow it, and thus to detect any movement; but there was none. Some time before the last revolution the lower part of the internode had ceased to move. . . . It moved during five days; but the more rapid movement after the third revolution lasted during three days and twenty hours. The regular revolutions from the 9th to the 36th inclusive, were performed at the average rate of 2 h. 31 m. The weather was cold, and this affected the temperature of the room, especially during the night, and consequently retarded a little the rate of movement. . . . After the seven-

teenth revolution the internode had grown from $1\frac{3}{4}$ to 6 inches in length, and carried an internode $1\frac{7}{8}$ inch long, which was just perceptibly moving; and this carried a very minute ultimate internode. After the 21st revolution the penultimate internode was $2\frac{1}{2}$ inches long, and probably revolved in a period of about three hours. At the 27th revolution our lower internode was $8\frac{3}{8}$, the penultimate $3\frac{1}{2}$, and the ultimate $2\frac{1}{2}$ inches in length; and the inclination of the whole shoot was such that a circle 19 inches in diameter was swept by it. When the movement ceased the lower internode was 9 and the penultimate 6 inches in length; so that, from the 27th to the 37th revolutions inclusive, three internodes were at the same time revolving." — (pp. 3, 4.)

The shoots of many climbers sweep their circles more rapidly than the Hop, the common Pole Bean (*Phaseolus vulgaris*) in rather less than two hours, Convolvuluses of various species in the same time or rather less; while more woody stems naturally move more slowly, some requiring from 24 to 50 hours for each revolution. But the thickness or texture of the shoot does not govern the rate, many slender shoots moving slower than some stout ones, and some lignescent quicker than other purely herbaceous ones. The movement appears to be accelerated, up to a certain point, by raising the temperature, or rather is retarded by lowering it; but while the conditions are nearly the same, the rate is often remarkably uniform. The quickest rate of revolution of a proper stem observed by Mr. Darwin was that of a Scyphranthus, in 77 minutes. When the light comes from one side, the semicircle towards the light is usually described in less time, often in less than half the time, of that from the light. The tendency of young stems to turn toward the light is here active as usual, but is overcome by a superior force. The end of the shoot describes circles or broad ellipses, or else, from insufficient power or mechanical disadvantages, narrow ellipses, semicircles, or irregular figures. A horizontal shoot of considerable length will thus be found, not unfrequently, to sway from side to side in a semicircular course, while the extreme internodes are making complete revolutions.

A striking illustration of the amount of space that may be swept over is afforded by a case in which Mr. Darwin allowed the top of a Ceropegia to grow out almost horizontally to the length of 31 inches, — three long internodes terminated by two short ones. The whole revolved at rates between $5\frac{1}{4}$ and $6\frac{3}{4}$ hours for each revolution, the tip sweeping a circle of above five feet in diameter and 16 feet in circumference, traveling therefore at the rate of at least 32 inches per hour. "It was an interesting spectacle to watch the long shoot sweeping, night and day, this grand circle, in search of some object around which to twine."

As to the nature of this revolving movement, Mr. Darwin clearly shows that it is not a torsion of the axis, but a successive bending (similar to that by which ordinary stems bend toward the light), the direction of which is constantly and uniformly changing. "If a colored streak be painted (this was done with a large number of twining plants) along, we will say, the convex line of surface, this colored streak will, after a time depending on the rate of revolution, be found to lie along one side of the bow, then along the concave side, then on the opposite side, and, lastly, again on the opposite convex surface. This clearly proves that the internodes, during the revolving movement, become bowed in every direction. The movement is, in fact, a continuous self-bowing of the whole shoot, successively directed to all points of the compass." It is an automatic movement, of the same character as those which these and other parts of plants effect in changing position or direction, sometimes slowly and sometimes with a visible motion. The movement may be likened in one case to that of the hour-hand or the minute-hand of a clock, in the other to the second-hand, but in both is as truly a vital movement as is the contraction of an involuntary muscle. It must be effected — as Mr. Darwin recognizes — either by the contraction of the cells on the concave side, or by the turgescence and elongation of those on the convex side of the internode, or by both, — probably the former, as various facts go to show; but questions of that kind are not investigated in the present essay.

No differences in this regard are observable in the behavior of exogenous or endogenous stems, or even of those of climbing Ferns. *Lygodium scandens*, according to Darwin, revolves like other twiners; it completes its revolutions in six hours, or on a very hot day in five (moving against the sun, which is much the commoner case); this is about the average rate of Phænogamous twiners, like which it comports itself in all respects. Our own *L. palmatum*, we find, revolves in the same way, in about four hours, the temperature being 75° Fahr.

The power of revolving depends, of course, upon the general health and vigor of the plant, and upon the age of the shoot, is retarded by lowering the temperature, is interrupted by any considerable disturbance, such as exposure to cold or to much jarring; carrying the plant from one place to another, or cutting off a shoot and placing it in water, stops the movement for a time, just as it does the more vivid automatic movement of *Desmodium gyrans*. But each internode is so independent that cutting off an upper one does not affect the revolutions of the one beneath. Twining stems are far from being insensible to the action of light (as Mohl supposed), the half-revolution toward the light being not uncommonly twice faster than that from it; but as the rate of revolution by day and by night is nearly the same, the one half of the circuit is accelerated just as much as the other is retarded. This influence of the light is quite remarkable when we consider the slenderness of most revolving internodes, the small surfaces they expose, and that their leaves are little developed.

The design, as we must term it, of this revolving of the end of twining stems is obvious, and usually effectual. Such stems, even when no supporting object is within their reach, will reach each other, and by twining together make a mutual support, from which, as they lengthen, they may reach yet farther. The connection of the revolving with twining is obvious, though the latter is not a necessary consequence; for many stems revolve which do not twine, but climb in some other way.

"When at last the [revolving] shoot meets with a support,

the motion at the point of contact is necessarily arrested, but the free projecting part goes on revolving. Almost immediately another and upper point of the shoot is brought into contact with the support and is arrested; and so onward to the extremity of the shoot; and thus it winds round its support. When the shoot follows the sun in its revolving course, it winds itself round the support from right to left, the support being supposed to stand in front of the beholder; when the shoot revolves in an opposite direction, the line of winding is reversed. As each internode loses from age its power of revolving, it loses its power of spirally twining round a support. If a man swings a rope round his head, and the end hits a stick, it will coil round the stick according to the direction of the swinging rope; so it is with twining plants, the continued contraction or turgescence of the cells along the free part of the shoot replacing the momentum of each atom of the free end of the rope.

"All the authors, except Von Mohl, who have discussed the spiral twining of plants maintain that such plants have a natural tendency to grow spirally. Mohl believes (S. 112) that twining stems have a dull kind of irritability, so that they bend toward any object which they touch. Even before reading Mohl's interesting treatise, this view seemed to me so probable that I tested it in every way that I could, but always with negative results. I rubbed many shoots much harder than is necessary to excite movement in any tendril or in any foot-stalk of a leaf-climber, but without result. I then tied a very light forked twig to a shoot of a Hop, a Ceropegia, Sphærostema, and Adhatoda, so that the fork pressed on one side alone of the shoot and revolved with it; I purposely selected some very slow revolvers, as it seemed most likely that these would profit from possessing irritability; but in no case was any effect produced. Moreover, when a shoot winds round a support, the movement is always slower, as we shall immediately see, than whilst it revolves freely and touches nothing. Hence I conclude that twining stems are not irritable; and indeed it is not probable that it should be so, as nature always economizes her means, and irritability would be

superfluous. Nevertheless I do not wish to assert that they are never irritable; for the growing axis of the leaf-climbing, but not spirally twining, *Lophospermum scandens* is, as we shall hereafter see, certainly irritable; but this case gives me confidence that ordinary twiners do not possess this quality, for directly after putting a stick to the Lophospermum, I saw that it behaved differently from any true twiner or any other leaf-climber.

"The belief that twiners have a natural tendency to grow spirally probably arose from their assuming this form when wound round a support, and from the extremity, even whilst remaining free, sometimes assuming this same form. The free internodes of vigorously growing plants, when they cease to revolve, become straight, and show no tendency to be spiral; but when any shoot has nearly ceased to grow, or when the plant is unhealthy, the extremity does occasionally become spiral. I have seen this in a remarkable degree with the ends of the shoots of the Stauntonia and of the allied Akebia, which became closely wound up spirally, just like a tendril, especially after the small, ill-formed leaves had perished. The explanation of this fact is, I believe, that the lower parts of such terminal internodes very gradually and successively lose their power of movement, whilst the portions just above move onward, and in their turn become motionless; and this ends in forming an irregular spire.

"When a revolving shoot strikes a stick, it winds round it rather more slowly than it revolves. For instance, a shoot of the Ceropegia took 9 hours and 30 minutes to make one complete spire round a stick, whilst it revolved in 6 hours; *Aristolochia gigas* revolved in about 5 hours, but took 9 hours and 15 minutes to complete its spire. This, I presume, is due to the continued disturbance of the moving force by its arrestment at each successive point; we shall hereafter see that even shaking a plant retards the revolving movement. The terminal internodes of a long, much-inclined, revolving shoot of the Ceropegia, after they had wound round a stick, always slipped up it, so as to render the spire more open than it was at first; and this was evidently due to the force which caused the revolu-

tions being now almost freed from the constraint of gravity, and allowed to act freely. With the Wistaria, on the other hand, a long, horizontal shoot wound itself at first, in a very close spire, which remained unchanged; but subsequently, as the shoot grew, it made a much more open spire. With all the many plants which were allowed freely to ascend a support, the terminal internodes made at first a close spire; and this, during windy weather, well served to keep the shoots in contact with their support; but as the penultimate internodes grew in length, they pushed themselves up for a considerable space (ascertained by colored marks on the shoot and on the support) round the stick, and the spire became more open.

"If a stick which has arrested a revolving shoot, but has not as yet been wound round, be suddenly taken away, the shoot generally springs forward, showing that it has continued to press against the stick. If the stick, shortly after having been wound round, be withdrawn, the shoot retains for a time its spiral form, then straightens itself, and again commences to revolve. The long, much-inclined shoot of the Ceropegia previously alluded to offered some curious peculiarities. The lower and older internodes, which continued to revolve, had become so stiff that they were incapable, on repeated trials, of twining round a thin stick, showing that the power of movement was retained after flexibility had been lost. I then moved the stick to a greater distance, so that it was struck by a point $2\frac{1}{2}$ inches from the extremity of the penultimate internode; and it was then neatly wound round by this part and by the ultimate internode. After leaving the spirally wound shoot for eleven hours, I quietly withdrew the stick, and in the course of the day the curled part straightened itself and re-commenced revolving; but the lower and not curled portion of the penultimate internode did not move, a sort of hinge separating the moving and the motionless part of the same internode. After a few days, however, I found that the lower part of this internode had likewise recovered its revolving power. These several facts show, that, in the arrested portion of a revolving shoot, the power of movement is not immediately lost, and that when temporarily lost it can be re-

covered. When a shoot has remained for a considerable time wound round its support, it permanently retains its spiral form even when the support is removed.

"When a stick was placed so as to arrest the lower and rigid internodes of the Ceropegia at the distance at first of 15 and then of 21 inches from the centre of revolution, the shoot slowly and gradually slid up the stick, so as to become more and more highly inclined; and then, after an interval sufficient to have allowed of a semi-revolution, it suddenly bounded from the stick and fell over to the opposite side, to its ordinary slight inclination. It now recommenced revolving in its usual course, so that after a semi-revolution it again came into contact with the stick, again slid up it, and again bounded from it. This movement of the shoot had a very odd appearance, as if it were disgusted with its failure but resolved to try again. We shall, I think, understand this movement by considering the former illustration of the sapling, in which the contracting surface was supposed to creep from the southern, by the eastern, to the northern, and thence back again by the western side to the southern face, successively bowing the sapling in all directions. Now with the Ceropegia, the stick being placed a very little to the east of due south of the plant, the eastern contraction could produce no effect beyond pressing the rigid internode against the stick; but as soon as the contraction of the northern face began, it would slowly drag the shoot up the stick; and then, as soon as the western contraction had well begun, the shoot would be drawn from the stick, and its weight coinciding with the northwestern contraction, would cause it suddenly to fall to the opposite side with its proper slightly inclined positions; and the ordinary revolving movement would go on. I have described this case because it first made me understand the order in which the contracting or turgescent cells of revolving shoots must act.

"The view just given further explains, as I believe, a fact observed by Von Mohl (S. 135), namely, that a revolving shoot, though it will twine round an object as thin as a thread, cannot do so round a thick support. I placed some long revolving shoots of a Wistaria close to a post between 5 and

6 inches in diameter, but they could not, though aided by me in many ways, wind round it. This apparently is owing to the flexure of the shoot, when winding round an object so gently curved as this post, not being sufficient to hold the shoot to its place when the contracting force creeps round to the opposite surface of the shoot; so that it is at each revolution withdrawn from its support." — (pp. 9–13, *passim.*)

The successive shifting of the contracting side of the shoot, which explains the revolution or bowing in turn in every direction, no less explains the twining round a proper support, leaving however some idiosyncrasies unexplained. Some tendrils and some petioles of leaf-climbing plants equally possess this revolving power; but their usefulness depends mainly upon additional and more special endowments, — mainly upon the power of directly responding by curvature to the contact, more or less prolonged, of an extraneous body.

Of Leaf-climbers, no instance is more familiar than that of Clematis or Virgin's Bower. Little more was known of them than that they climbed by curling their petioles (common or partial) around neighboring objects. Mr. Darwin made observations upon eight species of Clematis, seven of Tropæolum, the common species of Maurandia, Lophospermum, Fumaria, etc., as also upon Gloriosa and Flagellaria, which climb by a tendril-like production of the tip of the leaf. From the summary it appears that plants which belong to eight families are known to have clasping petioles, and those of four families climb by the tips of their leaves. In almost all of them the young internodes revolve, in some of them as extensively as in twining plants, — the movement being plainly serviceable in bringing the petioles or the tips of the leaves into contact with surrounding objects. Those whose shoots revolve most freely are also capable of twining spirally around a support; but when the stem twines (as in *Clematis Sieboldii* and *C. calycina*, but not in *C. Viticella*), it has the peculiarity of winding first in one direction for two or three turns, and then in the opposite direction. The petioles are principally efficient in these plants, and that by means of an endowment which is not shown to belong to twining stems, with one or two exceptions. That

is, the petioles or their divisions are sensitive to the contact of an extraneous body, contracting on the side touched so as to curve or coil around it. That the footstalk is directly sensitive to the touch, just as tendrils are, Mr. Darwin proved by lightly rubbing them with a twig for a few times, when in the course of some hours it bends to the rubbed side, afterwards becoming straight again; or by leaving the body in contact it is permanently clasped by the footstalk. So sensitive are some footstalks that "a loop of thread weighing a quarter of a grain caused them to bend; a loop weighing one-eighth of a grain sometimes acted, and sometimes not." In one instance, in *Clematis Flammula*, even the sixteenth part of a grain caused a petiole to bend through nearly 90 degrees. With rare exceptions only the young petioles are sensitive. Take the cultivated *Clematis Viticella* for an illustration of the mode in which the leaves do the work of climbing.

"The leaves are of large size. There are three pairs of lateral leaflets and a terminal one, all borne by rather long petioles. The main petiole bends a little, angularly, downward at each point where a pair of leaflets arises, and the petiole of the terminal leaflet is bent downward at right angles; hence the whole petiole, with its rectangularly bent extremity, acts as a hook. This, with the lateral petioles directed a little upward, forms an excellent grappling apparatus by which the leaves readily become entangled with surrounding objects. If they catch nothing, the whole petiole ultimately grows straight. Both the medial and lateral petioles are sensitive; and the three branches, into which the basi-lateral petioles are generally subdivided, likewise are sensitive. The basal portion of the main petiole, between the stem and the first pair of leaflets, is less sensitive than the remainder, but it will clasp a stick when in contact. On the other hand, the inferior surface of the rectangularly bent terminal portion (carrying the terminal leaflet), which forms the inner side of the end of the hook, is the most sensitive part; and this portion is manifestly best adapted to catch distant supports. To show the difference in sensibility, I gently placed loops of string of the same weight (in one instance weighing .82 of a grain) on the sev-

eral lateral and on the terminal sub-petioles; in a few hours the latter were bent, but after 24 hours no effect was produced on any of the lateral petioles. Again, a terminal sub-petiole placed in contact with a thin stick became sensibly curved in 45 minutes, and in 1 hour 10 minutes had moved through ninety degrees, whereas a lateral petiole did not become sensibly curved until 3 hours and 30 minutes had elapsed. In this latter case, and in all other such cases, if the sticks be taken away, the petioles continue to move during many hours afterward; so they do after a slight rubbing; but ultimately, if the flexure has not been very great or long-continued, they become, after about a day's interval, straight again." — (p. 31.)

In numerous cases, notably in *Solanum jasminoides*, the petiole when clasped increases very greatly in thickness and rigidity, undergoing a change in its woody structure by which the fibro-vascular bundles, originally semi-lunar in cross-section, develop into a closed ring, like that of an exogenous stem.

Lophospermum scandens of the gardens climbs, like its allies Maurandia and Rhodochiton, by clasping petioles; but in this plant, alone, the young internodes are also sensitive to the touch.

"When a petiole clasps a stick, it draws the base of the internode against it; and then the internode itself bends toward the stick, which is thus caught between the stem and the petiole as by a pair of pincers. The internode straightens itself again, excepting the part in contact with the stick. Young internodes alone are sensitive, and these are sensitive on all sides along their whole length. I made fifteen trials by lightly rubbing two or three times with a thin twig several internodes; and in about 2 hours, but in one case in 3 hours, all became bent; they became straight again in about 4 hours, subsequently. An internode, which was rubbed as much as six or seven times with a twig, became just perceptibly curved in 1 hour 15 minutes, and subsequently in 3 hours the curvature increased much; the internode became straight again in the course of the night. I rubbed some internodes one day on one side, and the next day on the opposite side or at right

angles; and the curvature was always toward the rubbed side."

Here, then, is one case in which the sensibility of a stem is manifest, and is turned to useful account. The peduncles of the allied *Maurandia semperflorens* are also sensitive and flexuous, although Mr. Darwin insists that they are useless for climbing. That some stems should be sensitive might have been expected; for tendrils of axial nature (*e. qr.* of *Passiflora gracilis*) are not less sensitive than those of foliar nature, as of *Leguminosæ*, *Cucurbitaceæ*, and Cobæa. And if twining stems in general are not endowed with "a dull kind of irritability," as Mohl conjectured, it may well be because the equally wonderful automatic revolving movement leaves no need for it. In general, the most striking cases of automatic movement belong to leaves or their homologues.

The distinction can be only somewhat arbitrarily drawn between Leaf-climbers — especially those with small or undeveloped leaflets, or where the tip of the leaf forms a hook or tendril-like projection — and Tendril-climbers. The tendril, however, whether answering to leaf or stem, is the more specialized organ, adapted only for climbing, and endowed in different plants with very various and some highly remarkable powers. To this subject Mr. Darwin has devoted more than half of his essay. An analysis of it must be deferred, for want of space.

Near the close of the essay, under Hook-climbers, Mr. Darwin remarks that: —

"Even some of the climbing Roses will ascend the walls of a tall house, if covered with a trellis; how this is effected I know not; for the young shoots of one such Rose, when placed in a pot in a window, bent irregularly toward the light during the day and from it during the night, like any other plant; so that it is not easy to understand how the shoots can get under a trellis close to a wall."

Now we have had occasion to observe that the strong summer-shoots of the Michigan Rose (*Rosa setigera*, Mx., *R. rubifolia*, R. Br.), trained on a latticed wall, are strongly disposed to push into dark crevices and away from the light; they

would, many of them, pretty surely place themselves under the trellis, and the lateral shoots of the next spring would emerge as they seek the light. We suspect this is also true of the Sweet Brier.

Twiners and Leaf-climbers having been considered, Tendril-bearers, which are the highest style of climbing plants, next demand our attention. But our analysis of this important part of Mr. Darwin's treatise must be disproportionably brief.

There are two kinds of movement exhibited by plants, which should be distinguished. 1st, Automatic, usually continued movements, not set in action by extraneous invitation. The gyratory movement of the small leaflets of *Desmodium gyrans* is an exalted instance of this. 2d, Movements in consequence of the contact or action of an extraneous body, — of which those of the leaves of the Sensitive Plant may be taken as the type. Twining stems, as has been seen, strikingly exhibit the first, and their coiling around a support is a consequence of it.

Tendrils for the most part execute both kinds of movement. They revolve, with some exceptions, like twining stems; and they are all more or less sensitive to contact, — usually more so than the petioles of leaf-climbers, — bending toward the impinging body so as to hook or clasp around it, if the size will allow. Different tendrils act differently in some respects, some revolving freely, and sweeping wide circuits, some less evidently, and some, like those of the Virginia Creeper, do not revolve at all, but turn from the light to the dark. But whether the tendril is the homologue of a leaf, or of a stem (or of a peduncle, which is the same thing), appears to make no difference in its action. On the other hand their diversity of gifts in one and the same family, or even in species of the same genus, is very remarkable, as may be seen especially in the Bignonia Family, the Grape Family, etc. So, also, the tendrils are commonly aided in their endeavors by the revolving of the internodes of the stem, but sometimes not, even in plants of the same genus or family. Mr. Darwin takes up tendril-bearing plants by

natural families, beginning with *Bignoniaceæ*, which order contains tendril-bearers, leaf-climbers, twiners, root-climbers, and various combinations of these diverse modes. We, however, will first consider the tendrils of the Gourd, and Passion-flower families, regarding them as typical and simple representatives of tendril-climbers.

Passiflora gracilis, a delicate annual species, lately introduced into the gardens, of the easiest cultivation, one which differs from most of its relatives in the young internodes having the power of revolving, is said by Mr. Darwin to exceed all other climbing plants in the rapidity of its movements, and all tendril-bearers in the sensitiveness of its tendrils. In the latter respect it decidedly surpasses our Echinocystis; but it is nearly if not quite equalled by Sicyos, in which the coiling upon contact was first noticed as a visible movement. The revolving internodes, when in the best condition, make almost hourly revolutions, and the long, delicate, straight tendrils revolve nearly in the same manner and at the same rate. The sensitiveness of the tendril, when full-grown, is correspondingly great, a single light touch on the concave surface of the tip causing a considerable curvature. "A loop of soft thread weighing $\frac{1}{3}\frac{1}{2}$d of a grain, placed most gently on the tip, thrice plainly caused it to curve, as twice did a bent bit of thin platinum wire weighing $\frac{1}{5}\frac{1}{0}$th of a grain; but this latter weight, when left suspended, did not suffice to cause permanent curvature." After touch with the twig, the tip begins to bend in from 25 to 39 seconds. After coiling into an open helix upon transient irritation, they soon straighten again, recovering their sensibility; but if left in contact, the action continues. We found it a pretty experiment, last summer, during the warmest days, to bring the upper part of an outstretched tendril by its inner or concave side against a twig or cord, and to see how promptly it would clasp it, revolving its free apex round and round it. A curious discrimination in the sensibility of such tendrils is mentioned by Mr. Darwin, as follows: —

"I repeated the experiment made on the Echinocystis, and placed several plants of this Passiflora so close together that

the tendrils were repeatedly dragged over each other; but no curvature ensued. I likewise repeatedly flirted small drops of water from a brush on many tendrils, and syringed others so violently that the whole tendril was dashed about, but they never became curved. The impact from the drops of water on my hand was felt far more plainly than that from the loops of thread (weighing $\frac{1}{3\,2}$d of a grain) when allowed to fall upon it, and these loops, which caused the tendrils to become curved, had been placed most gently on them. Hence it is clear, either that the tendrils are habituated to the touch of other tendrils and to that of drops of rain, or that they are sensitive only to prolonged though excessively slight pressure. To show the difference of the kind of sensitiveness in different plants, and likewise to show the force of the syringe used, I may add that the lightest jet from it instantly caused the leaves of the Mimosa to close; whereas the loop of thread weighing $\frac{1}{3\,2}$d of a grain, when rolled into a ball and gently placed in the glands at the base of the leaflets of the Mimosa, caused no action." — (p. 90.)

Of Cucurbitaceous tendrils, the most active, after those of Sicyos (which Mr. Darwin has not observed), are those of *Echinocystis lobata*. The internodes and tendrils revolve in about an hour and three quarters, the former sweeping a circle or ellipse of two or three inches in diameter, the latter often one of 15 or 16 inches in diameter. Perhaps the most remarkable appearance of discrimination in tendrils is that which Mr. Darwin first noticed in this plant, but which may be seen in others, — and which he thus describes: —

"I repeatedly saw that the revolving tendril, though inclined during the greater part of its course at an angle of about 45° (in one case of only 37°) above the horizon, in one part of its course stiffened and straightened itself from tip to base, and became nearly or quite vertical. . . . The tendril forms a very acute angle with the extremity of the shoot, which projects above the point where the tendril arises; and the stiffening always occurred as the tendril approached and had to pass, in its revolving course, the point of difficulty, — that is, the projecting extremity of the shoot. Unless the ten-

dril had the power of thus acting, it would strike against the extremity of the shoot, and be arrested by it. As soon as all these branches of the tendrils begin to stiffen themselves in this remarkable manner, as if by a process of turgescence, and to rise from an inclined into a vertical position, the revolving movement becomes more rapid; and as soon as the tendril has succeeded in passing the extremity of the shoot, its revolving motion, coinciding with that from gravity, often causes it to fall into its previous inclined position so quickly, that the end of the tendril could distinctly be seen travelling like the minute-hand of a gigantic clock." — (p. 75.)

Cucurbitaceous tendrils are mostly compound, in this case three-forked. When one of the lateral branches has firmly clasped any object, the middle branch continues to revolve. If a full-grown tendril fails to reach and lay hold of any object, it soon ceases to revolve, bends downwards, and coils up spirally from the apex. Indeed it often coils while still outstretched and revolving, the tendency to shorten (as we presume) on the inner side from the tip downward, which is usually brought into action by contact with an extraneous body, at length operating spontaneously. Uncaught tendrils when they thus coil up throw themselves of course into a simple helix or spire. One end being free, this is the simple and necessary consequence of the relative shortening of the concave side, sufficiently continued.

In a caught tendril, the relative shortening of one side, (through which the tip hooks round and fixes itself to the supporting object), being propagated downwards, the whole now throws itself into a spiral form, — with more or less promptitude according to the species, — thus pulling the free portion of the tendril-bearing shoot nearer to the support, and within easier reach of the next tendril above. Both ends of the tendril being fixed, and the winding round an axis (real or imaginary) necessarily involving or being a twist, it is certain that the caught tendril cannot now coil into a simple spiral, but that the spire will at least be double, a coil near one end of the tendril in one direction requiring the other to twist in the opposite direction, unless indeed it undergoes torsion. So, as

is familiarly known, there is at least one neutral point in a caught and coiled-up tendril, usually in the middle, the turns on one side of it running from right to left, on the other side from left to right. That the coils, whether simple or double and reversed (as the case may be), are not determined by any peculiarity in the tendril, but merely by the relative shortening of one side, may be readily shown by a thread cut from a piece of india-rubber, of unequal tension of the two sides; this, when stretched and allowed to shorten while the two ends are held fast in the same plane, forms at once a pair of reverse coils, or three or four such coils, just as caught tendrils do.

Mr. Darwin explains the point by analogous practical illustrations. He shows, moreover, that an important service rendered by the coiling or spiral contraction "is that the tendrils are thus made highly elastic." In the Virginia Creeper, where the ends of the compound tendrils are peculiarly attached, "the strain is thus equally distributed to the several attached branches of a branched tendril; and this must render the whole tendril far stronger, as branch after branch cannot separately break. It is this elasticity which saves both simple and branched tendrils from being torn away during stormy weather. I have more than once gone on purpose, during a gale, to watch a Bryony growing in an exposed hedge, with its tendrils attached to the surrounding bushes; and as the thick or thin branches were tossed to and fro by the wind, the attached tendrils, had they not been excessively elastic, would have been instantly torn off and the plant thrown prostrate. But as it was, the Bryony safely rode out the gale, like a ship with two anchors down and a long range of cable ahead, to serve as a spring as she surges to the storm."

Moreover, while unattached tendrils soon shrink up or wither and fall off, as we observe in the Grapevine, Virginia Creeper, etc., these same plants show how an attached tendril thickens and hardens, gaining wonderfully in strength and durability. In a Virginia Creeper, "one single lateral branchlet of a (dead) tendril, estimated to be at least ten years old, was still elastic and supported a weight of exactly two pounds.

This tendril had five disk-bearing branches, of equal thickness and apparently equal strength; so that this one tendril, after having been exposed during ten years to the weather, would have resisted a strain of ten pounds."

Our space will not allow even an abstract of Darwin's account of the admirable adaptations and curious behavior of various tendrils, even of some very common plants; as for instance of the familiar *Cobœa scandens*, in which (the stem and the petioles being motionless) the great compound tendril borne at the summit of the leaf executes large circular sweeps with remarkable rapidity, carrying round an elaborate flexible grapnel, consisting of its five subdivisions, from 50 to 100 in number, which are very sensitive even to a slight touch, bending in a few minutes toward the touched side, so that they clasp twigs very promptly, and all tipped with minute, double or sometimes single, sharp hooks, which catch in little inequalities, and may prevent the tendril branchlets from being dragged away by the rapid revolving movement before their irritability has time to act, while the still free ones proceed to arrange themselves, by various queer and complicated movements, so as to secure the most advantageous hold; then contracting spirally so as to bring other portions up within reach of the support, until all are inextricably knotted and fastened, and finally growing stouter, rigid and strong, binding the plant firmly to its support.

We cannot omit all mention of *Bignonia capreolata*, a not uncommon climber of our Southern States, of which we especially wish to obtain fresh seeds or young plants, that we may ourselves observe the remarkable behavior of its tendrils which Mr. Darwin describes. These are said to turn from the light, as in many other cases; they will clasp smooth sticks, but soon lose their hold and straighten themselves again. A rough, fissured, or porous surface alone satisfies them; their young tips seek and crawl into dark holes and crevices, in the manner of roots; then they develop their hooked extremity, and, especially when they meet with any fibrous matter, the hook swells into irregular balls of cellular tissue, which first adhere to the fibres by a viscid cement, and then grow so as to de-

velop them. This tendril can do nothing with a smooth post, fails to attach itself to a brick wall, but is well adapted to climb trees with rough and mossy bark.

The Virginia Creeper also turns its tendrils from the light, and, although they will occasionally clasp a slender support, in the manner of its relative the Grapevine, they uniformly seek dark crevices, or especially broad flat surfaces, as a wall, a rock, or the trunk of a tree. Having brought their curved tips into contact with such a surface, these swell and form, in the course of a few days, the well-known disks or cushions by which they firmly adhere. Here is a tendril-climber which emulates a root-climber, such as the Ivy, in the facility with which it ascends smooth trunks, rocks, or walls.

A very short chapter is devoted to Hook-climbers and Root-climbers. The stems of the latter are said to "have usually no power of movement, not even from the light to the dark. But *Hoya carnosa*, which twines, also climbs by rootlets spreading over the face of a damp wall; and *Tecoma radicans* (our Trumpet Creeper) exhibits in its young shoots some vestiges of the revolving power with which its twining relatives are endowed."

In a dozen pages of *Concluding Remarks*, Mr. Darwin gives much interesting matter in the way of deduction and speculation, which it would be difficult to condense into an abstract.

Plants become climbers, he remarks, in order to reach the light, and expose a large surface of leaves to its action and that of the free air. Their advantage is, that they do this with wonderfully little expenditure of organized matter in comparison with trees, which have to support a heavy load of branches by a massive trunk. Of the different sorts of climbers hook-climbers are the least efficient, at least in temperate countries, as they climb only in the midst of an entangled mass of vegetation. Next root-climbers, which are admirably adapted to ascend naked faces of rock; but when they climb trees they must keep much in the shade, and follow the trunk; for their rootlets can adhere only by long-continued and close contact with a steady surface. Thirdly,

spiral twiners, with leaf-climbers and tendril-bearers, which agree in their power of spontaneously revolving and of grasping objects which they reach, are the most numerous in kinds, and most perfect in mechanism; they can easily pass from branch to branch, and securely ramble over a wide and sunlit surface.

After adducing some considerations in support of his opinion that both leaf-climbers and tendril-bearers "were primordially twiners, that is, are the descendants of plants having this power and habit," Mr. Darwin asks: "Why have nearly all the plants in so many aboriginally twining groups been converted into leaf-climbers or tendril-bearers? Of what advantage could this have been to them? Why did they not remain simple twiners? We can see several reasons. It might be an advantage to a plant to acquire a thicker stem, with short internodes, bearing many or large leaves; and such stems as are ill fitted for twining. Any one who will look during windy weather at twining plants will see that they are easily blown from their support; not so with tendril-bearers or leaf-climbers, for they quickly and firmly grasp their support by a much more efficient kind of movement. In those plants which still twine, but at the same time possess tendrils or sensitive petioles, as some species of Bignonia, Clematis, and Tropæolum, we can readily observe how incomparably more securely they grasp an upright stick than do simple twiners. From possessing the power of movement on contact, a tendril can be made very long and thin; so that little organic matter is expended in their development, and yet a wide circle is swept. Tendril-bearers can, from their first growth, ascend along the outer branches of any neighboring bush, and thus always keep in the full light; twiners, on the contrary, are best fitted to ascend bare stems, and generally have to start in the shade. . . .

"The object of all climbing plants is to reach the light and free air with as little expenditure of organic matter as possible; now, with spirally-ascending plants the stem is much longer than is absolutely necessary; for instance, I measured the stem of a Kidney-bean which had ascended exactly two

feet in height, and it was three feet in length. The stem of a Pea, ascending by its tendrils would, on the other hand, have been but little longer than the height gained. That this saving of stem is really an advantage to climbing plants I infer from observing that those that still twine, but are aided by clasping petioles or tendrils, generally make more open spires than those made by simple twiners." — (p. 110.)

The gradations between one organ and another, and their special endowments, and the great diversity of their movements, are illustrated at length; and the very large number of natural families which exhibit these endowments, in some of their members, is indicated; and it is noted that two or three genera alone have those powers in some of the largest and best defined natural orders, such as *Compositæ*, *Rubiaceæ*, *Liliaceæ*, Ferns, etc.; from which he infers "that the capacity for acquiring the revolving power, on which most climbers depend, is inherent, though undeveloped, in almost every plant in the vegetable kingdom" (p. 117).

Mr. Darwin somewhere throws out the remark that the larger number, and the most perfectly organized climbing plants, as of the scandent animals, belong to one country, tropical America.

In abruptly closing these extracts and brief commentaries, we would add, that the Linnæan Society has issued a separate reprint of this charming treatise, thus opening to it a wider circle of readers.

WATSON'S BOTANY OF THE 40TH PARALLEL.

We propose to notice this volume[1] particularly: indeed it well deserves a more thorough examination and more extended review than our time and space will now allow us to devote to it. It is published "by order of the Secretary of War, under the authority of Congress," as one of the Engi-

[1] *United States Geological Survey of the 40th Parallel.* V. Botany. By Sereno Watson. Washington, 1871. (American Journal of Science and Arts, 3 ser., iii. pp. 62 and 148.)

neer Corps series, has been carefully edited and beautifully printed, so that the volume is every way an attractive one. Errors of the press are to be found, but they are apparently few, and the whole typography is remarkably excellent for the Government Printing Office. Our comparison is naturally with corresponding volumes of the Pacific Railroad Survey, and of the Mexican Boundary Survey, upon which the present volume is a notable improvement. The forty plates, filled with well chosen subjects, if not of the very highest style, are so well done and of such excellent promise that the name of the draughtsman (who is new to this class of work, we believe), Mr. J. H. Emerton, of Salem, Massachusetts, should properly have been appended to them.

The General Report, of 53 pages, forms a separately paged introduction to the "Catalogue," as it is termed with excessive modesty, *i. e.*, the systematic account of the plants collected, which makes up the principal bulk of the volume. This General Report will naturally be most interesting to general readers and naturalists, but no less so to special botanists. It is thoroughly readable matter, and we expect to see it reproduced in the scientific journals. Four or five pages sketch the geographical features of the region, tersely and clearly. But, when a stream of water is said to "become demoralized with alkali and is lost," we could wish that this popularized use of the word were buried with it. The meteorological notes, with tabulated observations by thermometer, evaporator, etc., are equally interesting, displaying the dryness of the Great Basin, its cold winters and hot summers. The notes on the general character of the vegetation picture to us the botanical aspect of the region, the relative prevalence of the predominant species, the slow and cross-grained growth of what timber there is in the cañons, etc. A dead branch, apparently of *Pinus monophylla*, 8 inches in diameter, had the fibres so twisted that in 7 feet they made four complete circuits. A saw-mill in Ruby Valley offered the opportunity of ascertaining the age and dimensions of several specimens of *Pinus flexilis* from the upper cañons of the Humboldt Mountains; sections from 22 to 30 inches in

diameter showed from 400 to 486 annual rings. The "everlasting sage-bush," *Artemisia tridentata*, displayed 65 rings on a section 8 inches in diameter, 37 upon 4 inches, etc. A *Juniperus occidentalis*, 12 inches in diameter, showed 250 rings. *Cercocarpus ledifolius*, it appears, may form a trunk of 2 feet in diameter, with 160 rings. The alkaline species, aquatic and meadow species, those of the drier valleys and foot-hills, the mountain species, etc., are separately enumerated; the introduced species, about 30 in number, are recorded, and finally the number of indigenous genera and species is given under their orders, and their distribution in the basin or over the borders on either side is tabulated. Of the 1141 species of the basin and of the Wahsatch and Uintas, 60 per cent. appear to inhabit also the Pacific slope, about 60 per cent. are not found east of the Rocky Mountains, 15 per cent. only approach the Mississippi or the Saskatchewan, 25 per cent. approach the Atlantic, 17 per cent. are Mexican or southern, and nearly 15 per cent. are Arctic.

A few pages at the close are devoted to the consideration of the agricultural resources of the basin, the limit to which is fixed by the deficiency of water. "The most fertile localities lie at the base of the Sierras; but, as a rule, there is an apparent absence everywhere of a true soil or mould resulting from the decomposition of vegetable matter." A moderate amount of alkali in the soil appears not to be detrimental to culture. The soil which produces "sage bush" seems to be always cultivable when it can be irrigated. With the present supply of water, most economically used, it is thought that only one thousand out of 34,000 square miles of northern Nevada could be brought under cultivation; of the southerly portion and of western Utah much less. Eastern Utah, with larger and more constant supplies of water from the Wahsatch and the Uinta Mountains, is much more favorably situated. The absence of graminivorous animals, excepting rabbits in the valleys and rarely a few mountain sheep or antelopes in the higher ranges, shows that the country is ill adapted for grazing. *Eurotia lanata* and a few other Chenopodiaceous plants are eaten by sheep as a substitute for grass.

Mr. Watson raises the question whether — considering the amount of low shrubby and perennial vegetation which inhabits the plains and thrives without irrigation — these plants themselves, or some more serviceable substitutes equally adapted to the climatic conditions, may not be turned to some profitable account under the necessities of a future population; and whether, in time to come, some forms of orchard, vineyard, or tree-culture may not possibly be made to thrive in that region. He finds that the present plants on the whole are not lacking in expansion of foliage or succulence, at least that the more prevalent plants had an average of from 55 to 80 per cent. of foliage or working surface; and a series of rough but seemingly well-devised experiments demonstrated that they give off by evaporation daily an amount equal to three eighths of the weight of their available material. Dry as the soil appears to be, it is this, and not the atmosphere, that must furnish the supply to make good this loss. Yet water is rarely to be had under a depth of 100 to 300 feet, often not even at that depth. The porous soil must allow of the free upward diffusion of moisture, also of deep penetration of the roots from above.

An excellent map is given, exhibiting the district from above the 42d parallel to below the 39th, on which the routes of the three several years are traced in colored lines, and the mountain ranges with the general configuration of the surface represented. We will endeavor hereafter to review the systematic part of this work.

(Second Notice.) — Under the modest name of a catalogue of the known plants of Nevada and Utah, Mr. Watson has given us a treatise, not to say a Flora, of a wide stretch of country between the Sierra Nevada and the Rocky Mountains, which is invaluable to the botanist studying the plants of that region in herbaria, and still more to explorers on the ground, — of which we hope there may be many. For not only are new or revised species described, but all species not contained in the common eastern manuals, etc., which every collector is supposed to possess; the characters of western genera are appended in foot-notes, and synopses of recently elaborated genera — some of them reprints or translations of scattered

papers with corrections or additions, others original revisions by the editor himself — are added in an appendix, so as to afford every possible help to the student or collector who has not access to a full botanical library, and indeed most acceptable facilities to those few who have.

After thus calling attention to a volume of so much importance, we propose to restrict our comments to sundry details of criticism, or points of information, where opportunity occurs.

Under *Thalictrum Fendleri* some synonyms are adduced which are not all certain; as there is another Oregon species which has been confounded with *T. dioicum*, but is distinct from both in the fruit, which was sparingly collected in the British Boundary expedition, and lately by Mr. Hall.

Ranunculus alismæfolius var. *montanus* is essentially equivalent to the variety *alismellus* Gray; although the specimens from the "head of Provo River in the Uintas" are a stouter and larger-flowered form, identical with Parry's No. 79, which we had wrongly named when distributed, and which may be rightly characterized as merely a dwarf mountain state of Geyer's *R. alismæfolius*. We may now add that there is a much older name for this species, especially for this mountain form of it, namely, *R. Pseudo-Hirculus* of Schrank, 1842, a Songarian plant. It may also be noted that, while this species in eastern America takes the place of the European *R. Flammula*, both occur on the western side of the continent (as also in Siberia), and in forms so much alike that only the character of the style and that of the petal and its scale (so well indicated by Mr. Watson) will serve to distinguish them. Of amply developed *R. Flammula* — as large as any European form — copious specimens have been collected in Oregon last year, by Mr. Elihu Hall, and are soon to be distributed.

As to *R. fascicularis*, there is no clear evidence that this species extends to California, Nevada, or even to Oregon. The plant referred to and so named in Lyall's collection, though not in fruit, is apparently *R. orthorhynchus*, a plant most rare in collections, but now, thanks to E. Hall's collec-

tion of last summer, likely to be supplied to botanists. As to Mr. Watson's *R. orthorhynchus* var. *alpinus*, that is certainly not of this species, but a wholly new one, unless it be the rare, and to us obscure, *R. pedatifidus* of Smith, or at least of Hooker. For since Schlechtendal's plant of that name has been referred to *R. affinis*, the Siberian one of Smith may also be of that species.

A yellow-flowered Aquilegia, with flowers rather smaller and sometimes much smaller than those of *A. Canadensis*, and with spurs shorter than the widely spreading sepals, after the manner of *A. formosa*, and more or less curved (thus approaching the European type), which has been collected by Lyall, Bourgeau, and others, is now characterized as a new species, under the name of *A. flavescens*, Watson. It should be noted that this has been cultivated in European gardens, from seeds collected by Rœzl, under the name of *A. aurea*, but it is doubtful if it is yet published under that name.

The *Cruciferæ* constitute an important order in the interior basin and its borders. One of Mr. Watson's most notable discoveries is that of Brown's *Parrya macrocarpa*, hitherto found only on the Arctic coast. It was detected on the highest peak of the Uintas, at an altitude of 12,000 feet. The next point of interest is found in our author's discoveries and views of plants of the Streptanthus and Thelypodium type. Two or three well-marked new species are introduced, and Nuttall's obscure *Streptanthus cordatus* is confidently identified. In the present view this is the only Streptanthus of the collection; Mr. Watson, having ascertained that several species, such as *S. procerus*, the curious *S. crassicaulis*, and two new species, have oblong seeds in a terete elongated pod, and cotyledons inclining to be incumbent in the manner of Thelypodium, combines them into his new genus, Caulanthus. And Iodanthus, with a few other species, some of which had already been excluded from Streptanthus, are referred, as had also been tentatively suggested, to Thelypodium. Which is all to be highly approved, except, perhaps, the expediency of the new genus, when all could be disposed in the two genera: Streptanthus for the species with flat or flattish pods, flat

seeds, and truly accumbent cotyledons; Thelypodium, for those with more or less terete pods, narrow seeds, and more or less incumbent cotyledons.

We are bound, moreover, to take steps for the suppression of a nominal species which is here introduced in consequence of our own short-sightedness. In an evil moment we gave the name of *Smelowskia ? Californica* to a plant of Professor Brewer's collections, thought to be perennial, with exceedingly short few-seeded pods. This Mr. Watson identified with a common Sisymbrium of the region, distinguished from *S. canescens* by its seeds, strictly in a single series, and transferring the name, calls it *S. Californicum*. He had overlooked an article in this Journal (for September, 1866) upon this Sophia group of Sisymbrium, from which it would have been seen that the plant in question is *Sisymbrium incisum* of Engelmann, and the later *S. longipedicellatum* of Fournier, besides one or two other names of the same author more or less strictly referable to it.

DECAISNE'S MONOGRAPH OF THE GENUS PYRUS.[1]

A VOLUME of Decaisne's great work — or rather of one of his great works — "Le Jardin Fruitier du Muséum, un Iconographie de touts les Espèces et Varietes d'Arbres Fruitiers cultives dans cet Etablissement," etc. (produced in first-rate style by Firmin Didot Frères), devoted to the genus Pyrus, is now before us. It is a complete monograph of the species of this genus, taken in its restricted sense, illustrated by figures of the wild types, and also of the cultivated races of those cider-pears known in France under the name of *Sauger*. There is a list of the cider-pears cultivated in the different provinces of France, a general alphabetical catalogue of all the published varieties of pears, and a table in which the synonyms are referred to the names severally adopted. The other volumes, and the illustrations of the edible varieties of

[1] American Journal of Science and Arts, 3 ser., iv. 489; x. 481.

pears, may have more interest for the horticulturist. But the present attracts the special attention of the scientific botanist.

As stated in the Introduction, Professor Decaisne entered upon his great undertaking more than twenty years ago, when, in the year 1850, he became the Professor of Culture. He cites the instructions under which the separate collection of fruit-trees was constituted, and the professor of culture was charged with its management, and was directed to bring together all the known varieties, with all their names, "afin d'établir une uniformité de nomenclature necessaire pour toutes les parties de la République." This is a decree of the National Convention, June 10, 1793. The collection which Decaisne has so diligently and acutely studied actually dates from the year 1792, when the fruit-garden of the Chartreux of Paris was broken up, and two trees of each variety transported to the Jardin des Plantes. In 1793 it contained 185 varieties. In 1824, when Thouin died, there were in it 265 varieties of pears alone; it has now more than 1400 varieties of this fruit. It is interesting and important to know that the collection still preserves the greater portion of the very types described a century ago by Duhamel. For seven years Professor Decaisne studied the incomparable collection under his charge, making drawings and analyses, in which he is so skilful, and an herbarium of their flowers and foliage, before he commenced the publication of the "Jardin Fruitier du Muséum," which he is now bringing to a close.

As to giving a correct nomenclature and available characters, this is difficult enough, as all botanists know, for the species themselves (which must needs have, or be assumed to have, real distinctions) in any large genus, such as Quercus, Rosa, Rubus, and the like; how much more difficult, even to impossibility, it must be in the case of cultivated varieties, of ever increasing numbers, usually named without system, sometimes of mixed origin, and often too like each other to be distinguished by any available descriptions. Here colored plates are a necessity; and those of this great standard work, upon which no pains have been spared, leave little to be desired that art can supply.

In France alone they count about 800 sorts of pears; the origin of most of them is unknown, and many are undoubtedly very ancient. Indeed, according to Jordan and his school these differences are primitive, and the so-called races and varieties, both of wild and cultivated plants, represent so many closely related species. But M. Decaisne, not content with the *reductio ad absurdum* of having about 2000 species of pears to be dealt with, proceeded to an experimental demonstration of the variability of the cultivated races. He sowed the seeds from four very distinct varieties in 1853, the Poire d'Angleterre, the Bosc, the Belle Alliance, and the Cirole. Of the last the four trees raised bore fruit of four different forms. From the Belle Alliance he obtained, in this first generation, nine new varieties, none of them representing the parent, neither in the form, size, color, nor even the time of ripening of the fruit. The Bosc equally produced new varieties. Of the Angleterre nine trees produced as many new forms, one of them a winter-pear similar to the Saint Germain, another apple-shaped fruit identical with one which was raised from the Belle Alliance. On plate 33, Decaisne gives figures of six different pears raised from the Angleterre. These results even led him to doubt the cases cited by Darwin of the reproduction of certain pears from seed. He insists, moreover, that very bad fruits may be raised from choice cultivated pears, and that good varieties may be obtained from the seeds of wild pears. The latter is not what one would expect in the first generation.

Our author proceeds to state that the trees raised from seed taken from the same fruit differed, not merely in their fruits and in the time of ripening, but no less in their flowers and in the form of the leaves. Some were thorny, others thornless; some produced slender shoots, others thick and stout shoots, etc. It is worth noticing, however, that no mention is made of any precautions to prevent cross-fertilization of the flowers from which the seeds planted were derived, which might have influenced the product through the now well-ascertained influence of the pollen upon the pericarp. We perceive, however, that he would regard this as unimportant,

since pear varieties are of the lowest grade, incapable of propagating fruit by close-fertilization, and therefore wholly unlikely to impress by their pollen any characteristic upon the pericarp of another variety.[1] A large part of the Introduction is occupied with further evidence that the Pear-trees of cultivation are all of one species, from which have proceeded six races, completely fertile *inter se*, and varieties *ad infinitum*. In this respect the Pear-tree has but followed the example of most fruit and fruit-trees, and of the Grains, etc., which had apparently diverged into races, or distinct but closely related types, in very early times, and those under cultivation have themselves varied and subdivided more and more. Finally, M. Decaisne maintains, seemingly with good reason, that to combine into one genus the Apple, Pear, Quince, Sorb, and Mountain Ash, as done by Linnæus and followed by the latest authorities, is to misconceive the laws of the natural system; that "to unite generically these plants, which differ in the character of their wood, the vernation of their leaves, their inflorescence, the æstivation of the corolla, and the structure of their fruit," logically leads to the combination of all *Pomaceæ* into one genus. He accordingly restricts the genus Pyrus, or (restoring the classical orthography) Pirus, as did Tournefort and Jussieu, to the Pear proper. To the organography of this restricted genus, from the wood to the embryo, a full chapter is devoted. In the course of this the relative systematic value of characters observed is brought out. He

[1] Yet the Apple, which is in the same case, does so. An interesting instance of this kind lately came under our notice, an apple from a Spitzenberg tree, one half (at least as to the surface) *Spitzenberg*, the other half *Russet*. A tree of the latter fruit stood about 200 yards off. Several cases of this sort are known, in which, as in this, the division is into two exactly equal parts of the circumference, and the line of demarcation abrupt. This is quite unexpected, as the Secretary of the Smithsonian Institution, who sent us the fruit, remarked; for as the styles and carpels were five, we should have expected the division to be into fifths, and according to the number of the stigmas which were acted upon by the foreign pollen. It is, moreover, to be noticed that the action of the pollen in this case is manifest upon what is morphologically the calyx, not upon the pericarp. The apple we refer to was grown in the orchard of William Wicksham, of Washington County, Pennsylvania. — A. G.

notes that the vernation of the leaves is involute in Pyrus, but not in Cydonia, Mespilus, and Aria; that the cottony-leaved varieties, no less than the smooth ones, are glabrous in the seedling stage; that all varieties of the common Pear blossom at Paris whenever, in the month of April, the mean temperature reaches about 10° Centigrade, without perceptible difference between the earliest and the latest-ripening varieties; that the æstivation of the corolla is convolute in Cydonia, but imbricate in the Pear, although ordinarily quincuncial in other *Pomaceæ* (but in the two diagrams of Pear-flowers on Plate A, one has the quincuncial, *i. e.*, in our view typically imbricative æstivation of the corolla; in the other, there is only one wholly outer and one inner petal, — a combination of the quincuncial and the convolute modes which often occurs, but which need not be taken as the type of imbrication); that there are two types as to size of the corolla in the common Pear, the smaller flowered type comprehending most of the cultivated varieties; that the odor of Pear-blossoms is rather disagreeable than otherwise, in contrast with those of Malus, which are sweet-scented. Moreover, the anthers in the Pear genus are tinged with violet; those of the Apple genus are yellow.

As to the morphology and development of the gynæcium, Decaisne reproduces in full the note which he published in the "Bulletin of the Botanical Society of France" in 1857. From his investigation it appears that the five carpels in their early development are free and distinct in the concave centre of the flower; that at a later stage, when the concave receptacle has become much deeper, a cellular tissue develops from its base and inner face, moulds itself around and over the carpels, so as separately to envelop them, except at their inner angle, while it carries up the petals and stamens, and forms the perigynous disk upon which they are inserted; this forms the *core* or central part of the flesh of the fruit, which we have always regarded as receptacle, never ceasing to protest against the still prevalent notion (continued in the latest general works), that the cartilaginous or bony "cells" are "endocarp." But, while we were disposed to regard the whole

exterior flesh as calyx, Professor Decaisne (no doubt correctly) regards it as mainly receptacle or axis, an *hypanthium* which in common pears is largely a hypertrophy of the peduncle, after the fashion of Anacardium.

In the proper Pear genus, the ovules never exceed a single pair; this should therefore enter into the generic character.

"Theophrastus had already remarked that the older the Pear-tree, the more prolific, and every day's experience confirms the justice of this observation." The gritty grains or lignified cells which are so abundant in the flesh of many sorts of pears are not wholly absent from any of them. To them is due the roughish surface of the skin, as contrasted with the smooth skin of apples. It is curious to remark that Meyen, in his "Pflanzen-Pathologie," considered the gritty grains to be a disease which attacked pears and quinces.

It appears that pear-growers are able to produce fruits of abnormal size by supporting the growing pear from underneath, instead of allowing it to hang on the peduncle. M. Decaisne has seen Poires de Livre of a kilogram, Gloumorceau of 600 grams, and a Chaumontel of 700 grams weight, produced in this way.

The testa of all Pomaceous seeds is smooth and more or less mucilaginous, except of a Photinia, in which it is reticulated. The cotyledons are accumbent relative to the rhaphe, except in a Photinia, Cotoneaster, Pyracantha (*Cratægus Pyracantha*, Pers.), and Eriobotrya, in which they are incumbent. At first there is a thin layer of albumen, which disappears at maturity of the seed.

Pears are commonly grafted upon a Quince stock. But it is confidently asserted, and generally supposed, that there are more than forty varieties which absolutely refuse this union, and which are therefore managed by subgrafting upon a Pear stock of a proper sort which has itself been engrafted upon the Quince. But, as Professor Decaisne remarks, horticulturists are too apt to generalize their impressions and to limit nature to the narrow horizon of their own practice. Upon the first trial of the experiment under his own observation, he succeeded with twenty of these antipathetic varieties without

the least difficulty; but some (among which are the Clairgeau and the Bosc) obstinately refuse to unite with the Quince stock. He naturally discredits the assertion made by Cabanis and by Downing (cited by Darwin), that when certain pears are grafted on the Quince, their seeds produce trees of types different from those which they do when they are raised upon a Pear stock. Decaisne found, as already stated, that Pear-seeds produce indifferently new varieties in any case; that these varieties are not at all fixed into races. He regards as wholly unproven all the assertions that the fruit is ameliorated or in any degree altered by grafting upon a Quince or any other stock. He records a very exceptional instance in which the antipathy of the Pear to the Apple as a stock was so far overcome that the graft survived at least six years, but without vigor, and bore fruit; still this antipathy confirms the generic difference between Pyrus and Malus.

We must pass over the sections on the diseases of the Pear, and the parasitic plants and insects hurtful to it; while as to that on the classification of the Pears of cultivation, we may mention merely the conclusion, which is, that a natural classification of Pears is thus far an impossibility; and that in practice nothing better can be done than to follow the example of the older pomologists, who arranged them according to the period of ripening. A general list of the adopted names of the published varieties of cultivated Pears, alphabetically arranged, fills four pages of the volume. A list of their synonyms, in which each is referred to the adopted name, fills over 12 pages! Then follows a list of pears classed according to the period of maturing, and in which the best varieties are designated.

Finally comes a botanical monograph of the genus Pyrus, with a full generic character, and descriptions and figures of the *races*, as he would term them, considering as he does all known forms of the restricted genus as a single and very polymorphous species.

The six races are: 1. The Celtic, *Proles Armoricana*, of three quasi-species, *P. cordata*, *Boissieriana*, and *longipes*. 2. The Germanic, *Proles Germanica*, or *Pyrus communis*,

including our common pears, both pear-shaped and apple-shaped, "both forms being often met with upon the same tree." Under this head Professor Decaisne gives some interesting pages upon the history of the cultivation of pears in France, which cannot be ancient, and of cider (perry) as a drink. It appears that it took the place of beer in the north of France in the fifteenth century or later, and is now giving way to wine and perhaps beer again; and that pears would have disappeared before this from a part of Normandy, were it not that they are carried in immense quantities to Epernay, where they are used in the manufacture of champagne. 3. The Hellenic Race, which comprises *P. parviflora* and three other subspecies. 4. The Pontic Race, *P. salicifolia* and its allies. 5. The Indian Race, *P. Pashia* and its relatives. 6. The Mongolian Race, *P. Sinensis* and its varieties. As one turns over the excellent plates one can hardly be persuaded that such extremely diverse forms can practically be regarded as of one species.

A list of the species remanded from Pyrus to other genera shows that the result of our author's prolonged and sagacious study is to increase the genera about as much as he diminishes the species of the Linnæan Pyrus.

A detailed analysis of Decaisne's monograph of the genus Pyrus was given in this Journal (3 ser., iv. 489, Dec., 1872). Some of the views taken in that work are fully expounded in the present paper,[1] which embracing the results of a prolonged study of an important group, by a botanist of great experience and ability, is worthy of particular attention. As the veteran author states it: —

"My principal object is here to call the attention of botanists to certain characters which have been neglected in systematic works, by the aid of which the ancient genera merged in Pyrus by most of the recent systematists may be neatly circumscribed. Such is the constancy and the value of these characters that the details of organization peculiar to each generic group may be expressed by very general propositions,

[1] *Memoire sur la Famille des Pomacées.* Par J. Decaisne (*Nouvelles Archives du Muséum*, x. pp. 113, 192). Paris, 1875.

which is the very object of a good method. Indeed, when a special organization is common to a large number of different plants, it is evident that comparatively slight but constant modifications of this structure ought to be particularly attended to; and this proposition seems to be especially true of the *Pomaceæ*." M. Decaisne puts foremost his strongest point when he declares of the Quince, that "the nature of its bark and wood, its prefoliation, inflorescence, the æstivation of the corolla, the structure of the ovary and of the fruit differ essentially from that of the Pears, among which certain botanists still class it." Rather than combine the Quince and the Japan Quince with Pyrus, we are confident that botanists will generally accept his Docynia, along with Chænomeles Lindl. and Cydonia, as independent genera. The same may be said of Mespilus; and it must be allowed that the character which Kunth had noticed and which Decaisne has turned to account, that of the deformation of one of the ovules which becomes a kind of stipitate hood for the other, being common to it and to Cratægus, indicates a relationship to the latter genus rather than to Pyrus. Much nicer and more questionable characters are assigned to the genera here re-established from Pyrus in the Candollean sense, to which we are in this generation accustomed. These are, Aronia, Pers., our Chokeberry (in which eight species are set up from what we take to be a single polymorphous one); Sorbus, Tourn., the Mountain Ash (the synonym *S. microcarpa* omitted from *S. Americana*, and *S. sambucifolia* is still taken to belong only to our western coast, whereas it extends across the continent); Aria, Host., the Beam Trees, all of the Old World; Torminaria, Roem., for *Pyrus torminalis;* also Cormus, Spach, for *Sorbus domestica*, L., the Service-tree of Europe, with *Pyrus trilobata*, DC., and an allied species; Micromeles, a new genus for four Himalayan species thus far little known; lastly, Malus, Tourn. Here it is to be observed that *M. diversifolia* is held to be distinct from *M. rivularis;* and that a subgenus, Chloromeles, proposed for *M. angustifolia*, our narrow-leaved Crab-Apple, thus widely separated from *M. coronaria*, on account, as is stated, of its reddish anthers and the structure

of the disk. Pyrus, Tourn., is thus brought down to the Pear; and this, as Decaisne had formerly announced, to a single collective species, of six geographical *proles* or forms. We continue to write Pyrus from old habit and custom, not doubting, however, that Pirus is the correct orthography.

Of Amelanchier, following Lindley, there are enumerated twelve species, six for the Old World and six for North America, and there are names for four more. Without being able to clear them up (and no wonder), Decaisne thinks that they may be distinguished into at least three groups, characterized by the distinct or united styles, and the glabrous or downy ovaries. We are continually impressed with the idea that there must be three or four American species, and the seeds may aid in their definition. But thus far the characters elude investigation. Peraphyllum, Nutt., referred by Bentham and Hooker to Amelanchier, has not been studied by Decaisne. When he examines the excellent specimens in flower and in fruit, which Mr. Siler has supplied from southern Utah, he will conclude that the genus must certainly be reinstated. The likeness is only in the peculiar structure of the fruit.

As respects the remaining genera, the difference between this monograph and the disposition in Bentham and Hooker's "Genera Plantarum" is mainly this: Eriobotrya, with its baccate fruit (what is termed endocarp reduced to a soft pellicle), large turgid seeds with thickened cotyledons, and undulate petals, is upheld as a good genus; Heteromeles is adopted from J. Rœmer for the Californian *Photinia arbutifolia*, and a second (probably not good) species, *H. Fremontiana*, is added. The characters appear to be the 10 instead of 20 stamens, in pairs opposite the calyx-lobes, their filaments dilated at base and somewhat monadelphous. In the tabular conspectus the petals are said to have "préfloraison tordue," but in the generic character it is "æstivatione imbricativa vel convolutiva," the latter term with the French botanists meaning the same as imbricated, only more enrolling. The diagram represents the whole five petals with one edge covered, *i. e.*, "tordue" or contorted (or, as we say, convolute), and so we

find them in all the flower-buds now examined. But before adopting the genus it may be well to examine the *Photiniæ* generally. Photinia, of which *P. serrulata* is the type, is characterized as having imbricative æstivation, and Decaisne's diagram represents it as regularly (*i. e.*, quincuncially) so. But in *P. prunifolia* and in *P. Blumei* we find occasionally only one exterior petal, and the four others successively overlapping in the "contorted" way; and in one of Wallich's specimens of *P. integrifolia* the first flower-bud inspected showed the "contorted" æstivation complete. This is also the case in *P. dubia* (in one of Hooker's and Thompson's Khasyia specimens), and this Decaisne refers to Eriobotrya, which has imbricative æstivation. Next is Pourthiæa, a new genus of eleven Japanese and Indian species, the type being *Photinia arguta*, *villosa*, *lævis*, etc., and the character, among others, "æstivatione contorta." But we as commonly find one petal wholly exterior. So we think it evident that the æstivation of the corolla furnishes no characters for the division of the genus Photinia.

Finally, as to the proper stone-fruited genera, Pyracantha is adopted from J. Rœmer for *Cratægus Pyracantha* and an allied Indo-Chinese species, and placed near Cotoneaster; and a character not before used is introduced, namely, the position of the cotyledons, which in this genus are, as regards the rhaphe, accumbent.

There are eight plates, six of them filled with admirable dissections, neatly done upon stone by Riocreux from the author's sketches.

ENGELMANN'S NOTES ON THE GENUS YUCCA.[1]

This modest title comprises the principal results of Dr. Engelmann's long study of a difficult genus of plants. Pursuing his botanical investigations now for many years only

[1] *Notes on the Genus Yucca.* By George Engelmann, in Trans. St. Louis Acad. iii. St. Louis, 1873. (American Journal of Science and Arts, 3 ser., vi. 468.)

in the intervals and spare moments of a busy and exacting professional life, Dr. Engelmann has made them tell most effectively and advantageously upon the science which numbers him as a distinguished votary, by taking up one subject at a time and investigating it as thoroughly as possible. In this way he has mastered, in turn, our *Cuscutæ* (upon which his earliest essay was published in this Journal, thirty-one years ago, and his latest was a full monograph of all the known species throughout the world), our *Cactaceæ*, our Mistletoes, Euphorbias, Junci, Callitriches, etc., not to speak of several other genera or groups, or taking account of his sedulous and long-continued study of our Oaks, and, above all, of our *Coniferæ*. Nor need we look to this as the close of the series, but rather see before him "fresh fields and pastures new," and wish for him more time to expatiate in them. Upon the principle "to him that hath shall be given," he well deserves it, as having accomplished far more in these rescued moments than others who could mainly devote their days as well as nights to scientific work. Almost without exception these monographs relate to difficult subjects, and such as require long-protracted investigation. This is also true of the present essay upon the genus Yucca. It is not a large one, only a dozen species being clearly made out; but those of long cultivation in Europe have been much confused, and recent ones described without flowers, while fruit is rarely formed out of their native stations, and dried specimens of any completeness are difficult to make, so that means of comparison are much restricted.

The true anthesis, as is now shown, is nocturnal, the flowers remaining half closed during the day. The anthers, with comparatively large and few grains of glutinous pollen, open rather earlier than the flower. The tips of the style, which were naturally taken for stigmas, are now shown to be functionless, the stigmatic surface being the moist and glutinous lining of a stylar tube, which extends downward nearly to the cells of the ovary and even communicates directly with them. As soon as it became evident that fertilization must depend upon nocturnal insects, it was found that they were most fre-

quently and regularly visited by "white moths, which, usually in pairs, disported in the open flowers at dusk, and were found quietly ensconced in them when closed in the day-time." Professor Riley of St. Louis, the distinguished entomologist, was at this point associated with Dr. Engelmann in the investigation. The result has been given to the scientific world in his interesting memoir on *Pronuba yuccasella*, first read at the Dubuque meeting of the American Association for the Advancement of Science, in August, 1872, and now also published, as a sequel to Dr. Engelmann's monograph, in the "Transactions of the St. Louis Academy."

"The rootstock of all the Yuccas is, under the name of 'Amole,' an important article in a Mexican household, being everywhere used as a substitute for soap, as it is replete with mucilaginous and saponaceous matter, probably a substance analogous to the saponine of the Saponaria root. It is curious to learn that the negroes of the coast of South Carolina repeatedly destroyed Dr. Mellichamp's carefully observed clumps of Yuccas, in order to obtain the saponaceous rootstock." In Colorado Territory we found that *Yucca angustifolia* is as generally called "Soap-plant" as is the Chlorogalum in California.

While the nature of the fruit, whether capsular or baccate, is a tribal character in *Liliaceæ* generally, Yucca has both kinds; and Dr. Engelmann turns this character, with accompanying differences in seeds, to good account in the systematic arrangement of the species. The common "Spanish bayonet," *Y. aloifolia* of the Southern States, and some related Texan and Mexican species, represent the pulpy-fruited section; *Y. brevifolia*, which ranges across the Arizonian border of the United States, has a spongy indehiscent pod, probably at first more or less drupaceous; while the Bear-Grass, *Y. filamentosa*, and its allies bear a dry capsule. It may here be recorded that the name *Y. canaliculata* of Hooker must replace that of *Y. Treculiana* of Carrière, the latter being a name published without characters, in 1858, the former described and figured in 1860, in the "Botanical Magazine." The prince of Yuccas must be *Yucca baccata*, which, in its

variety *australis*, forms "trees twenty-five to thirty feet high, and two or three feet in diameter, with ten or a dozen branches," or sometimes reaches to even fifty feet of elevation according to the late Dr. Gregg, although the most northern form of it is almost stemless. Its pulpy fruits are "savory, like dates," are eaten fresh by both whites and Indians, and are cured by the latter for winter provisions. They also make a stew of the flower-buds and flowers, which Dr. Palmer found to be pleasant and nourishing. The seeds are said to be actively purgative. The fibers of the leaves are used for cordage, the trunks for palings, or are riven into slabs for the coverings of huts, and the tender top of the stem is roasted and eaten.

Professor Riley's curious paper upon the mutual relations of Yuccas with Pronuba, a Tineideous moth that does the work of pollination, will be read with interest.

RUSKIN'S PROSERPINA.

Mr. Ruskin, "having been privileged to found the School of Art in the University of Oxford," now proposes to found a new school of botany.[1] Of course, it will be a vagarious school. One crying evil to be remedied is "that there are generally from three or four up to two dozen Latin names current for every flower," and "the most current and authoritative names" are "of the devil's own contriving." This is not seemly. As Wesley would not allow the devil to have the singing of all the good tunes, so neither will Ruskin allow him to have the naming of all the sweet flowers. He proposes "to substitute boldly . . . other generic names for the plants thus faultfully hitherto titled." He "will not even name the cases in which they have been made," but will "mask those which there was real occasion to alter by sometimes giving new names in cases where there was no necessity of such kind."

[1] *Proserpina*, Studies of Wayside Flowers, etc. By John Ruskin. London and New York, 1875. (The Nation, No. 528, August 12, 1875.)

That is to say, the evil of a redundancy of botanical names is to be overcome by making more, some of them avowedly needless; and innocent names are to suffer, lest bad ones should become notorious by being discarded without company. For it appears that the diabolical names to be sent to their own place in this reform are not discarded because they are cacophonous, although that is the common charge, but because they are immoral. Of the two evils to be dealt with, the first is simply a superfluity of Latin names; the second, and the worse, a superfluity of naughtiness. As Mr. Ruskin forcibly puts it: —

"The second, and a much more serious one, is of the devil's own contriving (and, remember, I am always quite serious when I speak of the devil), namely, — that the most current and authoritative names are apt to be founded on some unclean or debasing association, so that to interpret them is to defile the reader's mind."

This reminds us of the fine lady who thanked Dr. Johnson for omitting indelicate words from his dictionary, to whom the blunt moralist rejoined: "I perceive, madam, that you have been looking for them." Now, if the case be really as it is represented, the sound practical axiom, "Quieta non movere," would seem to suggest the proper treatment; for his purposes, one would think the Latin and Greek names might be left untranslated; and the reform might begin, and end, with the popular English names, — almost the only ones the author may need to use, — some of which are coarse and vulgar enough. Indeed, as to botanical generic names, far from finding any confirmation of Mr. Ruskin's sweeping charges, we can recall barely one or two the translation or etymology of which would be embarrassing at the parlor-table. Moreover, if the following passages really refer to "nomenclature" (though terminology seems to be meant), our author, upon his own showing, need not waste his time in the endeavor to reform it: —

"The mass of useless nomenclature, now mistaken for science, will fall away, as the husk of a poppy falls from the bursting flower. . . . When the science becomes approxi-

mately perfect, all known plants will be properly figured, so that nobody need describe them, and unknown plants will be so rare that nobody will care to learn a new and difficult language in order to give an account of what in all probability he will never see."

Well, for that matter, the English Botany, in its various editions, furnishes fairly good figures of all British plants; and the "Botanical Magazine" — a page of an early number of which, eighty years old, is gibbeted by Mr. Ruskin — has gone on to figure more than 6000 cultivated exotics, and is continuing at the rate of nearly a hundred a year; so that our author's ideal is practically all but realized already. There are wellnigh pictures enough, if one knows how and where to find them. And it amusingly appears, from Mr. Ruskin's trouble with St. Bruno's Lily at the beginning, and from his investigation of moss further on, as well as from scattered statements, that his mode of proceeding in systematic botany is the simple one of searching high and low for a picture to match the specimen in hand. Accordingly, it is not surprising that his "botanical studies were, when [he] had attained the age of fifty, no further advanced than the reader will find them in the opening chapter of this book."

As to this, the conclusions which the reader will draw are all along anticipated by the author. Next to the pervading — well — bumptiousness, nothing is so prominent in the book as the profession, not to say the parade, of ignorance of the topics treated. As to "the elements of the science of botany," "I can scarcely say that I have yet any tenure of it myself." "And, meanwhile I don't know very clearly so much as what a root is or what a leaf is." "Some one said of me once, very shrewdly, When he wants to work out a subject, he writes a book on it. . . . This book will be nothing but processes. I don't mean to assert anything positively in it, from the first page to the last. Whatever I say is to be understood only as a conditional statement — liable to and inviting correction. And this the more because, as on the whole I am at war with the botanists, I can't ask them to help me, and then call them names afterwards." So "for many reasons, I am forced to

print the imperfect statement, as I can independently shape it." To get at the facts, "I should have to write a dozen of letters before I could print a line, and the line at last would be only like a bit of any other botanical book — trustworthy, it might be perhaps, but certainly unreadable." The converse is preferred. Readable it certainly is, and in its way interesting, not so much for what it tells about botany as for what it tells about Mr. Ruskin; and the art student, out of the abundance of golden chaff, may pick some grains of knowledge that might not otherwise fall in his way. But the seeker of botanical information must glean warily, especially where the author grows positive. For almost the only instance in which he does pronounce decidedly happens to be a vexed question in vegetable physiology, and there is reason to fear that he decides it wrongly. At least the recent investigators who have had the matter in hand in the way of experimental inquiry, will not agree with him that the plant can get water from the atmosphere directly and "for the most part does so; though when it cannot get water from the air, it will gladly drink by its roots." Still "our natural and honest mistakes will often be suggestive of things we could not have discovered but by wandering." Very likely; but why invite learners to go forth with him upon his wanderings? In many a book the want of sufficient knowledge is pleaded as an excuse; in this, it is paraded as a recommendation. Ignorance, no doubt, has its uses; but it is questionable whether teaching is altogether the best use to put it to. As the member of Parliament who yawned desperately while delivering his speech was thought to trench upon the privilege of his hearers, so the students of "Proserpina" may complain that the playing of the rôle both of teacher and learner at once involves some incongruity and inconveniences.

The second part of "Proserpina" has just come to hand. It treats of the leaf and the flower, in a discursive and oracular way, leading into æsthetical questions, where we need not follow and do not greatly admire. Now and then a scientific topic is taken up, and the point missed, as usual. Treating of foliage and its office, we are bid "to think awhile of its dark

clear green, and the good of it to you." We look for an exposition of the fact, in which the whole meaning of vegetation inheres, that leaves under the sun's influence create all the food of the world, and are therefore the basis of all animal existence. Instead of which we have: —

"Scientifically, you know, green in leaves is owing to 'chlorophyll,' or, in English, to 'green leaf.' It may be very fine to know that; but my advice to you, on the whole, is to rest content with the general fact that leaves are green when they do not grow in or near smoky towns, and not by any means to rest content with the fact that very soon there will not be a green leaf in England, but only greenish-black ones. . . . Well, this much the botanists really know and tell us" — that vegetation "is made chiefly of the breath of animals. . . . So that you may look upon the grass and forests of the earth as a kind of green hoar-frost, frozen upon it from our breath, as, on the window panes, the white arborescence of ice."

Mr. Ruskin evidently has no idea of the essential independence of the vegetable kingdom; that, as all the carbon of the breath of animals comes from plants, so they, in their decay, would furnish this material for succeeding vegetation perhaps as rapidly, on the whole, without the intervention of animals. At most, the latter somewhat expedite the decomposition.

"But how is it made into wood?" As to that and matters therewith connected, "under the impression that it had been ascertained, and that I could at any time know all about it, I have put off till to-day the knowing of anything about it at all. But I will really endeavor now to ascertain something, and take to my botanical books accordingly."

Behold the result of the cram, "the gist of the matter": —

"Hence generally, I think we may conclude thus much, that at every pore of its surface, under ground and above, the plant in the spring absorbs moisture, which instantly disperses itself through its whole system 'by means of some permeable quality of the membranes of the cellular tissue invisible to our eyes even by the most powerful glasses'; that in this way subjected to the vital power of the tree, it becomes sap, prop-

erly so called, which passes downwards through this cellular tissue, slowly and secretly; and then upwards, through the great vessels of the tree, violently, stretching out the supple twigs of it as you see a flaccid water-pipe swell and move when the cock is turned to fill it. And the tree becomes literally a fountain, of which the springing streamlets are clothed with new-woven garments of green tissue, and of which the silver spray stays in the sky — a spray, now, of leaves."

Then as to the blossom: "The flower exists for its own sake, not for the fruit's sake. . . . But the flower is the end of the seed, not the seed of the flower." "The corolla leads and is the object of final purpose. The stamens and the treasuries [Mr. Ruskin's new term for pistils] are only there in order to produce future corollas." Without criticising anybody's notion of final causes, we only notice how Mr. Ruskin fails to make his own point. He has seen "among the speculations of modern science, several, lately, not uningenious, and highly industrious, on the subject of the relation of color in flowers to insects, to selective development," etc. And he proceeds to intimate that even Mr. Darwin must be ranked among "the men of semi-faculty or semi-education who are more or less incapable of so much as seeing, much less thinking about color," etc., referring merely to the latter's speculations upon the ocelli of the Argus Pheasant, in blissful ignorance, it would seem, that he has to deal with Mr. Darwin upon this very subject of color and use in flowers, and that he is not prepared even to state his own side of the question.

EMERSON'S TREES AND SHRUBS OF MASSACHUSETTS.

Tree-lore[1] is no longer confined to the few, and books like this address a large and various audience, or will do so when they become better known. Mr. Emerson's original volume

[1] *A Report on the Trees and Shrubs of Massachusetts*, growing naturally in the forests of Massachusetts. By George B. Emerson. 2d ed. Boston, 1875. (The Nation, No. 539, October 8, 1875.)

was published twenty-five years ago, "agreeably to an order of the Legislature, by the Commissioners on the Zoölogical and Botanical Survey of the State" of Massachusetts, being a supplement to the Geological Survey of that period, prosecuted under Edward Everett's governorship. It was the most popular report of the series, and the edition was ere long exhausted. When this came to pass — without waiting for the new survey which the State last year came near authorizing but failed to do so — Mr. Emerson, unassisted, set about the preparation of a new edition, devoted several years to it, and to the study of what had been done for the preservation and utilization of forests in the Old World, and for their waste and destruction here in the New; and he has at length brought out this second edition, in two goodly octavo volumes, illustrated and adorned by a large number of well-executed plates. These being interspersed through the pages, unnumbered, and nowhere enumerated, the only way of ascertaining their actual amount was to count them. We find 144 plates, of various kinds and merits. The least satisfying to us are those of portraitistic or scenic character, borrowed from the German "Der Wald" and the very French "Vegetable World" of Figuier; yet to others these may be the most attractive. Very good, though unpretending, are figures, mainly in outline, contributed by Mr. Isaac Sprague to the first edition, here reproduced. Best of all are those contributed by the same hand to the new edition, original figures of the foliage, flowers, and fruit of many of our trees and shrubs not before illustrated, transferred from Sprague's drawings to stone, and printed in colors. The plates representing our two northern Azaleas, the Roxbury Waxwork (as they name it around Boston), the Virginian Creeper in its autumn dress; the Red Maple, both in vernal and autumnal robes; and the Flowering Raspberry, from which seemingly one may almost shake the mountain dew, are good illustrations of what may be done in this way. Hand-coloring is too expensive, and chromo-lithography can really be turned to excellent account in its place for natural-history illustrations, whatever be its merits or demerits in other regions of art. The letter-press

we find is a reprint of the first edition, as to the descriptive part. The popular descriptions seem to have hit the mark.

Mr. Emerson's instructions and appeals for the planting and care of trees, and for the renovation of our woods, wherever practicable and profitable, are worthy of all attention, as his efforts in this regard are worthy of all honor. They began long ago, and have been redoubled now in his later years, in this work and elsewhere. The memorial addressed to the President and Congress by the American Association for the Advancement of Science he took a large part in preparing; and his personal furtherance of it at Washington, of which he makes modest mention in the preface, may yet be fruitful of benefit. It may not be improper to add that the only permanently endowed arboretum in America — the Arnold Arboretum, entrusted to Harvard University — owes its existence to our author's thoughtfulness and sense of the importance of tree-culture.

DARWIN'S INSECTIVOROUS PLANTS.[1]

This long expected work appeared last autumn, was immediately reprinted by the American publishers, and before this time has been so widely read that no detailed account of it is at all necessary. Its main topic is Drosera or Sundew, upon which the vast number and diversity of the observations and experiments — at once simple, sagacious, and telling — which it records, are about as wonderful as the results. As to the latter, it is established beyond question that the common Sundews are efficient fly-catchers; that the stalked glands, or tentacles as Mr. Darwin terms them, are sensitive and turn inward or even in other required directions in response to irritation; that they equally respond and move in obedience to a stimulus propagated from a distance through other tentacles and across the whole width of the leaf; that the sensi-

[1] *Insectivorous Plants.* By Charles Darwin. London and New York, 1875. (American Journal of Science and Arts, 3 ser., xi. 69.)

tiveness belongs only to the glands and tips of the tentacles, but is propagated thence down their stalks and across the blade of the leaf through the cellular tissues; that they accurately and delicately discriminate animal or other nitrogenous matter from anything else; that the glands absorb such matter; that when excited by contact, or by the absorption of nitrogenous matter by the viscid enveloping liquid, an acid secretion is poured out and a ferment analogous to pepsin, the two together dissolving animal matter; so that the office and action of these glands are truly analogous to those of the glands of the stomach of animals. Finally, that animal or nitrogenous matter, thus absorbed and digested in the glands, is taken in, and conveyed from cell to cell through the tentacles into the body of the leaf, was made evident by ocular inspection of the singular changes in the protoplasm they contain. So particularly have the investigations been made and so conscientiously recorded, that the account of those relating to one species of Sundew, *Drosera rotundifolia*, fills 277 pages of the English edition, or more than half of the book. After all it ends with the remark: "and we see how little has been made out in comparison with what remains unexplained and unknown." The briefer examination of six other Sundews follows, some of them equally and others less efficiently fly-catchers and feeders.

Dionæa is next treated, but with less detail. Indeed, except as to the particular nature of the secreted digesting fluid, there is little in this chapter that had not been made out or already become familiar here. That the secretion has digestive powers, and that it is reabsorbed along with whatever has been digested, is now proved beyond reasonable doubt. That the motor impulse is conveyed through the cellular parenchyma, and not through the vascular bundles, or spiral vessels, and that the latter do not originate the secretion, as Rees and Wills in a recent paper seem to suppose they must, appears to be shown by the facts, and was antecedently probable. "The wonderful discovery made by Dr. Burdon Sanderson is now universally known: namely, that there exists a normal electrical current in the blade and footstalk, and that when

the leaves are irritated the current is disturbed in the same manner as takes place during the contraction of the muscle of an animal." The conclusion here needs to be checked by parallel experiments, to see whether the same reversion of current does not take place whenever a part of any leaf or green shoot is forcibly bent upon itself.

Aldrovanda vesiculosa, of the Drosera family, "may be called a miniature aquatic Dionæa;" for, as discovered by Stein in 1873, "the bilobed leaves open under a sufficiently high temperature, and when touched suddenly close." Being submerged, their prey is confined to minute aquatic animals. For want of proper material and opportunity, Mr. Darwin was able to follow up only for a little way the observations of Stein and Cohn, — enough, however, to show that it also captures and consumes animals, but perhaps avails itself of the nitrogenous matter only when passing into decay.

Drosophyllum, a rare representative of the order, confined to Portugal and Morocco, grows on the sides of dry hills near Oporto; so that, as to station, it is the very counterpart of Aldrovanda. Its leaves are long and slender, in the manner of our *Drosera filiformis*, and are covered with much larger glands. To these, flies adhere in vast numbers. "The latter fact is well known to the villagers, who call the plant the 'fly-catcher,' and hang it up in their cottages for this purpose." Mr. Darwin found the glands incapable of movement, and their behavior in some other respects differs from that of Drosera; but they equally secrete a digestive juice. Insects usually drag off this secretion instead of being fixed on the glands by it; but their fate is no better; for as the poor animal crawls on and these viscid drops bedaub it on all sides, it sinks down at length exhausted or dead, and rests on a still more numerous set of small sessile glands which thickly cover the whole surface of the leaf. These were till then dry and inert, but as soon as animal matter thus comes in contact with them, they also secrete a digestive juice, which, as Mr. Darwin demonstrated, has the power of dissolving bits of coagulated albumen, cartilage, or meat, with even greater readiness than that of Drosera.

Mr. Darwin next records various observations and experiments upon more ordinary glandular hairs of several plants. To certain Saxifrages his attention was naturally called, on account of the presumed relationship of *Droseraceæ* to this genus. He declares that "their glands absorb matter from an infusion of raw meat, from solutions of nitrate and carbonate of ammonia, and apparently from decayed insects." To such plants the vast number of little insects caught may not be useless, as they may be to many other plants (Tobacco, for instance) with sticky glands, in which Mr. Darwin could detect no power of absorption. The prevalent idea, that glandular hairs in general serve merely as secreting or excreting organs, and are of small or no account to the plant, must now be reconsidered. Those of the common Chinese Primrose (*Primula Sinensis*), although indifferent to animal infusions, were found to absorb quickly both the solution and vapor of carbonate of ammonia. Now, as rain-water contains a small percentage of ammonia, and the atmosphere a minute quantity of the carbonate or nitrate, and as a moderate-sized plant of this Primrose was ascertained (by estimate from a count on small measured surfaces by Mr. Francis Darwin) to bear between $2\frac{1}{2}$ to 3 millions of these glands, it begins to dawn upon us that these multitudinous organs are neither mere excrescences nor outlets, nor in any just sense insignificant.

Mr. Darwin next investigates the densely crowded short glandular hairs, with their secretions, which form the buttery surface of the face of the leaves of Pinguicula, the Butterwort. He finds that the leaves of the common Butterwort have great numbers of small insects adhering to them, as also grains of pollen, small seeds, etc.; that most substances so lodged or placed, if yielding soluble matter to the glands, excite them to increased secretion; but that if non-nitrogenous the viscid fluid poured out is not at all acid, while if nitrogenous it invariably has an acid reaction and is more copious; that in this state it will quickly dissolve the muscles of insects, meat, cartilage, fibrin, curds of milk, etc.; that when the surface of a plane leaf is fed, by placing upon it a row of flies along one margin, this margin, but not the other, folds

over within twenty hours to envelop them; and when placed on a medial line, a little below the apex, both margins incurve. He concludes "that *Pinguicula vulgaris*, with its small roots, is not only supported to a large extent by the extraordinary number of insects which it habitually captures, but likewise draws some nourishment from the pollen, leaves, and seeds of other plants, which often adhere to its leaves. It is therefore partly a vegetable as well as an animal feeder." The leaves in one or two other species were found capable of greater and more enduring inflection, and the glands excitable to increased secretion even by bodies not yielding soluble nitrogenous matter.

The aquatic type of this family is Utricularia; and the bladder-bearing species of this genus are to Pinguicula nearly what Aldrovanda is to Dionæa and Drosera — the bladders imprisoning minute aquatic animals by a mechanism almost as ingenious as that of Dionæa itself. Observations of the same kind were made in this country by Mrs. Treat, of Vineland, New Jersey, before Mr. Darwin's investigations were made known. These submerged aquatic stomachs, ever deluged with water, apparently do not really digest their captures, but merely absorb the products of their decay.

The same must in all probability be said of such Pitcher-plants as Sarracenia and Darlingtonia, which Mr. Darwin merely alludes to at the close of his volume but does not treat of. Nepenthes, however, according to Dr. Hooker's investigations, has attained a higher dignity, and converted its pitcher into a stomach. This parallelism, and this higher and lower mode of appropriating organic products by each of the three well-marked carnivorous families of plants, are highly suggestive.

In concluding this notice of a book for which we have no room to do justice, — but which is sure to be in the hands of many interested readers, — there is something to be said in regard to the discovery of the lure in some of our Sarracenias. We have by degrees to discover our discoverers. In this Journal, only so far back as the number for August, 1873, is a notice of the discovery of a sweet secretion at the orifice of

the pitcher of *Sarracenia flava*, by Mr. B. F. Grady, of Clinton, North Carolina (in the article by an oversight called "Mr. Hill"), which effectively lures flies to their destruction. This statement, made in a letter, had been for several months in our hands, awaiting the opportunity of confirmation, when an allusion to the same thing appeared in the English edition of LeMaout and Decaisne's System of Botany, without reference to any source, and on inquiry we learned that the authority for the statement was forgotten. But early in the following year, when the monograph of the order appeared in the last volume of De Candolle's "Prodromus," a reference was found to a paper by Dr. Macbride in the Transactions of the Linnæan Society. His observations (made upon *S. variolaris*), it appears, were communicated to Sir J. E. Smith, read before the Linnæan Society in 1815, and published soon after. They are referred to by his surviving friend and associate, Eliott, in his well-known work, and therefore need not have gone to oblivion, or needed rediscovery here in our days by Mr. Grady and Dr. Mellichamp, the latter greatly extending our knowledge of the subject. Probably the main facts were all along popularly known in the regions these species affect, and where their use as fly-traps is almost immemorial. But the gist of these remarks is, that a colleague has just called our attention to an earlier publication than that of Dr. Macbride, namely, an article on "Certain Vegetable Muscicapæ," by Benjamin Smith Barton (one of our botanical fathers), published in "Tilloch's Philosophical Magazine" for June, 1812. Among other matters not bearing directly upon this point, he says of Sarracenia, without reference to any particular species: "A honeyed fluid is secreted or deposited on the inner surface of the hollow leaves, near their *faux* or opening; and this fluid allures great numbers of the insects which they are found to contain into the ascidia."

Here is earlier publication by three years. Yet we suspect that Dr. Barton knew little about it at first hand, and we find clear evidence that he had not anticipated Dr. Macbride. All his references have an indefiniteness quite in contrast with

Dr. Macbride's narrative; he says that "some if not all the species of the genus appear to possess a kind of glandular function," without mentioning those that have it, or the absence of it in the only species growing around him at the north; and he adds that he "was entirely unacquainted with this curious economy . . . when I published the first edition of my 'Elements of Botany,' and even when I printed the appendix (in vol. i.) to the second edition of this work." Now his paper is dated September 11, 1811; and the volume referred to, as just printed, is dated 1812. But Macbride states that his observations were chiefly made 1810 and 1811; he corresponded intimately with Eliott, through whom, if not directly, his observations would probably find their way at once to the Philadelphia naturalists.

NAUDIN ON THE NATURE OF HEREDITY AND VARIABILITY IN PLANTS.[1]

Why is it the nature and essence of species to breed true, and why do species sometimes vary? In other words, why is offspring like parent, and when unlike in certain particulars, what is the cause and origin of the difference? We commonly and properly enough take these two associated yet opposed facts as first principles. But it is equally proper and legitimate to enquire after the cause of them.

M. Naudin, a good many years ago, took up the study of hybrid plants, and followed up for a series of generations, the course of life of certain self-fertile ones, notably of Datura. We gave at the time an abstract of his observations of the manner in which the characters of two closely related common species, *D. Stramonium* and *D. Tatula*, were mixed, and in which the characters of the two began to separate in the close-bred progeny of the next generation, ending in a complete division of the amalgamated forms into those of the two constituent species after a few generations.

[1] American Journal of Science and Arts, 3 ser., xi. 153.

The "Comptes Rendus" of September 27 and October 4, 1875, contain an abstract of a paper communicated by M. Naudin to the French Académie des Sciences, of which the text was suggested by a hybrid between the wild *Lactuca virosa* and a variety of *L. sativa*, the common Lettuce. The hybrid was an accidental one: its seeds were fully fertile; a great number of young plants were raised from them, of which twenty were preserved for full development and study. Like other hybrids the original showed no character which was not evidently derived from the two parents; and, fertilized by its own pollen, the offspring all agreed in this respect, although they varied exceedingly among themselves in the division of the parental heritage, no two being quite alike. This exceeding vacillation between the two parental forms, but not overpassing the limits on either hand, — which Naudin finds to be the common characteristic of fertile hybrids, close-bred, — he names disordered variation (*variation désordonnée*). His explanation is that the hybrid is a piece of living mosaic, that two specific natures are at strife in it; in the progeny each endeavors to reclaim its own, like seeks like; whence in the course of a very few generations (as he first showed in Datura) a segregation takes place, part of the progeny reverting completely to one ancestral type, part to the other. What Naudin now insists upon is that out of all this disturbance comes nothing new; that there is here no variation beyond the line of inheritance; and therefore from crossing no possible development of species.

To this proposition we accede, so far as respects the direct consequence of crossing. To fill up the interval more or less between two forms or species with intermediate patterns may tend to the fusion or confusion of the two, but not to the origination of new forms or species. Although Naudin's own experiments lead him to deny all tendency to variation overpassing these limits, we do not forget that his countryman, the late M. Vilmorin, — working in a different way and with another object, — arrived at a different conclusion. He succeeded, as we understand, in originating floricultural novelties from species which refused to vary *per se*, by making a cross,

— not to infuse the character of the male parent, for he fertilized the progeny with the pollen of the female parent, and thus early bred out the other blood, but to induce variation, which, once initiated in the internal disorder consequent upon the crossing, was apt to proceed, or might be led on by selection, to great lengths, according to Vilmorin. The variations in question, being mainly such as are sought in floriculture, may not have passed the line laid down by Naudin, or actually have introduced new features. But such plants would surely have no exemption from the ordinary liability to variation. If other plants vary, in the sense of producing something new, so may these.

This brings us to another inference which Naudin draws. Having observed that his hybrids in their manifold variation exhibited nothing which was not derivable from their immediate ancestry, he directly (and in our opinion too confidently) concludes that all variation is atavism, — that when real variations are set up in ordinary species, this is not an origination but a reversion, a breaking out of some old ancestral character, a particular and long deferred instance of this *variation désordonnée*, which would thus appear to be the only kind of variation. This view has been presented before, but not, perhaps, so broadly. Adducing some theoretical considerations in its favor — to which we may revert — and some sound reasons against the view that variation is caused by external influences, he declares it "infinitely more probable that variation of species properly so called is due to ancestral influences rather than to accidental actions." We might think so if these two categories were exhaustive, and external conditions must be supposed to act immediately, as the cause rather than the occasion of variation. But the supposition that "accidental actions," whatever they may be, and external influences of every sort do not produce but educe and conduct variation — which is our idea of what natural selection means — avoids the force of Naudin's arguments.

Moreover, Naudin's view, regarded as an hypothesis for explaining variation, leaves the problem just where it finds it. To explain the occurrence of present and actual variations,

hypothetical ones like those of a former time are assumed; the present diversity implies not only equal but the very same anterior diversity, and so on backwards. Or rather it demands a much greater diversity at the outset than now; for these aberrant forms are the rare exception, and if due to atavism they imply the loss of the many and the incidental reappearance of the few. Else they would be the rule instead of the exception, and atavism would be simply heredity. This comes to the view which Mr. Agassiz strongly maintained, that really there are no varieties, — meaning, we understand, that all the forms are aboriginal, except the transient ones evidently due to circumstances.

That some variation is atavism is clear enough. This is the natural explanation of the appearance of characters wanting in the immediate parents but known in their ancestors or presumed ancestors. But the assumption of hypothetical ancestors to account for variation generally is quite another thing. Besides its inutility as an explanation, to which we have adverted, its improbability as an hypothesis is set in a strong light by Naudin's own forcible conception of the nature of heredity. What is heredity? he asks. In other words, what keeps species so true, offspring like parent, through the long line of generations? He illustrates hereditary force by comparing its action with that of physical force, in which the movement from one state of equilibrium to another is always that in which there is least resistance. From which it follows that when it has once begun to proceed in a certain course, its tendency to continue in that direction increases, because it facilitates its way as it overcomes obstacles. In other words, this line becomes fixed by habit; *vires acquirit eundo;* the stream deepens its bed by flowing; and the more remote the commencement of a certain course, the more fixed its direction, and the greater its power of overcoming opposition. The species is kept true in its course by the sum of the hereditities which press each individual forward in its actual direction. So that, as Naudin remarks, if we could calculate the energy with which millions of ancestors tend to impel the living representative of the line onward in the same direction, we

should better apprehend the persistence of species, and feel the great improbability that the stream will ever escape from its ancient and well-worn bed, and strike into new courses.

Now, in the first place, the more lively the conception we thus form of the invariability of species, through a happy metaphorical illustration of it, the more unlikely does it appear that early characters, long lost in the flow, should reappear through atavism as varieties. To continue the simile, the more impetuous the stream, the less the possibility of its turning back upon itself, and resuming old characteristics. The eddies of atavism (the resumption of dropped characters) are not likely to extend back very far; and it seems gratuitous to have recourse to them in explanation of new forms. Moreover, although the stream has made its bed and lies in it, not escaping from its own valley, it is flexible enough to obstacles, is ever changing its particular course as it flows, and may by its own action send off here and there a bayou (variety) or branch into a delta of channels (derivative species).

Like Agassiz, Naudin conceives of species as originating with a large number of individuals of the same structure, and of which numerous reciprocal crosses have determined the direction of the line in which their posterity have evolved. But he maintains that these individuals, and all existing species, had a common origin in a "proto-organism"; and that the various lines of descent acquired fixity into species only as they acquired sexuality. If we rightly apprehend it, Naudin's idea of the purport of sexual reproduction (as contrasted with that by buds) is to give fixity to species. Our idea is a different one, both as to the essential meaning of sexuality and as to its operation in respect to fixity. His conception may be tested by inquiring which are the more variable, or sportive, seedlings or plants propagated from buds. This we suppose can be answered only one way.

M. Naudin is a veteran and excellent investigator, and nothing which he writes is to be slighted. We have frankly set down our impressions upon a first perusal of his important communication; but are ready to revise them, if need be, upon more deliberate consideration.

CROSS AND SELF-FERTILIZATION IN THE VEGETABLE KINGDOM.

Mr. Darwin, in the title of his new work,[1] refers only incidentally to adaptations for cross-fertilization, — a subject which has given origin to a copious literature since he opened it anew in his book on the Fertilization of Orchids, in 1862. A new edition of this latter book is on the eve of publication in England, and we believe that this author's scattered papers on cross-fertilization, as secured by various contrivances, are about to be collected, revised, and published in book form. In the volume now before us, Mr. Darwin deals with the effects of cross and self-fertilization, recounts at length the experiments he has devised and carried on, collects and criticises the results, glances at the means of fertilization, and the habits of insects in relation to it, and ends with some theoretical considerations of inferences suggested by or deduced from the facts which have been brought to light.

If writing for the popular press, we should be bound to say that the book is not light reading. Three fourths of its pages and of the chapters are devoted to the details of the experiments and the sifting and the various presentation of the results; and the remainder, although abounding in curious facts and acute suggestions, is yet of a solid character. The bearings of various points upon what is called "Darwinism" are merely touched or suggested, here and there, in a manner more likely to engage the attention of the thoughtful scientific than of the general reader.

That cross-fertilization is largely but not exclusively aimed at in the vegetable kingdom, is abundantly evident. As Mr. Darwin declares, "it is unmistakably plain that innumerable flowers are adapted for cross-fertilization, as that the teeth and talons of a carnivorous animal are adapted for catching prey, or that the plumes, hooks, and wings of a seed are adapted

[1] *The Effects of Cross and Self-Fertilization in the Vegetable Kingdom.* By Charles Darwin. London and New York, 1876. (American Journal of Science and Arts, 3 ser., xiii. 125.)

for its dissemination." That the crossing is beneficial, and consequently the want of it injurious, is a teleological inference from the prevalence of the arrangements which promote or secure it, — an inference the value of which increases with the number, the variety, and the effectiveness of the arrangements for which no other explanation is forthcoming. That the good consisted in a re-invigoration of progeny, or the evil of close-breeding in a deterioration of vigor, was the suggestion first made (so far as we know), or first made prominent, by Knight, from whom Darwin adopted it. However it be as to animals, there was until now no clear and direct evidence that cross-fertilization in the vegetable kingdom did re-invigorate. Indeed, the contrary might be inferred from the long and seemingly indefinite perpetuation of bud-propagating varieties, which have no fertilization at all. But the inference from this is not as cogent as would at first appear. For, although bud-propagation is, we think, to be considered as the extreme of close-breeding, yet in it the amount of material contributed by parent to offspring is usually vastly more than in sexual reproduction; and, accordingly, the diminution to an injurious degree of any inherited quality or essence might be correspondingly remote. Yet, as sexual reproduction may be and often must be much closer in plants than it can be in most animals, the ill effects of self-fertilization, or the good of cross-fertilization, might the sooner be noticeable. Mr. Darwin arranged a course of experiments to test this question, prosecuted it as to some species for eleven years; and the main object of this volume is to set forth the results.

Ipomœa purpurea, the common Morning Glory of our gardens, was the leading subject. The flowers of this species self-fertilize, but must also be habitually cross-fertilized, as they are visited freely by bumble-bees and other insects. Ten flowers of a plant in a green-house were fertilized with their own pollen; ten others were crossed with pollen from a different plant. The seeds from both were gathered, allowed to germinate on damp sand, and as often as pairs germinated at the same time the two were planted on opposite sides of the same pot, the soil in which was well mixed, so as to be uniform

in composition. "The plants on the two sides were always watered at the same time and as equally as possible, and even if this had not been done the water would have spread almost equally to both sides, as the pots were not large. The crossed and self-fertilized plants were separated by a superficial partition, which was always kept directed towards the chief source of light, so that the plants on both sides were equally illuminated." Five pairs were thus planted in two pots, and all the remaining seeds, whether or not in a state of germination, were planted on the opposite sides of a third pot, so that the plants were crowded and exposed to a very severe competition. Rods of equal diameter were given to all the plants to twine up, and as soon as one of each pair had reached the summit, both were measured. But a single rod was furnished to each side of the crowded pot, and only the tallest plant on each side was measured. This was followed up for ten generations; the close-fertilization being always self-fertilization, *i. e.*, by pollen to stigma of the same flower; the crossing, between individuals in successive generations of this same stock, except in special instances, when an extraneous stock was used as one parent, — to eminent advantage, as will be seen.

The difference in vigor between the cross-bred and the close-bred progeny, as measured by early growth, was well marked throughout. In the mean of the ten generations it was as 100 to 77. In the tenth generation it was 100 to 54, that is, five cross-bred plants grew to the average height of 93.7 inches while the close-bred were reaching the average of 50.4 inches. This was a notably greater difference than in any previous generation. But this was probably accidental or anomalous, for it was not led up to by successive steps. Indeed, the difference in the first generation was a trifle greater than the average of all ten, being as 100 to 76. The second generation was as 100 to 79; the third as 100 to 68; the fourth as 100 to 86; the fifth as 100 to 75; the sixth as 100 to 72; the seventh as 100 to 81; the eighth as 100 to 85; the ninth as 100 to 79; the tenth as already stated, 100 to 54. The general result is made striking in the following illustration.

"If all the men in the country were on an average six feet high, and there were some families which had been long and closely interbred, these would be almost dwarfs, their average height during ten generations being only four feet eight and one-quarter inches." (p. 53.)

It is remarkable that the difference between the close-bred and the cross-bred individuals should have been as great as it was in the first generation; and, this being the case, it might have been expected that the difference would have gone on increasing in the succeeding generations. If self-fertilization is injurious, the ill effects would be expected to be cumulative. "But," instead of this, "the difference between the two sets of plants in the seventh, eighth, and ninth generations taken together is less than in the first and second generations together." Upon this Mr. Darwin remarks: "When, however, we remember that the self-fertilized and crossed plants are all descended from the same mother plant, that many of the crossed plants in each generation were related, often closely related, and that all were exposed to the same conditions, which, as we shall hereafter find, is a very important circumstance, it is not at all surprising that the difference between them should have somewhat decreased in the later generations." (p. 56.)

Further light was thrown upon these points by two kinds of subsidiary experiments. In one case, the cross was made between two flowers of the same plant of Ipomæa, while other flowers were self-fertilized as before. On raising seedlings from the two lots, it was found that such crossing gave no superiority; indeed, the offspring of the self-fertilized flowers appeared to be rather more vigorous than the close-crossed. And other experiments led to the same conclusion, namely, that there was no particular benefit from cross-fertilization on the same plant. In the other case, the cross was made not only between the flowers of distinct plants, but between those from different sources, and which had presumably grown under somewhat different conditions. For instance, several flowers of the ninth generation of crossed plants of Ipomæa were crossed with pollen taken from the

same variety but from a distant garden. The resulting seedlings showed the benefit of the fresh stock remarkably, being as much superior in vigor to those of the tenth intercrossed generation as the latter were to the self-fertilized plants of a corresponding generation. In height they were as 100 to 78, over the ordinary intercrossed; and in fertility, as 100 to 51. Indeed, Mr. Darwin's main conclusion from all his observations is, "that the mere act of crossing by itself does no good. The good depends on the individuals which are crossed differing slightly in constitution, owing to their progenitors having been subjected during several generations to slightly different conditions, or to what we call in our ignorance spontaneous variation."

The greater constitutional vigor of the crossed plants of Ipomæa was manifested in other ways than their rate or amount of growth: they better endured exposure to a low temperature or sudden changes of temperature; they blossomed earlier; and they were more fertile. The difference in fertility varied greatly in degree (the extremes in different experiments and in different generations being 100 to 99 and 100 to 26), but was always sustained. Also, "the impaired fertility of the self-fertilized plants was shown in another way, namely, by their anthers being smaller than those in the flowers on the crossed plants. This was first observed in the seventh generation, but may have occurred earlier. . . . The quantity of pollen contained in one of the self-fertilized was, as far as could be judged by the eye, about half of that contained in one from a crossed plant. The impaired fertility of the self-fertilized plants of the eighth generation was also shown in another manner, which may often be observed in hybrids — namely, by the first-formed flowers being sterile."

Similar experiments were made, but not carried to the same extent, upon fifty-seven other species of plants, belonging to fifty-two genera, and to thirty great natural families, the species being natives of all parts of the world. The results — the details and discussion of which occupy the bulk of this volume — vary greatly, some plants making a better and

others a less good showing for the advantage of cross-fertilization, and this advantage manifesting itself in different ways, some in vigor or amount of growth, some in hardiness, most in fertility; but with twelve cases in which the crossed plants showed no marked advantage over the self-fertilized. There were, however, fifty-seven cases in which the crossed exceeded the self-fertilized by at least five per cent., generally by much more.

Increase of vigor, as evinced in growth, appears generally to be accompanied by increased fertility; but sometimes the good of crossing was manifested only in productiveness, *i. e.*, in a larger amount of seed. This proved to be the case in Eschscholtzia, in which — strange to say — self-fertilized plants of several generations were superior in size and weight to intercrossed plants, even when the crossing was between flowers derived on one side from American, on the other from English seed, from which, upon Mr. Darwin's view, the maximum benefit should be gained. This instance, however, stands alone. Yet it is approached by several others, in a manner which might have negatived the general conclusions of the research, if they had been hastily gathered from a small number of trials.

For example, in the sixth self-fertilized generation of *Ipomœa purpurea*, one of these plants took the lead of its competitor, kept it almost to the end, and was ultimately overtopped only by half an inch on a total height of several feet. To ascertain whether this exceptionally vigorous plant would transmit its power to its seedlings, several of its flowers were fertilized with their own pollen, and the seedlings thus raised were put into competition with ordinary self-fertilized and with intercrossed plants of the corresponding generation. The six children of Hero (the name by which this individual was designated) beat the ordinary self-fertilized competitors at the rate of 100 to 84, and the intercrossed competitors at the rate of 100 to 95; and in the next generation the self-fertilized grandchildren beat those from a cross between two of the children at the rate of 100 to 94. In the next generations the seedlings were raised in winter in a hot-house, became

unhealthy, and the experiment terminated without marked result. Moreover, the remarkable vigor of growth in Hero and its progeny was attended by somewhat increased fertility. Here, then, an idiosyncrasy arose from some utterly unknown cause, — a spontaneous variation of constitution, which was transmitted to posterity, and which gave all the benefit of cross-fertilization, and somewhat more, both as to vigor and fertility. A similar idiosyncrasy made its appearance in the third generation of seedlings of *Mimulus luteus*.

Discordant or anomalous facts like these seem confusing, even though too few to affect seriously the grand result of the numerous experiments; but upon Darwinian principles, in which adaptations are ultimate results, they are to be expected, as a consequence of the general and apparently vague proclivity to vary.

In Foxglove, — the flowers of which are naturally self-sterile or nearly so, and in which crossing gave a marked advantage over self-fertilizing, both as to growth and productiveness, — a decided, though small advantage appeared to come from the crossing of flowers on the same plant.

In *Origanum vulgare*, crosses were made between different plants of a large clump, long cultivated in a kitchen-garden, which had evidently spread from a single root by stolons, and which had become in a good degree sterile, as is usual under such conditions. The crossing caused rather more seed to form; but the seedlings from the crossed did not surpass in growth those of the self-fertilized; "a cross of this kind did no more good than crossing two flowers on the same plant of Ipomæa or Mimulus. Turned into the open ground, and both self and cross-fertilized the following summer, and equal pairs of the resulting seeds planted on opposite sides of two very large pots, the crossed plants from seed showed a clear superiority over their self-fertilized brethren, at the rate of 100 to 86. But this excess of height by no means gives a fair idea of the vast superiority in vigor of the crossed over the self-fertilized plants. The crossed flowered first and produced thirty flower-stems, while the self-fertilized produced only fifteen, or half the number. The pots were then bedded out,

and the roots probably came out of the holes at the bottom, and thus aided their growth. Early in the following summer, the superiority of the crossed plants, owing to their increase by stolons, over the self-fertilized plants, was truly wonderful. . . . Both the crossed and the self-fertilized plants being left freely exposed to the visits of bees, manifestly produced much more seed than their grandparents, — the plants of original clumps still growing close by in the same garden, and equally left to the action of bees."

These few cases must here suffice, and they give a fair general idea of the main results reached, — somewhat qualified, however, by certain instances in which little or no benefit was observed. Let it be remarked that while most of the cases show decided and unequivocal good from the crossing, none of them unequivocally tell to the contrary, as the advantage appears sometimes in one direction, sometimes in another. "Thus, the crossed and self-fertilized plants of Ipomæa, Papaver, *Reseda odorata*, and Limnanthes were almost equally fertile, yet the former exceeded considerably in height the self-fertilized plants. On the other hand, the crossed and self-fertilized plants of Mimulus and Primula differed in an extreme degree in fertility, but by no means to a corresponding degree in height or vigor."

We must wholly omit — among many other things — the interesting account of self-sterile plants, meaning here not those in which the pollen does not reach the stigma unaided, but those in which it is impotent, or nearly so, when applied, although efficient upon the stigma of another individual. Verbascum, Passiflora, Corydalis, and many Orchids afford instances of this sort. In these, the advantage of cross-fertilization arises to a necessity. A noteworthy fact respecting them (of which Mr. Darwin makes much) is, that such self-sterility, or the reverse, is influenced by slight changes in the conditions, such as difference in temperature, grafting on another stock, and the like. In South Brazil, Fritz Müller found that for six generations all his plants of *Eschscholtzia Californica* were completely sterile, unless supplied with pollen from a distinct plant, when they were completely fer-

tile. This was not the case in English plants, which, when covered by a net, set a considerable number of capsules, the seeds of which, by weight, were as 71 to 100 of those on plants intercrossed by bees. These Brazilian seeds, sent to England, yielded plants with moderately self-fertile flowers, and this limited self-fertility was increased in two generations of English growth. Conversely, seeds from English plants grown in Brazil were more self-fertile than those reared in Brazil for several generations; yet "one which did not flower the first year, and was thus exposed for two seasons to the climate of Brazil, proved quite self-sterile, like a Brazilian plant, showing how quickly the climate had acted on its sexual constitution." Having observed that certain individuals of Mignonette were self-sterile, Mr. Darwin secured several such plants under separate nets, and by intercrossing these for a few generations, obtained plants which inherited this peculiarity, so that "without doubt a self-sterile race of Mignonette could easily have been established."

Nine of the twelve chapters are devoted strictly to the effects of cross and self-fertilization. The tenth considers the "means of fertilization." Cross-fertilization is favored or ensured by: 1, the separation of the sexes; 2, the maturity of the male and female sexual elements at different periods; 3, dimorphism, or even trimorphism; 4, various mechanical contrivances; 5, the more or less complete inefficiency of the flower's own pollen on its stigma, and the prepotency of pollen from any other individual over that from the same plant. We understand that Mr. Darwin is just now occupied in revising and extending his various papers upon these topics, with a view to their publication in a volume. Here he gives a list of plants which, when insects are excluded, are either quite sterile or produce less than half the number of seeds yielded by unprotected plants. This is followed by a list of plants which, when protected from insects, are either quite fertile or yield more than half the number of seeds produced by unprotected plants.

"Each of these lists contains by a mere accident the same number of genera, namely, forty-nine. The genera in the first

list include sixty-five species, and those in the second sixty species; the *Orchideæ* in both being excluded. If the genera in this latter order, as well as in the *Asclepiadeæ* and *Apocynaceæ*, had been included, the number of species which are sterile if insects are excluded would have been greatly increased; but the lists are confined to species which were actually experimented upon. The results can be considered as only approximately accurate, for fertility is so variable a character, that each species ought to have been tried many times. The above number of species, namely, 125, is as nothing to the hosts of living plants; but the mere fact of more than half of them being sterile within the specified degree, when insects are excluded, is a striking one; for whenever pollen has to be carried from the anthers to the stigma in order to insure full fertility, there is at least a good chance of cross-fertilization. I do not, however, believe that if all known plants were tried in the same manner, half would be found to be sterile within the specified limits; for many flowers were selected for experiment which presented some remarkable structure; and such flowers often require insect-aid." — (p. 370.)

It is worth noticing that *Trifolium repens* and *T. pratense* (the common White and Red Clovers) have a place in the first list; *T. arvense* and *T. procumbens* in the second. Darwin refers to Mr. Miner's statement that "in the United States hive-bees never suck the Red Clover," and says it is the same in England, except from the outside through holes bitten by humble-bees; yet that H. Müller has seen them visiting this plant in Germany for the sake both of pollen and nectar, which latter they obtained by breaking apart the petals. Darwin has not qualified his statement, long ago made, of the complete sterility of Red Clover protected from insects; but Mr. Meehan asserts that protected plants are fertile in this country, without, however, giving details or the rate of fertility. In *T. arvense*, "the excessively small flowers are incessantly visited by hive and humble-bees; when insects were excluded the flower-heads seem to produce as many and as fine seeds as the exposed heads."

As to cross-fertilization, "the most important of all the means by which the pollen is carried from the anthers to the stigma of the same flower, or from flower to flower, are insects, belonging to the orders of Hymenoptera, Lepidoptera, and Diptera; and in some parts of the world, birds." In a note the author cites all the cases known to him of birds fertilizing flowers. These are chiefly humming-birds. "In North America they are said to frequent the flowers of Impatiens" (for which Gould, "Trochilidæ," is referred to as authority, and a reference is given to the "Gardener's Chronicle," which we find relates to something else in South America); and this is all concerning the United States. Can it be that there are no references in print to the most familiar fact that our humming-bird is very fond of sucking the blossoms of Trumpet Creeper (*Tecoma radicans*) and of Honeysuckles? Both these are, in size and arrangement of parts, well adapted to be thus cross-fertilized.

Flowers are rendered conspicuous to birds and still more to insects, by bright colors. And as "almost every fruit which is devoured by birds presents a strong contrast in color with the green foliage, in order that it may be seen and its seeds disseminated," so the proportionally large size and the bright colors of the corolla, or, in some cases, the equally bright hues of the adjoining parts of the flower, or of the inflorescence, are correlated to visiting insects, — have come to pass, as Darwin would say, in consequence of the visits of insects, through the advantages in vigor and productiveness gained by cross-fertilization. He is ready to adopt even the idea of Conrad Sprengel, which seemed to be so fanciful, that marks and streaks on the corolla serve as guides to the nectary; for, although insects are well able to discover the nectar without the aid of guiding marks, yet they are of service by facilitating the search and enabling insects to suck a greater number of blossoms within a given time, which is tantamount to greater opportunity for cross-fertilization.

That odors attract insects is certain, and many flowers are both conspicuous and odoriferous, while others make up in fragrance what they lack in show. "Nägeli affixed artificial

flowers to branches, scenting some with essential oils, and leaving others unscented; and the insects were attracted to the former in an unmistakable manner."

"Of all colors white is the prevailing one; and of white flowers a considerably larger proportion smell sweetly than of any other color, namely, 14.6 per cent.; of red, only 8.2 per cent. are odoriferous. The fact of a larger proportion of white flowers smelling sweetly may depend in part on those which are fertilized by moths requiring the double aid of conspicuousness in the dusk and of odor. So great is the economy of nature, that most flowers which are fertilized by crepuscular or nocturnal insects emit their odor chiefly or exclusively in the evening. Some flowers, however, which are highly odoriferous depend solely on this quality for their fertilization, such as the night flowering-stock (Hesperis) and some species of Daphne; and these present the rare case of flowers which are fertilized by insects being obscurely colored."

"The shape of the nectary and of the adjoining parts are likewise related to the particular kinds of insects which habitually visit the flowers: this has been well shown by H. Müller, by his comparison of lowland species, which are chiefly visited by bees, with alpine species belonging to the same genera, which are visited by butterflies."

"Pollen contains much nitrogen and phosphorus,—the two most precious of all the elements for the growth of plants,—but in the case of most open flowers, a large quantity of pollen is consumed by pollen-devouring insects, and a large quantity is destroyed during long-continued rain. With many plants this latter evil is guarded against, as far as possible, by the anthers opening only during dry weather, by the position and form of some or all of the petals, by the presence of hairs, etc.; also, as Kerner has shown in his interesting essay, by the movements of the petals or of the whole flower during cold and wet weather. In order to compensate the loss of pollen in so many ways, the anthers produce a far larger amount than is necessary for the fertilization of the same flower. I know this from my own experiments on Ipomæa, given in the Introduction; and it is still more plainly shown

by the astonishingly small quantity produced by cleistogene flowers, which lose none of their pollen, in comparison with that produced by the open flowers borne by the same plants; and yet this small quantity suffices for the fertilization of all their numerous seeds. Mr. Hussall took pains in estimating the number of pollen-grains produced by a flower of the Dandelion, and found the number to be 243,600, and in a Peony 3,654,000 grains. The editor of the 'Botanical Register' counted the ovules in the flowers of *Wistaria sinensis*, and carefully estimated the number of pollen-grains, and he found that for each ovule there were 7,000 grains."— (pp. 376, 377.)

These are probably fair averages of the numerical ratio of pollen to ovules in flowers which are adapted to be fertilized by insect agency. Their meaning in the "economy of nature" is seen by a comparison on the one hand with *anemophilous*, *i. e.* wind-fertilized, flowers, in most of which there is a vastly greater disproportion between the numbers, — compensating for inevitable waste, — and on the other hand with *cleistogenous* flowers, namely, those small and less developed blossoms which some plants produce in addition to the ordinary sort, and which fertilize as it were in the bud, necessarily by their own pollen. Here is no waste, and accordingly the anthers are very small, and the pollen-grains are not many times more than the ovules: also such flowers are never brightly colored, never odoriferous, and they never secrete nectar.

The only advantages of this close-fertilization which we can think of are sureness and strict likeness; both of which are quite as well secured by budding-reproduction. Now, as cleistogene flowers are borne, we believe, chiefly and perhaps only, by species whose normal blossoms are adapted for insect-fertilization, they must be regarded as a subsidiary arrangement, a safeguard against failure of proper insect-visitation. As the volume before us amply shows, this failure is in general provided for by a more or less wide margin of self-fertilization in the very flowers which are adapted for crossing. In Impatiens, Viola, and the like, it is provided for by separate flowers, the special adaptations of which are unmistakable.

H. Müller appears to have shown "that large and conspic-

uous flowers are visited much more frequently and by many more kinds of insects than are small inconspicuous flowers. He further remarks that the flowers which are rarely visited must be capable of self-fertilization, otherwise they would quickly become extinct." Mr. Darwin's list seems to show that, as a rule, they are so; yet many very small flowers, like those of *Trifolium arvense*, and small and dingy ones, like those of Asparagus, are freely visited by bees; and, conversely, many large and conspicuous flowers which are frequented by insects are none the less self-fertilizable. Throughout we find that such things do not conform to arbitrary or fixed rules; and this favors the idea that the differences have been acquired. Mr. Darwin conjectures that the self-fertilizing capabilities of many small and inconspicuous flowers may be comparatively recent acquisitions, on the ground that, if they were not occasionally intercrossed, and did not profit by the process, all their flowers would have become cleistogenous, "as they would thus have been largely benefited by having to produce only a small quantity of safely protected pollen."

Mr. Darwin's experiments tending to prove that cross-fertilization between flowers on the same plant is of little or no use, he is naturally led to consider the means which favor or ensure their fertilization with pollen from a distinct plant. This must needs take place with diœcious plants, and is likely to occur with the monœcious, and is in some cases secured (as in Walnut and Hazelnut) by some trees being proterandrous and others proterogynous, so that they will reciprocally fertilize each other. In ordinary hermaphrodite species the expansion of only a few blossoms at a time greatly favors the intercrossing of distinct individuals, although in the case of small flowers it is attended with the disadvantage of rendering the plants less conspicuous to insects. Our common Sundews furnish a good illustration of this. They abound wherever they occur, and are for a long while in blossom, but each plant or spike opens but one flower at a time. The fact of bees visiting the flowers of the same species as long as they can, instead of promiscuously feeding from the various

blossoms nearest within reach, greatly favors such intercrossing. So does the remarkable number of flowers which bees are able to visit in a short time (of which mention will be made), and the fact that they are unable to perceive without entering a flower whether other bees have exhausted the nectar. Then dichogamy (the maturation of one sex in a hermaphrodite flower earlier than the other) is so prevalent that it may almost be regarded as the rule; and this ensures such crossing between few-flowered plants, and greatly favors it in the case of spikes, racemes, and the like. For, proterandry being the commonest arrangement, so that the younger flowers act as male, and the older as female, the bees habitually alighting at the bottom and proceeding upward, they carry the pollen from the upper and younger flowers to stigmas of the lower and older flowers of the next spike, and so on. Heterogonism, which is less common, operates precisely like complete dicœcious separation of the sexes in this respect, and with the advantage that all the individuals are seed-bearing. Most of the special arrangements peculiar to certain families, such as Orchids, — or to plants, such as Posoqueria, with its wondrous mechanism for quickly stopping out access to the stigma when the pollen is violently discharged upon some insect, but opening the orifice the next day, — are of a kind to favor the crossing of distinct plants. Prepotency of other pollen, which may accompany the other arrangements or exist independently, acts largely and powerfully toward the same end. Our author investigates this at some length: we cite for illustration a single but strong case. The stigmas of a long-styled Cowslip were supplied with pollen from the same plant, and again after twenty-four hours, with pollen of a short-styled, dark-red Polyanthus, a variety of the same species: from the resulting seeds twenty seedlings were raised, and all of them bore reddish flowers; so that the effect of the plant's own pollen, though placed on the stigmas twenty-four hours previously, was destroyed by that of the red variety. The same thing is shown by the impossibility in many cases of raising two varieties of the same species pure if they grow near each other. "No one who has had any

experience would expect to obtain pure cabbage-seed, for instance, if a plant of another variety grew within 200 or 300 yards." And a veteran cultivator once had his whole stock of seeds seriously bastardized by some plants of purple Kale which flowered in a cottager's garden half a mile away. Mr. Gordon records a case of the crossing between Primroses and Cowslips through pollen carried by bees over more than two kilometers, or an English mile and a quarter.

We must copy the close of this section — long though it be — because of its capital illustration of the topic in hand, and for the teleological lesson which it teaches.

"The case of a great tree covered with innumerable hermaphrodite flowers, seems at first sight strongly opposed to the belief in the frequency of intercrosses between distinct individuals. The flowers which grow on the opposite sides of such a tree will have been exposed to somewhat different conditions, and a cross between them may perhaps be in some degree beneficial; but it is not probable that it would be nearly so beneficial as a cross between flowers on distinct trees, as we may infer from the inefficiency of the pollen taken from plants which have been propagated from the same stock though growing on different roots. The number of bees which frequent certain kinds of trees when in full flower is very great, and they may be seen flying from tree to tree more frequently than might have been expected. Nevertheless, if we consider how numerous are the flowers, for instance, on a Horse-Chestnut or Lime-tree, an incomparably larger number of flowers must be fertilized by pollen brought from other flowers on the same tree, than from flowers on a distinct tree. But we should bear in mind that with the Horse-Chestnut, for instance, only one or two of the several flowers on the same peduncle produce a seed; and that this seed is the product of only one out of the several ovules within the same ovarium. Now we know from the experiments of Herbert and others that if one flower is fertilized with pollen which is more efficient than that applied to the other flowers on the same peduncle, the latter often drop off; and it is probable that this would occur with many of the self-fertilized flowers

on a large tree, if other and adjoining flowers were cross-fertilized. Of the flowers annually produced by a great tree, it is almost certain that a large number would be self-fertilized; and if we assume that the tree produced only 500 flowers, and that this number of seeds were requisite to keep up the stock, so that at least one seedling should hereafter struggle to maturity, then a large proportion of the seedlings would necessarily be derived from self-fertilized seeds. But if the tree annually produced 50,000 flowers, of which the self-fertilized dropped off without yielding seeds, then the cross-fertilized flowers might yield seeds in sufficient number to keep up the stock, and most of the seedlings would be vigorous from being the product of a cross between distinct individuals. In this manner the production of a vast number of flowers, besides serving to entice numerous insects and to compensate for the accidental destruction of many flowers by spring-frosts or otherwise, would be a very great advantage to the species; and when we behold our orchard-trees covered with a white sheet of bloom in the spring, we should not falsely accuse Nature of wasteful expenditure, though comparatively little fruit is produced in the autumn."

The Horse-Chestnut is not altogether a well-chosen example, for in it, as in our Buckeyes, a very large proportion of the flowers in the thyrsus are usually male, with barely a vestige of pistil. These serve, however, to increase the show, in the manner here illustrated, as well as to furnish abundance of pollen.

The section on anemophilous (wind-fertilized) plants, — their interest as survivals of the earlier phænogamic vegetation, the speculation as to how, when flying insects came to prevail, an anemophilous plant may have been rendered entomophilous, — how pollen, being a most nutritious substance, would soon have been discovered and devoured by insects, and by adhering to their bodies be carried from anthers to stigma and from one flower to another, — how a waste secretion, such as honey-dew or glandular exudations, may have been developed into nectar and utilized as a lure, — the interesting illus-

trations of the vast amount of pollen produced by anemophilous plants, and the great distances to which their light pollen is often carried by the wind, — all these inviting topics we must now pass by.

In passing we note the remark that "the excretion of a sweet liquid by glands seated outside of a flower is rarely utilized as a means of cross-fertilization by the aid of insects;" and the sole exception alluded to is that of the bracts of *Marcgraviaceæ*. But a parallel case is afforded by many species of Euphorbia, and notably in a striking species cultivated in conservatories, under the name of Poinsettia. Here the attraction to the eye is supplied by the intense red coloration of ordinary leaves placed next to the inflorescence, and that to the palate or tongue (if either term be allowed), by a large cup-shaped gland on the side of the involucre, which contains or surrounds the naked and greatly simplified flowers of both sexes.

That anemophilous plants are prevailingly declinous (either monœcious or diœcious) is speculatively connected with their antiquity; that they are very largely trees or shrubs is because "the long life of a tree or bush permits of the separation of the sexes with much less risk of evil from impregnation occasionally failing, and seeds not being produced, than in the case of short-lived plants. Hence it is probably, as Lecoq has remarked, that annual plants are rarely diœcious." The number of anemophilous species is comparatively small, but that of individuals of the species strikingly large, so that they form of themselves, in cold and temperate regions, where plant-fertilizing insects are fewer, either vast forests, as of *Coniferæ*, Birches, Beeches, etc., or meadows and glades, as of Grasses, Sedges, and Rushes. Being thus either necessarily or prevailingly cross-fertilizable and gregarious, it is not wonderful that they should hold their own unchanged in various parts of the world. Still their advantage is gained at the expense of the production of an enormous superfluity of pollen, a costly product; and, when diœcious, half the individuals produce no seed. Hermaphroditism with dichogamy, or some equivalent, and transportation by an appeal

to the senses and appetites of insects, secures all the advantages with least expenditure. The earliest fertilization in plants took place by the locomotion of the fertilizing or even of the fertilized material, in manner of most of the *Algæ:* mainly losing this as vegetation became terrestrial, the transportation was committed to the winds, and finally in the higher plants more economically consigned to insects.

The eleventh chapter, on the habits of insects in relation to the fertilization of flowers, is one of the interesting and readable although one of the shortest. It appears that the prince of naturalists, Aristotle, had observed more than two thousand years ago that the hive-bees visited the flowers of the same species as long as possible before going to a different species. This holds true of all kinds of bees and certain other insects, generally, but not absolutely; although, as Lubbock has recently proved, bees are much guided by color, yet they hold to the practice just mentioned in spite of difference in this respect, being botanists enough to know that color is not a good specific character. Mr. Darwin has repeatedly seen humble-bees flying straight from a red Fraxinella to a white variety, from one Larkspur to a different-colored variety; and the same as to Primroses and Pansies. But two species of Poppy were by some bees treated as one; and H. Müller traced hive-bees from blue Hyacinths to blue Violets. On the other hand, Darwin's bees fly straight from clump to clump of yellow Œnothera without turning an inch in their course to Eschscholtzias with yellow flowers which abound on either side. This constancy to species, however, is manifested only when their flowers abound; a fact which may have led Mr. Darwin to his explanation of the reason of it.

"The cause probably lies in insects being thus enabled to work quicker; they have just learned how to stand in the best position on the flower, and how far and in what direction to insert their proboscides. They act on the same principle as does an artificer who has to make a half a dozen engines, and who saves time by making consecutively each wheel and part for all of them. Insects, or at least bees, seem much influenced by habit in all their manifold operations;

and we shall presently see that this holds good in their felonious practice of biting holes through the corolla." — (p. 420.) As to this latter practice —

"The motive which impels bees to gnaw holes through the corolla seems to be the saving of time, for they lose much time in climbing into and out of large flowers, and in forcing their heads into closed ones. They were able to visit nearly twice as many flowers, as far as I could judge, of a Stachys and Pentstemon by alighting on the upper surface of the corolla and sucking through the cut holes, than by entering in the proper way. Nevertheless each bee before it has had much practice, must lose some time in making each new perforation, especially when the perforation must be made through both calyx and corolla. This action therefore implies foresight, of which faculty we have abundant evidence in their building operations; and may we not further believe that some trace of their social instinct, that is, of working for the good of other members of the community, may here likewise play a part? Many years ago I was struck with the fact that humble-bees as a general rule perforate flowers only when these grow in large numbers near together," etc., etc. (p. 433.)

It appears that the cutting of these holes is done only by humble-bees, never by hive-bees. Yet the latter are quick to take advantage of them.

"In the early part of the summer of 1857 I was led to observe during some weeks several rows of the scarlet Kidney-bean (*Phaseolus multiflorus*), whilst attending the fertilization of this plant, and daily saw humble and hive-bees sucking at the mouths of the flowers. But one day I found several humble-bees employed in cutting holes in flower after flower; and on the next day every single hive-bee, without exception, instead of alighting on the left wing petal and sucking the flower in the proper manner, flew straight without the least hesitation to the calyx, and sucked through the holes which had been made only the day before by the humble-bees, and they continued this habit for many following days. Mr. Belt has communicated to me (July 28, 1874) a similar case,

with the sole difference that less than half of the flowers had been perforated by the humble-bees; nevertheless all the hive-bees gave up sucking at the mouths of the flowers and visited exclusively the bitten ones. Now how did the hive-bees find out so quickly that holes had been made? Instinct seems to be out of the question as the plant is an exotic. The holes cannot be seen by bees whilst standing on the wing-petals, where they had always previously alighted. From the ease with which bees were deceived when the petals of *Lobelia Erinus* were cut off, it was clear that in this case they were not guided to the nectar by its smell; and it may be doubted whether they were attracted to the holes in the flowers of the Phaseolus by the odor emitted from them. Did they perceive the holes by the sense of touch in their proboscides, whilst sucking the flowers in the proper manner, and then reason that it would save them time to alight on the outside of the flower and use the holes? This seems almost too abstruse an act of reason for bees; and it is more probable that they saw the humble-bees at work, and understanding what they were about, imitated them and took advantage of the shorter path to the nectar. Even with animals high in the scale, such as monkeys, we should be surprised at hearing that all the individuals of one species within the space of twenty-four hours understood an act performed by a distinct species and profited by it." — (pp. 430, 431.)

But we must cut short our citations and remarks; passing by one of the most important points, relative to the amount of fertilizing work done by insects, namely, the evidence of the extraordinary industry of bees and the number of flowers visited within a short time; which, as well as the distance to which pollen is sometimes transported, is far greater than one would have supposed. But the volume is reprinting by the Appletons, and will soon be within the reach of all, — along with a new edition of the Orchid-fertilization book, the proper supplement to the present work, relating as it does to the class of plants in which the adaptation for fertilization by insects is carried to the highest degree of specialization and perfection.

THE HYBRIDIZATION OF LILIES.

Mr. Parkman, under the above title, gives a summary[1] of his experiments, during ten or twelve years, in crossing Lilies. One of the earlier results, and that which the horticulturists count as the eminent one, was the production of that magnificent hybrid between *L. auratum* and *L. speciosum*, with flower resembling the former in fragrance and form and the most brilliant varieties of the latter in color, which was brought out in England under the name of *Lilium Parkmanni*. The interesting physiological point which Mr. Parkman here records is, that this striking novelty was wholly unique; that all the other seeds of the same parentage which germinated, over fifty in number, gave rise to plants which in the blossom showed no trace of the male parent, *L. auratum*, but were exactly like the female parent, *L. speciosum*. That these plants were truly hybrids, notwithstanding, is well made out: 1, by the precautions taken against any possible access of own pollen; 2, by the scantiness of seed, most of which was abortive; 3. "such good seed as there was differed in appearance from the seed of the same Lily fertilized by the pollen of its own species," which is smooth, while this was rough and wrinkled; and 4, the stems were mottled after the manner of the male parent.

It would naturally be thought that this slight but evident impression of the character of the male parent might be deepened by iteration. That was tried next year, when the flowers of several of these plants were fertilized with the pollen of *L. auratum* precisely as their female parent had been fertilized. The result was an extremely scanty crop of seed, "but there was enough to produce 8 or 10 young bulbs. Of these, when they bloomed, one bore a flower combining the features of both parents, but though large, it was far inferior to *L. Parkmanni* in form and color; the remaining flowers

[1] *The Hibridization of Lilies.* By Francis Parkman, in Bull. Bussey Institution, ii., 1877. (American Journal of Science and Arts, 3 ser., xv. 144.)

were not distinguishable from those of the pure *L. speciosum*." The article records the results of various similar attempts to hybridize other Lilies. For instance, our *L. superbum* was pollenized with eight different old-world species. The result was, that capsules, apparently perfect, were abundantly produced; some of them contained nothing but chaff, others had a few imperfect seeds, still others gave a fair supply of good seed. From this seed several hundred young bulbs were produced. "But when these came into bloom, not a single flower of them all was in the least distinguishable from the pure *L. superbum*." Moreover, in this case (different from the other) "not one of the eight different male parents had imposed his features on his hybrid offspring. Not only in their flowers, but in their leaves, stems, and bulbs, the young plants showed no variation from their maternal parent." The experiment proceeded one generation farther. "In the following year I set some of them apart from the rest, and applied to them, as to their mother before them, the pollen of several species of Lilies. This time the seeds were extremely scanty. A few, however, were produced; but the plants and flowers that resulted from them were, to all appearance, *L. superbum* pure and simple."

In trials of other species results intermediate between these two cases were obtained. For instance, the pure white of the perianth of *L. longiflorum* came out unstained in the progeny raised by a crossing with *L. speciosum*, and the herbage was equally unaffected; but in that or the next generation "distinct evidence could be seen of the action of alien pollen" in the changed color of many of the anthers, and in the abortion of others. They also showed differences of habit among themselves, some being very tall and vigorous, and others compact and bushy, with a tendency to bloom in clusters; but these may have been mere seedling variations, with which the hybridization had nothing to do. Yet some of these marks correspond with known results of hybridization.

That offspring should partake unequally of the characters of the two parents is a matter of common observation. That in the genus Lilium the hybrid offspring should in forty

instances out of fifty take almost all its traits from the female parent, as Mr. Parkman has shown, is very remarkable. That, in not a few instances, it should take them all, so far as can be seen, — that the paternal influence should be represented by zero, — is most extraordinary. If parthenogenesis in plants were more unequivocally demonstrated, so as to be placed in certain instances quite beyond doubt (which is hardly the case), then we should regard the supposition which Mr. Parkman mentions as having been suggested to him, namely, that in the case of *L. superbum* the embryo was developed without male influence, to be quite as likely as the alternative of the progeny's inheriting everything from the female and nothing from the male parent; in fact the two suppositions approximate to the same thing. We are supposing the total absence of male parent's characters, and also that the alternative of fertilization by chance pollen of the species is absolutely excluded. Of this there is very high probability, yet not entire certainty. One of Mr. Parkman's "reasons for believing that parthenogenesis had nothing to do with the cases in question," namely, that some of the Lilies were young plants that never had bloomed before, has no application, but comes from a slight confusion of the idea of parthenogenesis with the effect in some animals of a previous male influence upon next succeeding progeny, which is quite a different thing.

The fact that more than one sort of hybrid may be generated between the same two species, copulated in the same way, must do away with the old mode of naming hybrids by a combination of the name of the two parents, that of the male preceding. The plan had the double advantage of indicating the origin of the cross, and of distinguishing hybrids from species in nomenclature; but in practice it proves insufficient.

PHYTOGAMY.

If this name has not been coined already it ought to be. For "the loves of the plants," so mellifluously sung by Dr. Erasmus Darwin in the days of our grandfathers, have been in our time, through a felicitous atavism, more scientifically, if prosaically, expounded by his grandson, in a series of articles and volumes, of which the subjoined are the principal titles.[1] If we have too long delayed our notice of these books, we make amends by calling attention to them at the season which invites and amply rewards the observations in field and garden which they suggest. Mainly in consequence of these writings, the subject which our new word connotes, namely, the connubial relations of plants, has become a popular and fruitful branch of biological science, which has its own laws and rules and technical terms, its distinction of legitimate and illegitimate unions, and tables of forbidden degrees. For example, it is not lawful, at least it is not *en règle* nor beneficial, for "thrum-eyed" Primroses to interbreed, nor for "pin-eyed" Primroses to interbreed. Such are illegitimate unions, seldom blessed with progeny. To the uncurious observer in Wordsworth's poem, —

> "A primrose by a river's brim
> A yellow primrose was to him,
> And it was nothing more."

But as concerns the Primrose, where seed-bearing is in question, if it be one of the thrum-eyed stock, the pollen brought to it must come from the pin-eyed, and vice versa, in order to secure full fertility. Tiny blue-eyed Houstonias, enamelling our meadows in early spring, and fragrant Mitchellas, carpeting Pine-woods in midsummer, are in a similar case. It is this kind of arrangement for cross-breeding to which

[1] *The Different Forms of Flowers on Plants of the Same Species.* — *The Various Contrivances by which Orchids are Fertilized by Insects.* Second Edition, revised. — *The Effects of Cross and Self-Fertilization in the Vegetable Kingdom.* By Charles Darwin. London and New York, 1876–77.

(The Nation, No. 667, April 11, 1878.)

the larger part of Darwin's latest volume on "The Different Forms of Flowers on Plants of the Same Species" is devoted. In such flowers — and they are rather numerous and of many families — the advantage of cross-breeding between different individuals of the same species is unquestionable, for it is essential to full fertility. The differences in structure, which consist of relative and reciprocal length of stamens and style in blossoms otherwise alike, have long been known; the meaning of it was one of Darwin's happy thoughts, and the confirmation is due to his labors. He demonstrated that the structure was correlated to the transport by insects of the pollen of the one sort to the stigma of the other, and that each pollen was inert, or nearly so, upon the stigma of the flower it belonged to, but potent upon the stigma of the other sort, upon which, in passing from blossom to blossom among the plants (of about equal number as to sort), the visiting insects are pretty sure to deposit it.

It is noteworthy that this significant dimorphism belongs to certain species of a considerable number of natural families, while others, sometimes even of the same genus, and in most of their species, show no trace of it; as if certain favored species had acquired a peculiarity in which their brethren have not shared. We ourselves call to mind some species in which this acquisition is either incipient or the correlations imperfect. But in his earliest work of the present series, on "The Various Contrivances by which Orchids are Fertilized by Insects," — a fascinating volume, which has recently been brought out in a second edition, — the "contrivances," as they may well be termed, are the common property of the whole order, although each genus seems to have patented a modification of its own. Here there is no dimorphism, but (with rare exceptions) all the flowers are alike, and all agree in having the pollen placed tantalizingly near the stigma, but prevented from reaching it, as well as in having some arrangement for the pollen's being transported by insects from one flower to another, ultimately from one plant to another. Wonderful arrangements, indeed, they are, which it requires a volume to describe, and of which we can here offer no details. Suffice it to say that,

in this great order, cross-fertilization must be all but universal as between different flowers of the same plant, and commonly between different individual plants.

In both these kinds of hermaphrodite flowers the practical separation of the sexes is hardly less than in Oaks, Willows, and other trees and herbs, in which the stamens and pistils occupy distinct plants or different blossoms. To these three classes, then, Mr. Charles Darwin's aphorism, "Nature abhors perpetual self-fertilization," undoubtedly applies. But there remains an equal number of plants with hermaphrodite blossoms, all alike, with no obvious obstacle to fertilization with their own pollen, while in many the adaptations are such as must apparently insure it, and indeed does very commonly insure it. Wherefore it is nowise surprising that self-fertilization was the orthodox doctrine — that there was thought to be a general adaptation for the falling of the pollen upon the stigmas of the same blossom. It is true that Christian Conrad Sprengel taught the contrary, in his work entitled "The Secret of Nature Discovered," published eighty-five years ago, and that he — mainly upon good observations — in a measure anticipated Mr. Darwin's aphorism; but he was accounted whimsical and untrustworthy by his own generation, and was forgotten by the next. Not so the contemporary "Loves of the Plants" — the hymnal of the old orthodox cult — which sings the —

"Gay hopes and amorous sorrows of the mead,"

in verse which our fathers were fond of, but from which we will not further quote. Had Dr. Erasmus Darwin known Sprengel's book, and brought to it the insight of the grandson, how different and how much richer the poem might have been. What curious facts and teeming fancies have been left unsung!

To H. Müller and to Hildebrand, two of Sprengel's countrymen, in our own day, may be credited the confirmation of the latter's thesis as respects the general run of hermaphrodite flowers; and this by showing what a large proportion even of these are functionally unisexual, either by the shedding of

their pollen before the stigma of that blossom is ready to receive it, or by the development and subsequent shrivelling of the stigma before the pollen matures, or by various other arrangements of like effect. And here, too, comes in the significant fact for the evolutionist, that these arrangements belong to widely different families, but only to certain of their species or groups of species, and not to their near relatives; also that they are more pronounced in some species than in others.

Yet, withal, there is much close-fertilization, and no one has demonstrated this better than Mr. Darwin, nor so well illustrated its meaning. The more particular and special the adaptations for cross-fertilization — depending, as they mainly do, upon insect-transportation, consequent upon visits for nectar or other floral products — the greater the chances of no fertilization through the failure of the proper insect visitation. So nature, not scorning a succedaneum, arranges for self-fertilization also as the next best thing, indicating her preference, however, by endowing the pollen with greater potency upon other stigma than its own; the principle throughout being to place the pollen where it will do the most good, all things considered. But Mr. Darwin insists, apparently with reason, that cross-breeding is the general plan, and close-breeding the subsidiary proceeding, or at least that no species of flowering plants is deprived of its chance of wide-breeding, or fails to receive the benefit of it for any long number of generations.

This assumes that wide-breeding is beneficial. The assumption is one which a teleologist like Darwin is bound to make, and which an investigator like Darwin is bound to verify, if possible. The assumption is that ends elaborately brought to pass in a large number of species, in a variety of ways, and by great nicety and exactness of adaptation, cannot be meaningless or useless — must somehow conduce to the well-being of the species. Happily, this inference holds equally good whether, with the old-fashioned teleologist, the word "end" denotes a result aimed at, or, as in Darwinian teleology, a result attained. The two senses are not contradictory, and, as concerns the validity of the inference, it matters not which

sense is adopted, or whether the two are combined. Darwin's investigation, undertaken to determine by experiments whether such crossing is beneficial, is published in the remaining volume of the series under consideration — that on "The Effects of Cross and Self-Fertilization in the Vegetable Kingdom." It does not fall within the scope and limits of this notice to set forth the nature and the extent of these experiments. Readers interested will go to the book, and probably have done so already. As to the results we may only say that, on the whole, they corroborate the inference — in some cases unequivocally and strongly, in others feebly, while in a very few the result was simply negative. While the crossing in many cases showed astonishing reinvigoration, and self-fertilization evident injury, the maximum good was obtained at the first or second crossing; and some close-fertilized plants soon became tolerant of that condition, and retained their fertility for several close-bred generations. If the Darwinian thesis was on the whole maintained, yet it was also shown that plants have many inexplicable idiosyncrasies, and that many unknown or obscure factors enter into the results of the experiment. On looking over the series we are reminded of the late Jeffries Wyman's aphorism: "No single experiment in physiology is worth anything."

It seems reasonably made out that the benefit of cross is, *cæteris paribus*, in direct relation to a certain difference in constitution between the two parents, or to some difference in their surroundings or antecedents, from which diversity of constitution may be inferred. The benefit is more decided when the parents come together from a distance than when grown side by side for several generations, and "a cross between two flowers on the same plant does no good, or very little good." The qualification is a proper one. It would be hasty to infer that it does absolutely no good, even though the advantage be inappreciable in any single instance. Still, however just and fairly well sustained the principle of Darwin's aphorism may be, it is confronted by the immense and seemingly endless vitality of long-propagated varieties which do not seed at all.

If we were writing a popular review of this volume on cross and self-fertilization, we should make much of the tenth and eleventh chapters, on the means of fertilization, and especially of cross-fertilization; on the plants which are sterile, or more than half-sterile, without insect aid; and, above all, on the habits of insects in relation to the fertilization of flowers. A closing chapter in the volume, on the Forms of Flowers, should also receive attention — that in which cleistogamous blossoms are discussed, namely, small and inconspicuous ones which never open, but are far more fertile than the showy ordinary blossoms of the same plants; for capital converse testimony, to the effect that all ordinary flowers are in primary reference to cross-fertilization, may be derived from the structure and behavior of these blossoms, in which the contrary intent is unmistakable. When nature means close-fertilization she makes her purpose manifest. Also, we should note that this cleistogamy is sporadic, affects certain families only, and certain members only of families not otherwise particularly related; so that this peculiarity also seems to be of special and apparently late acquisition. When we gather into one line the several threads of evidence of this sort, to which we have barely alluded, we find that they lead in the same direction with the clews furnished by the study of abortive organs: slender, indeed, each thread may be, but they are manifold, and together they bind us firmly to the doctrine of the derivation of species.

BENTHAM'S FLORA OF AUSTRALIA.

This volume [1] brings a great undertaking to a happy completion. The first volume was issued in the year 1863, and the work has made steady progress to the end. It is the complete phænogamous Flora of a continent, and the only one;

[1] *Flora Australiensis: a Description of the Plants of the Australian Territory.* By George Bentham, assisted by Baron Ferdinand von Mueller. *Roxburghiaceæ* to *Filices.* London, 1878. (American Journal of Science and Arts, 3 ser., xvi. 237.)

is worked up by one mind and hand, within a time and at an age which allows no sensible change of ideas or points of view, so that it is throughout comparable with itself. It is the work of the most experienced and wise systematic botanist of the day, and when we know that fully as much other work, of equal character, has been done within these fifteen years, it will not be denied that the author's industry and powers of accomplishment are unrivalled. No one else has done such good botanical work at such a rate. If, as some fear, the race of first-class systematic (phænogamous) botanists is destined to die out or dwindle, it will not be for the lack in our day of a worthy model.

In the concluding Preface, Mr. Bentham turns over to his able and equally indefatigable coadjutor, Von Mueller, the duty of incorporating addenda and corrections, and suggests the preparation of a methodical synopsis, for convenient use, especially in Australia, where such a handbook will be most helpful and needful. This trust, we doubt not, Von Mueller will duly undertake, and may be expected worthily to accomplish. His fellow-workers over the world are not unmindful of their great obligations to him in the development of Australian botany, and in rendering practicable the production of this "Flora Australiensis" which has been equally enriched by his vast collections and facilitated by his preliminary study of them.

Mr. Bentham now declines to undertake "a detailed examination of the relations, as well of the whole flora to that of other countries, as of its component parts to each other," referring instead to "the principles laid down by J. D. Hooker in the admirable essay prefixed to his 'Flora Tasmaniæ,'" but recapitulating shortly the general characteristics of the chief component parts of the present flora of Australia, the most peculiar one of any large part of the globe. Let us still hope that he may some day reconsider this determination, so far as to discuss in a general way the relations of Australian botany to the history of vegetation on the globe.

Peculiar as the Australian vegetation is, its treatment not rarely touches points which concern the student of the Ameri-

can flora. Especially interesting to us is the elaboration, in the present volume, of the *Gramineæ*, in which General Munro's matured views — as yet little known by publication — have passed under the independent consideration of a veteran general botanist, and in which the author's own conclusions regarding the morphology and terminology of the floral parts and their accessories are practically applied. We duly noticed Mr. Bentham's essay on this subject, and had to acknowledge that its conclusions are apparently incontrovertible.

Next to this order in importance is the order *Cyperaceæ*, upon the arrangement of which sound judgment is brought to bear. The great order *Liliaceæ* is made to include the *Smilaceæ*, and not the *Roxburghiaceæ*. We should have excluded both, but Smilax in preference. Contrary to Mr. Bentham's opinion, we should insist that the anthers in Smilax are unilecular but bilocellate. The diagnosis of *Roxburghiaceæ* in the conspectus distinguishes the order from Australian Liliaceæ only, and by an oversight the second genus of the order is said to be restricted to Japan, whereas it was founded on a North American plant.

DE CANDOLLE'S NEW MONOGRAPHS.

In this form[1] and way we may hope to see the Monocotyledonous orders elaborated, and some of the earlier Dicotyledonous ones re-elaborated. The middle of this volume is filled by the monograph of *Restiaceæ*, by Dr. Masters. This is an order allied on the one hand to *Juncaceæ*, on the other to *Cyperaceæ*, of twenty genera and two hundred and thirty-four species, wholly of the southern hemisphere, divided between South Africa (which has much the larger share),

[1] *Monographiæ Phanerogamarum Prodromi nunc continuatio, nunc revisio*, auctoribus Alphonso et Casimir De Candolle, aliisque Botanicis ultra memoratis. Vol. I. *Smilaceæ*, *Restiaceæ*, *Meliaceæ*, cum tabulis ix. Paris, June, 1878. (American Journal of Science and Arts, 3 ser., xvi. 325; xxxiv. 490.)

and Australia with New Zealand, and a single species in Chili. It is not a prepossessing family, and presents peculiar difficulties to the systematist, on account of the dicœcious character of most of them, and a striking difference between the plants of the two sexes, which in collections are hard to match. Much praise is due to Dr. Masters for his great labor, patience, and skill. The latter half of the volume is occupied by Casimir De Candolle with his neat revision of the *Meliaceæ*, chiefly a tropical order. The stamineal tube in the monadelphous *Meliaceæ* is concluded to be a staminiferous disk. The *Smilaceæ* by Alphonse De Candolle form the smaller but to us the most interesting part of the volume.

This order is restricted to three genera: two of them dicœcious, Heterosmilax with united sepals, no petals, and three monadelphous stamens (east Asiatic), Smilax with separate sepals, petals, and (6–15) stamens; the third, Rhipogonum (of New Zealand and Australia), with hermaphrodite flowers. Of Smilax one hundred and eighty-six species are characterized, and a dozen or two more are obscure or doubtful. There are thirty-eight pages of prefatory *generalia*, in De Candolle's best manner. We are pleased to find that he keeps up the *specific phrase*, and with true Linnæan curtness, relegating all particulars, not truly diagnostic under the sections and other divisions, to the description. In discussing the nature and characters of the leaf (which in its general sense is called "récentement et assez inutilement phyllome") the morphology of the petiolar tendrils has to be considered; the conclusion is that these answer rather to leaflets than to stipules, and the articulation, in some species well marked, between the blade and the petiole, or in the petiole, is noted as supplying good specific characters, which have been overlooked. The umbels are centrifugal or cymose. To distinguish, as is here done, the perianth into sepals and petals and to use these names when practicable, is most proper; but it hardly follows that the term perianth or perigone will then have no *raison d'être*. Whatever the number and position of the stamens, the carpels are superposed to the sepals, as indeed is the case in most Monocotyledons. It is pertinently noted that in Smilax,

always dicœcious, and with dull-colored perianth, the pollen is papillose as in most entomophilous flowers; but that Rhipogonum, the only hermaphrodite genus, has a smoothish pollen, more like that transportable by the winds. Most have odorous blossoms, some pleasantly, some the reverse. De Candolle asks whether in our Coprosmanthus (the name of which indicates the ill odor) this is common to both sexes and the same in both. Can any of our readers speak to this? An exposition of the geographical distribution of the order, and of what is known of it in a fossil state, is followed by a statement that all the four natural sections of Smilax and the two other genera — *i. e.*, all the types of the order — coexist in the comparatively small area comprised between the north of New Holland, the Fiji Islands, the Sandwich Islands, and Japan; that India has four of these six types, New Holland three, North America two, all Europe and Africa one; South America only one, but is rich in species. The speculative inference is, that, anterior to the eocene formations of Europe, the ancestors of the family occupied a continent situated in the region above indicated, of which the most ancient form was probably monœcious, gamosepalous, apetalous, monadelphous, and with more or less volatile pollen, — in short was like Heterosmilax; that this ancestor was in that region diversified, giving origin to the five other groups, beginning with Eusmilax, the widest diffused and most numerous in species, and finishing with Rhipogonum, which with Heterosmilax has clung to its birthplace. The sole Californian Smilax is referred, as a variety, to *S. rotundifolia*, but is nearer *S. hispida*, although distinct from both.

Vol. V. *Pars secunda: Ampelideæ*; by J. E. Planchon, has at length appeared. It occupies 350 pages; and it represents a great amount of labor, the permanent value and complete acceptance of which cannot be adjudicated off-hand. The plan of merging all the forms into one genus, Vitis, has been abundantly tried, not with very satisfactory results, — partly, it may be, because the groups have not been well worked out. Professor Planchon, a most experienced and keen botanist, who has especially investigated the Vines for a good

many years, has very naturally tried the other tack, and has developed the Linnæan Vitis and Cissus into ten genera. The principles upon which he has proceeded, as explained in the preface, are wholly legitimate; and one could wish that they have been successfully applied. This, only use can determine. We may be confident, however, that if this monograph had been in the hands of the authors of the latest Genera Plantarum, they would not have bodily adopted its conclusions, although they would have been much helped by the elaborate investigations, and might have seen their way to admit three or four genera. They would not have trusted over-much to the difference between polygamo-diœcious, polygamo-monœcious and partly pseudo-hermaphrodite, hermaphrodite and probably some pseudo-hermaphrodite, and hermaphrodite or rather physiologically polygamo-monœcious and with some blossoms pseudo-hermaphrodite, — differences which must be shadowy, — nor to variations in the mere shape of style and stigma. And as to the disk, which should be more tangible and hopeful, we gather from Planchon's synopsis and from our own observations that there are only three types. In the true Grapevines the disk is represented by nearly distinct and free nectariferous glands, alternate with the stamens. In most other *Ampelideæ*, it is cupular or annular (entire or crenate or lobed), with base or lower half more adnate to the base of the ovary, but at least the margin or lobes free. In the Virginia Creeper there is really no disk at all, as was first noted by Dr. Torrey in his "Flora of the Northern States," in 1824, and insisted on in the "Flora of North America," in 1838, and again in the "Genera Illustrata," where there are correct figures. Dr. Planchon expresses the same opinion in essence but in different language, *i. e.*, "Discus obsoletus ovarii basi plane adnatus et tantum colore proprio subdistinctus." We could not make much of the color; but the tissue does thicken more or less, and possibly may become obscurely nectariferous; but the flowers are not attractive to bees, as the allied species from Japan is. In the latter, while there is equally no hypogynous disk, there is much thickening of nectariferous tissue over all the lower part of the ovary

more or less in longitudinal ridges, the whole "plane adnatus" throughout. Now we should make more of these three types than Dr. Planchon does. For the first goes with the calyptrately caducous corolla and polygamo-dicœcious flowers of true Vitis. The third with disk, if so called, wholly confluent with the ovary itself, belongs to and includes all of the few known species (including Planchon's Landukia), which have the striking biological character of climbing by the dilatation and adhesion of the tendril tips; and their flowers are 5-merous, essentially hermaphrodite, and with expanding corolla. The second type of disk goes with 4-merous and some 5-merous flowers with corolla expanding in anthesis, that is, to the genus Cissus. We do not see the way to break this up into genera, certainly not on the number of parts, for this varies in some species, and while *C. stans* is 5-merous, the closely related *C. orientalis* is 4-merous. However it may be with some exotic groups, we must restore our two species, which formed part of Michaux's Ampelopsis, to the genus Cissus. Under that view the generic nomenclature is clear. The genus Ampelopsis (Michaux, p.p. and Torr. and Gray) is to be maintained on the lines long ago laid down in this country, and now reinforced, for those species which are popularly well known under this name. We do not feel obliged to defer to any work of Rafinesque as late as the year 1830. But, as to the present point, it seems to us that when Dr. Planchon followed him in the appropriation of one part of Michaux's Ampelopsis, he should also have adopted Rafinesque's name for the other part, namely, Quinaria, instead of making a new name, Parthenocissus, the former name being free for use. In our view both names are superfluous. As to true Vitis, it remains to be seen whether it will be at all possible to distinguish twenty or more North American species. Perhaps Engelmann allowed quite as many as can be defined. But Planchon's long and conscientious labors upon the genus and the family must be most helpful even where his conclusions are not at once accepted.[1]

[1] This is the last Review written by Professor Gray. — C. S. S.

EPPING FOREST.[1]

"EPPING FOREST, and How best to deal with it," is an article by Mr. Wallace in the "Fortnightly Review" for November, also separately issued, which should not be passed over as a matter of local concern. All Mr. Wallace's writings, even the most casual, will be found to touch and to illustrate some interesting question. A recent act of Parliament having decreed that one of the ancient woodland wastes, Epping Forest, which lies upon the very borders of London, shall be preserved forever as "an open space for the recreation and enjoyment of the public," the question what to do with it becomes a very practical one. It is not enough to say rejoicingly: "Here at length every one will have a right to roam unmolested, and to enjoy the beauties which nature so lavishly spreads around when left to her own wild luxuriance. Here we shall possess, close to our capital, one real forest, whose wildness and sylvan character is to be studiously maintained, and which will possess an ever-increasing interest as a sample of those broad tracts of woodland which once covered so much of our country, and which play so conspicuous a part in our early history and national folk-lore." Unfortunately much of it has been spoiled in all senses of the word. But Englishmen know how to plant, and the native trees which once covered the domain, with the undergrowth which of old accompanied them, could be made to flourish again. Probably the ancient forest could be essentially reproduced in all its former vigor, and former monotony. Mr. Wallace has something better than this in his mind, and his inspiration is caught from Professor Asa Gray's Harvard lecture on "Forest Geography and Archæology," which was published last summer in the "American Journal of Science," the ideas of which he adopts, happily summarizes, and applies to the case in hand. In re-foresting the open waste portions of Epping, he proposes to establish several distinct portions or broad tracts, each composed solely of trees and shrubs belonging to

[1] The Nation, No. 704, December 26, 1878.

some one of the great forest regions of the temperate zone. A climate of which it has, we believe truly, been said that it can grow treble the number of species of trees which the Atlantic United States can, and in which so many trees have been individually tested, offers favorable auspices for an undertaking of this kind upon a scale that may give a good idea of the features — not of this or that tree or shrub, but of a forest of the Alleghanies, of the Sierra Nevada, of British Columbia, and of Japan. Even the southern temperate zone may contribute from New Zealand its Kauri Pines and Beeches, under which Macaulay's overworked New Zealander may encamp on returning from his excursion to view the ruins of London bridge by moonlight.

When Mr. Wallace declares that "there is really no difficulty in producing in England an almost exact copy of a North American forest, with all its variety of foliage, with its succession of ornamental flowers, and with its glorious autumnal tints," we must agree that the experiment as a whole is hopeful, and much of it is already a success in piecemeal plantation. But we are not sure about autumnal tints under London skies, considering how much these differ between one season and another in New England. And, though every tree will grow in England, being put to no severe stress either in winter or summer, yet not every tree nurtured under our climate — so fierce in both seasons — will blossom in England, as witness our handsome leguminous tree, Cladrastis, or Yellow-wood. But a climate which will fairly nourish on one soil the trees of the Atlantic and the Pacific forests, those of Japan and Mantchuria, of Siberia, Himalaya, and the Caucasus, along with those to the manner born, deserves to possess them all. We, alas! can seldom grow on one side of our continent the trees and shrubs of the other. Moreover, there is very little forest east of the Rocky Mountains which an act of Congress could preserve; and, over that little, Congress and the Secretary of the Interior have lately been at loggerheads. Yet in California we have forests, still public domain, which are the veritable wonders of the world, which for the most part are doomed to irremediable destruc-

tion, but of which specimens ought to be preserved now when they may, now when it will cost nothing, and injuriously affect no man's interest. For the Redwood it is almost too late; yet a square mile, or half that area, of Redwood forest might still be reserved in Mendocino or Humboldt County. And from all accounts a square mile or two of true Big-tree forest, on or south of King's River, could well be set apart as a perpetual memorial. The Mariposa Grove is indeed such a reservation. But this is only a grove of a limited number of trees, many of them sadly injured by fires. Farther south this great tree is said to be the main constituent of extensive forests. A mile or two of Big-tree forest should be set apart before this district is invaded.

HOOKER AND BALL'S TOUR IN MAROCCO.

THE isolation of Marocco from the European world is strikingly shown in the fact that up to the publication of this goodly volume [1] the principal geographical authority for the interior of the country is Leo Africanus. This man was a Moor of Granada, who with his kinsfolk, at the time of the siege of Granada in 1492, fled to Fez, then the headquarters of Arabic culture, and was taken into favor by Mouley Ahmet, the founder of the dynasty still reigning in Marocco. He traveled throughout the empire, and wrote in Arabic a description of that part of Africa, which he must have had with him in manuscript when, in the year 1517, he was captured by Christian corsairs, and carried to Rome. The Pope, Leo X., hearing of this learned Moor, sent for him, treated him kindly, had him baptized, and gave him at the font his own names, Giovanni Leone; and so, as the writer of the narrative before us naively remarks, the Moor perhaps became as earnest a Christian as the pontiff himself. In Rome he translated his

[1] *Journal of a Tour in Marocco and the Great Atlas.* By Sir J. D. Hooker and John Ball. London and New York, 1878. (The Nation, No. 718, April 3, 1879.)

work into Italian; but it was not printed until 1550, in the first edition of Ramusio's "Collection of Voyages and Travels." Excepting Gerhard Rohlfs — who, by assuming the garb and professing the faith of a Mussulman, traversed districts where no Christian dare present himself, and who could make only surreptitious observations and trust them to memory — our British travelers were the first Europeans to travel in South Marocco and to reach the Great Atlas.

Their opportunity and their success were exceptional. It was a botanical expedition, in the spring of 1871, with the president of the Royal Society and director of Kew Gardens at its head, with Mr. Ball, a member, or at least an ex-member of Parliament, as his companion, and a third naturalist, Mr. George Maw, as their associate for half the journey. High influence was required for obtaining the Sultan's permission to penetrate the country at all; and utmost skill, determination, and no small assertion were called for to surmount the obstacles which were systematically interposed by the authorities, and to baffle attempts to lower the personal importance, and therefore frustrate the aims, of this scientific embassy. On reaching the city of Marocco the playing of the game began. The Sultan's letter instructed the governor of Mogador to "send the English *hakim* and his companions to the care of my slave El Graoui," said slave being the governor of the whole Great Atlas region. The city of Marocco was not in his province; and there the duty of providing for the sustenance and comfort of the travelers devolved upon Ben Daoud, governor of the city. Before entering the town our travelers had managed to learn that a very small house with only two rooms had been provided for them, and that Ben Daoud meant to make as little of them as possible. So a message was sent on that a larger house was needed, or else an enclosed garden in which to pitch their tents. In reply, a larger house with four rooms was offered. On reaching this house at nightfall it proved to be mean, dirty, and swarming; so when the *mona*, or present for the evening meal, came in, it was ordered back with scorn. It was felt that submission to any belittling at the outset would be the beginning of sorrows.

"Tell the Governor," said Hooker, "that my Sultana gives me a large house with a garden to live in; hospitality would require that the Governor of Marocco should provide me — the guest of his Sultan — with a better house; but, in any case, I shall not live in a worse one."

The messenger returned with the answer: "The Governor has no better house to give the Christians; but Marocco is large, and they are welcome to provide for themselves." Whereupon the cavalcade moved to the great square, or open space, beside the chief mosque and tower of the Kontoubia, sending at the same time a message to the Viceroy, son of the Sultan, that they should encamp in their own tents until a suitable house had been provided. The upshot was that on the following day the Viceroy installed them in the palace of Ben Dreis, with the adjoining garden, and soundly berated the city governor for his churlishness; but it leaked out that the poor governor had only obeyed the express orders of the Viceroy, who had directed him to begin by offering the mean house, then one somewhat larger, and to leave it to the Viceroy himself graciously to meet the higher demands if they should be insisted on. Fortunately, too, the row with the city governor threw the travelers into the hands and good graces of his rival, El Graoui, under whose protection and care they were to explore the Atlas range.

It was now full time to determine how this visit and the laborious journey they were to undertake were to be made intelligent and satisfactory to El Graoui and the other Moorish authorities. The gratification of a desire to learn something about the vegetation of the Great Atlas would have seemed a thin pretext for some sinister design. An insanity of this sort might possess the unaccountable soul of some one Christian; but that three should be simultaneously smitten with it would be thought to pass the bounds of probability. The pretense of collecting live plants for Kew Gardens could hardly be made plausible to the Moorish mind, except, haply, for that one use of plants that every one can understand. The Royal Gardens are literally the Queen's Gardens, and herbs that will cure diseases are among its most valuable collections.

Judicious representations upon this line would go far toward explaining the strange proceeding of the party of travelers, and would contain all the truth they were likely to get credit for. So "there is no doubt that the current belief among our own followers was, that the Sultana of England had heard that there was somewhere in Marocco a plant that would make her live forever, and that she had sent her own *hakim* to find it for her." And when it was seen what toil and hardship these botanical explorations entailed, the natural commentary upon the whole proceeding was: "The Sultana of England is a severe woman, and she has threatened to give them stick (the bastinado) if they do not find the herb she wants."

Those interested in the natural history of an almost unknown district lying on the very border of Europe; those interested in its geography and physical features; the statesman and philanthropist, who may here contemplate in a typical instance the decadence and depletion of a once prosperous people, and note the misery which two or three centuries of misrule may bring upon a smiling land; and those who enjoy the charm of fresh and graphic narrative, the sketch by a practised hand of keen and quick observations by practised eyes, — all these will find this volume attractive reading. To us it has been fascinating and full of suggestion. The main drawback to our travelers' enjoyment was the suffering which they unwittingly inflicted on the mountain villagers of the Atlas — the poor Berbers, who are altogether the worthier part of the population — through the insatiable rapacity of a large escort provided by the Moorish government for their protection. If the naturalists could have journeyed by themselves, with needful attendants under their complete control, their support would have entailed no hardship whatever; but the shameless extortion and abuse by the guards, which the travelers were unable to prevent, must have left a painful remembrance upon their minds as well as upon those of the despoiled mountaineers. It is hardly a consolation to the former that the latter are used to it.

The appendix to this charming volume is filled with scientific, philological, and geographical details. The most elabo-

rate article is that "On the Geology of the Plain of Marocco and of the Great Atlas," by Mr. Maw.

As to the orthography of the name of the city and country, Morocco is peculiar to the English. In the adoption of Marocco the authors follow, as regards the first vowel, the universal continental usage.

BENTHAM ON EUPHORBIACEÆ.

THIS thoughtful essay [1] presents the general views attained to by Mr. Bentham on working up the genera of the great order *Euphorbiaceæ* for the ensuing volume of the "Genera Plantarum." We need not specify any of the results, except to indicate the author's decision in the case of the *Buxeæ*. He does not follow his predecessors, Baillon and J. Mueller, who, much as they differ in other respects, agreed in setting up the order *Buxaceæ*, taking their cue from Agardh, and making much of the dorsal rhaphe. Bentham concludes that this small group, however well defined, ought not in a general view to be regarded as of higher grade than one of the primary divisions, or tribes, of *Euphorbiaceæ*. We are not the less pleased with this that we quite expected it.

A wider interest will be felt in Mr. Bentham's *excursus* on nomenclature, or rather on some questions which the study of *Euphorbiaceæ* brought up, and which some recent discussions have made pertinent. The general laws of nomenclature of our day, and the principles on which they rest, are laid down in the code which was reported by Alphonse De Candolle to the Paris International Convention, in the year 1867, and, being approved, was published with a commentary in the autumn of that year, and in an English translation early in the following year. The laws, without the commentary, were printed in this Journal for July, 1868. The ten years

[1] *Notes on Euphorbiaceæ.* By George Bentham; Journal Linnæan Society, xvii. London, 1880. (American Journal of Science and Arts, 3 ser., xvii. 335.)

succeeding have tested, somewhat thoroughly, the questions (nearly all of minor moment) upon which differing usages prevailed; and though one or two points are still mooted, the great majority of phænogamous botanists are coming to be of one mind and practice. But, as Mr. Bentham remarks: "The result has not been quite effectual in checking the ever-increasing spread of confusion in synonymy. Besides the young liberal-minded botanists who scorn to submit to any rule but their own, there are others who differ materially in their interpretation of some of the laws, or who do not perceive that in following too strictly their letter instead of their spirit, they are only adding needlessly to the general disorder. In the application as well as in the interpretation of these rules they do not sufficiently bear in mind two general principles: first, that the object of the Linnæan nomenclature is the ready identification of species, genera, or other groups for study or reference, not the glorification of botanists; and secondly, that changing an established name is very different from giving a new name to a new plant."

It is to the latter point that this most experienced and even-minded botanist addresses himself. "The rule that long-established custom amounts to prescription, and may justify the maintenance of names which form exceptions to those laws which should be strictly adhered to in naming new plants, is unfortunately now frequently ignored. . . . The law of priority is an excellent one; and when a genus or species has been well defined by an early botanist in a generally accessible work, but has subsequently been neglected, and the plant became known under other names, it is well that the original one should be restored. . . . On the other hand, it creates nothing but confusion to suppress a generic name, well-characterized and universally adopted by long custom, in favor of a long-forgotten one, vaguely designated in an obscure work, out of the reach of the great majority of botanists. . . . The greater number of Necker's genera have been so imperfectly characterized, with so absurd a terminology, that they are quite indeterminable; and his names deserve to be absolutely ignored, except in the very few cases

where Jussieu or other early French botanists have succeeded in identifying them, and corrected their characters; but even then it is doubtful whether these names should not bear the date of the correction, rather than of the original work. Adanson's 'Familles,' with all the inconveniences of its form and absurd orthography, is much more scientific, and many of his genera are well defined, and have therefore been properly adopted." . . .

Let us here interject a practical application. There is an old and well-established genus Smilacina of Desfontaines. There is a much older genus Tovaria of Ruiz and Pavon, founded in 1794, ever since accepted, and without a synonym. Recently Mr. Baker of Kew, finding that Necker has a Tovaria, published in 1790, and therefore four years earlier than that of Ruiz and Pavon, takes up this name in place of Smilacina, and leaves a new name to be made for the long-established homonymous genus. It will be said that the rule of priority demands the sacrifice, and that the identification of Necker's genus is sure, because the three Linnæan species of Convallaria which properly constitute Desfontaines' Smilacina are referred to it by name; and that, though it be a case of *summum jus summa injuria*, the injurious consequence is a necessity. But Mr. Bentham's characterization of Necker's work applies even to this instance. Twice over Necker's Tovaria is described as having a perianth of five sepals, and the berry is said to be one-celled. Desfontaines' Smilacina, on the other hand, is correctly characterized. Moreover, if we do not include this among those names of Necker which, Mr. Bentham says, "deserve to be absolutely ignored," we may yet find that the law of priority has another claim on it. In 1763 a much better botanist than Necker, namely, Adanson, founded a genus Tovara (essentially the same name as Tovaria) on *Polygonum Virginianum*, L., which is not unlikely to be taken up as a genus; and the name would supersede Necker's by the same rule that Necker's supersedes Desfontaines' Smilacina. All things considered, then, this is a case for the application of the homely but useful rule *Quieta non movere;* and much of Mr. Bentham's

pertinent advice may be condensed into this maxim. But there remain nice questions to settle with regard to the names and extent of the Liliaceous genus.

"The representing the Greek aspirate by an *h* was generally neglected by early botanists; but now, ever since De Candolle altered Elichrysum into Helichrysum, modern purists have insisted upon inserting the *h* in all cases; and this has been so far acquiesced in that it is difficult now to object to it, though it has the effect of removing so many generic names to a distant part of all indexes, alphabetical catalogues, etc. Admitting the propriety of adding the aspirate in new names, I had long declined to alter old names on this account; now, however, I find myself compelled to follow the current." Which is, on the whole, regrettable, especially as Alphonse De Candolle would hold out with him. See the latter's comment on his Article 66, in which the remark is dropped, that "we do not see why we should be more rigorous than the Greeks themselves." Oddly enough, these same writers who must supply the aspirate to the *e* omit it from the *r*, and write *rachis* and *raphe*, instead of *rhachis* and *rhaphe*, — which is exasperating to lovers of uniformity.

It is unnecessary here to cite Mr. Bentham's appropriate illustration of the indivisibility of the two-worded name of a plant. The proper apprehension of this, and of the paramount rule that no unnecessary new names should be given to old plants, will go far to rid the science of a principal remaining ambiguity in nomenclature. For it clearly follows that when a plant has a rightful name under its proper genus, the specific half of it is not to be changed because of any earlier specific name under some other genus, to which the plant does not belong.

HENSLOW ON THE SELF-FERTILIZATION OF PLANTS.

THIS paper[1] is elaborate, mostly able as well as ingenious, in all respects considerable, and unconvincing. Its thesis is the Darwinian "Nature abhors perpetual self-fertilization," read backward. It concludes that, "not only are the majority of plants self-fertilizing, but that those which are exclusively so propagate abundantly and with extraordinary rapidity, are best able to establish themselves in foreign countries, as, being quite independent of insects, they run no risk of extermination on that score; . . . that, so far from there being any necessarily injurious or evil effects resulting from the self-fertilization of plants in a state of nature, they have proved themselves to be in every way the best fitted to survive in the great struggle for life." The hypothesis is also advanced "that they are all degraded forms," and that therefore "their ancestral life-history is a longer one than that of their more conspicuous and intercrossing relations." We fail to see how this follows, except upon the assumption that the earliest phænogamous plants had the most highly organized blossoms; and that would not accord with vegetable palæontology.

Mr. Henslow rejoices that he has one stanch supporter; "for, as has been seen, Mr. T. Meehan has arrived at the same conclusion;" and indeed he builds not a little upon facts supplied by Mr. Meehan's observations. He cites the latter's "admirable paper, which was reproduced in the 'Gardner's Chronicle' for September 11, 1875, and is in fact an 'apology' for self-fertilization." As he then marshals twenty reasons for believing particular plants to be normally self-fertilizing, and nineteen "chief facts which may be regarded as occurring correlatively with self-fertilization, some being actual causes which directly or indirectly bring it about," it would appear that it is no longer self-fertilization, but

[1] *On the Self-Fertilization of Plants.* By George Henslow; Transactions Linnæan Society, 2 ser., Bot. i. London, 1879. (American Journal of Science and Arts, 3 ser., xvii. 489.)

rather the existence and *raison d'être* of cross-fertilization that stands in need of apology, or of explanation.

He freely concedes that the flowers of many plants, and some whole orders, are so constructed that intercrossing is for them a necessity; also that most of those which are believed "to be normally self-fertilizing" because they can and do fertilize themselves habitually, yet "may in some cases be cross-fertilized by insects." It is admitted that the structure of the latter is adapted — most variously and wondrously adapted — to being fertilized by particular insects. As this comes to pass in plants and flowers of the highest organization and greatest specialization, Darwin and his school conclude that this is a most advantageous outcome, and means some real good to the species; that when this is accompanied with a loss of self-fertility, it is the loss of something no longer useful, something better than self-fertility having taken its place. But Mr. Henslow, reading this the other way, having determined "that self-fertilization is *per se* a decided advantage," and free from injurious liability, comes to regard intercrossing as merely "a compensatory process for the loss of self-fertility."

But how and why did this "compensatory process" come to pass? It is conceived on both sides that flowers were "primordially inconspicuous." (To this Henslow adds hermaphrodite and self-fertile, but that need not here come into account.) Both agree that insects have mainly determined their conspicuousness. Darwin says this has been determined through natural selection by the survival of the more and more conspicuous variations, correlated with their producing something good for the insect of which the coloration was a sign, and that the preferential survival of the more showy and attractive was a consequence of some benefit of the intercrossing. Henslow propounds the view that insects have determined the conspicuousness more directly, and not by benefiting but by irritating the flowers. "These, by being greatly stimulated by the repeated visits of insects, tend to become hypertrophied. Hence the corolla enlarges, becomes more brightly colored, the nectariferous organs increase the quan-

tity of secretion, and the stamens develop more pollen. Such being the case, nourishment is withheld from the pistil, which is delayed in its development; consequently such a flower is very generally proterandrous." Mr. Darwin might accept this as an ingenious conception of the way the specialization comes about, still insisting on the advantage of the resulting intercrossing — "or else the thing would hardly come to pass," as the poet has it. And Mr. Henslow's hypothesis has to be supplemented to account for proterogyny, which is not much less common. But Henslow's supposed process works evil instead of good, and is therefore utterly anti-Darwinian and "dysteleological." For the result is a disturbance of the equilibrium and proper correlation between the andrœcium and gynœcium; and this, carried further, should upon this view result in the monœcious and diœcious states. So, accordingly, the cross-fertilization which comes into play in the case of separated sexes, and in that of self-sterile hermaphroditism, is not for any good there is in it *per se*, but because it may no better be. And all the elaborate, exquisite, and wonderfully various modes of adaptation of flowers to insects are only ways of repairing the damages inflicted upon blossoms by insects through their persistent visits! Did Mr. Henslow ever ask himself the question why the sexes are separate in animals?

The conclusion which Mr. Darwin has helped us to reach is, that intercrossing should be regarded as the aim in nature and on the whole most beneficial, and self-fertilization as a safeguard against the risks of crossing; that most hermaphrodite flowers have the advantage of both, the latter for immediate sureness, the former for ultimate benefit. Upon the new view, self-fertilization is the aim and the consummation, and cross-fertilization at best a succedaneum. By it insects may repair the damage they have caused to blossoms through endowing them with "the fatal gift of beauty," and stimulating their organs of secretion; and by it the winds may bring chance relief to those which, at length abandoned by their spoilers, have lost this attractiveness and fallen to the degradation of unisexuality. For these last, as has already been

stated, are hypothetically regarded as degraded from higher floral types.

We are bound to glance at some of the considerations which are adduced in support of this thesis. They are multifarious and of unequal value. As has occurred in other cases, so here also, the weightiest objections to Mr. Darwin's view are those which he has himself brought out, namely, the fact that, as tested experimentally under cultivation, while some plants are much increased in vigor and fertility by artificial intercrossing, others are not sensibly benefited; and that the benefit derived in marked cases is not cumulative, but reaches its maximum in two or three generations. And even close breeding under cultivation occasionally gives rise to very vigorous and fully prolific self-fertile races. Then many plants are fully self-fertile in nature, and it is not proved that any such have lost or are in the way of losing either fertility or vigor through continued inter-breeding. But, before drawing from this the conclusion that cross-fertilization is of little or no account in nature, it should be remembered that bud-propagated races are in similar case. Races exist which have been propagated only from buds for hundreds of years, with seemingly undiminished vigor, and there is no proof that any one has succumbed under the process. But for all that we do not doubt that sexual reproduction contributes something to the wellbeing of the species, besides facilitating its dispersion. Again, no one questions the necessity of fertilization by pollen to the production of embryo in the seed; yet, even in this, the necessity is not so imminent but that some embryos may originate without it.

In short, the facts brought out by Darwin and others, and all the considerations of the present essays, are best harmonized by the conception which the former has consistently maintained, namely, that an occasional cross suffices to secure the benefit of intercrossing, whatever that may be. Nothing yet appears which seriously disturbs our conviction that just this is what nature generally provides for.

Mr. Henslow's proposition, "The majority of flowers are self-fertile," is doubtless true in the sense that they are capa-

ble of self-fertilization, and is not improbable in the sense that they "can and do fertilize themselves habitually." But his inference that the majority of flowers, or that any flowers, actually propagate for a series of generations by self-fecundation, or that a cross if it occur is "exceptional," and of no account, is surely unwarranted by the evidence which he has adduced.

Occasionally the reported facts will not bear scrutiny. *Gentiana Andrewsii*, it is said, never opens at all in America. It opens in sunshine in the middle of the day here in New England. And while looking at closed flowers we have seen a humble-bee emerge from one. We have in this Journal shown how it is that self-fertilization is impossible during the first three or four days of anthesis, but neatly practicable afterwards. It is rash to infer (as on p. 330) that papilionaceous flowers which shed their pollen early in proximity to the stigma are therefore self-fertilized. In most of the cases adduced the pollen is not lodged upon the stigma, but upon the style below it, and the adaptations for intercrossing, though the mechanism be different, are as explicit as in the analogous case of Campanula. "Fremont pathetically describes the solitary bee that rested on his shoulder at the top of Pike's Peak." The pathos is wasted as respects all but this particular bee; for the entomologists find the alpine region of the Rocky Mountains to be as well stocked with flying insects as are alpine regions in other parts of the world. They do not super-abound, but if from the alpine flora we subtract the evidently entomophilous and the anemophilous blossoms, the remainder will be nearly nil. And as to the correlation of this comparative scarcity of insects with the marked conspicuousness of blossoms, this is the way the lesson is read by a most eminent physiologist: "Even the glowing hue of alpine flowers is accounted for by the attraction which brighter-colored individuals exercise upon the insects, scarce in those heights and necessary for fertilization."

One or two of the author's own observations are perhaps to be revised. "*Gaura parviflora* . . . has no corolla and is cleistogamous, in that it is self-fertilizing in bud, as I found

in specimens growing at Kew." Were they not imperfectly developed blossoms, perhaps late in the season? Here the flowers open freely, and have rose-colored petals. If he will examine fresh specimens of Scrophularia, it will soon be clear that his idea of their self-fertilization (p. 371) is a mistake. It is a mere slip in the "Genera Plantarum" through which abortive stamens are attributed to the cleistogamous flowers of Epiphegus. The authors evidently meant to describe the case just as Mr. Henslow found it to be, but used a wrong word.

"Weeds are probably all self-fertilizing or anemophilous. A weed is simply an unattractive plant, and possessing no feature worthy of cultivation." It may be as difficult to define "a weed" as to define "dirt." But, turning to the "Handbook of the British Flora," we find, as we expected, that the showy Corn Poppy, Cockle, and Larkspur are denominated weeds. Why weeds should possess the vigor and gain the predominance which they do is a large question, to which other solutions have been offered than that one which is in this essay very plausibly maintained. We cannot take up the topic here; but, without acceding to his general proposition, we are much disposed to agree with the author in this essay, as respects some of them, that aptitude for self-fertilization may have given them the advantage which has determined their wide dispersion.

The insistence upon the importance of self-fertilization is what gives this essay its value. As a whole it fortifies the proposition, well laid down by Herman Müller, which Mr. Henslow cites: "that, under certain conditions, the facility for self-fertilization is most advantageous to a plant, while, under other conditions, the inevitableness of cross-fertilization by the visits of insects is the more advantageous." But this is not our author's thesis. It comes to this: the plan of nature is either cross-fertilization supplemented by close-fertilization, or close-fertilization tempered by cross-fertilization. As restricted to plants the difference is not wide. Regarded generally, the Darwinian axiom is still best sustained.

PLANT ARCHÆOLOGY.

INVESTIGATIONS in fossil botany are recondite and technical, the materials generally unattractive, and the results unintelligible to the popular mind; but in Count Saporta's "Monde des Plantes,"[1] and under his happy exposition, the stony desert is made to rejoice and blossom as the Rose. The interest which we take in the vegetation of former periods is not so much geological as genealogical; and this interest diminishes with the distance from our own time and environment. We know nothing of the earliest plants — the beginnings of vegetable even more than of animal life are beyond our ken; no great satisfaction seems obtainable from the small acquaintance that has been made with the plants which flourished before the carboniferous period. And the botany of that age, notwithstanding its wealth of Ferns and its adumbrations of next higher types, impresses us as much with the sense of strangeness as of wonderful luxuriance. For even the fern-impressions, familiar as they may look to the unprofessional observer, are outlandish. The more the critical student knows of them the less likeness he finds in them, or in the coal-vegetation generally, to any species or genera now living.[2] But in the vegetation of cretaceous, and still more of tertiary times, familiar forms first come to view, and pedigrees may begin to be traced. Questions of ancestry touch us more nearly than those of history; so an enquiry into the source and parentage of the plants with which man is associated is more attractive than any question concerning the origin of the pristine vegetation of the earth. Moreover, our

[1] *Le Monde des Plantes avant l'Apparition de l'Homme.* Par le Comte de Saporta. Paris and New York, 1879. (The Nation, Nos. 742 and 743, September 18 and 25, 1879.)

[2] To those who wish to get a good coup d'œil of this vegetation from authentic records systematically arranged, we recommend the "Atlas to the Coal Flora of Pennsylvania and of the Carboniferous Formation throughout the United States," by Leo Lesquereux, an octavo volume of eighty-seven double plates, just issued by the Second Geological Survey of Pennsylvania. There is nothing else to be compared with it.

knowledge of the later fossil botany is comparatively full, — wonderfully so, considering how very recent this knowledge is, — and we are in a condition to apply it hopefully and confidently to the solution of problems which not long ago seemed to be beyond the reach of proper scientific enquiry, namely, to the explanation of the actual distribution of the species of plants over the earth. For the main data themselves, and for the clear exposition of them, we are most largely indebted to three men, who happily are still alive and active — Heer, Lesquereux, and Saporta.

The Linnæus and *facile princeps* of tertiary botany is Oswald Heer, of Zürich, now a septuagenarian, but still in harness. His "Recherches sur la Climat et la Vegetation du Pays Tertiare," rendered into French by C. T. Gaudin, was published nearly twenty years ago. It is a general and comparatively untechnical presentation of a long line of investigations, which have since been crowned by his several memoirs on arctic phyto-palæontology, now collected in the five volumes of his "Flora Fossilis Arctica." All these volumes, as well as others on the Swiss tertiary, have appeared within the last ten years, the latest only a year ago.

Leo Lesquereux, Heer's compatriot, and barely his junior, came to the United States fully thirty years ago, drawn hither from Neufchâtel by Agassiz. The greater part of his researches relate to the carboniferous flora, and he has recently thrown interesting light upon silurian botany, as has Dawson of Montreal upon the intermediate devonian. But those which at present concern us relate to the cretaceous and the tertiary of our own western regions. The most considerable of these works are the two notable quarto volumes, entitled "Contributions to the Fossil Flora of the Western Territories," published by the Geological and Geographical Survey of the Territories under Dr. Hayden, upon whom and whose survey they reflect high credit. One volume treats of the cretaceous, one of the tertiary flora.

Any proper enumeration of authorities upon the fossil botany of the later periods should include various other names, and especially that of Schimper, of Strassburg, who, like

Lesquereux, has divided his life between bryology and fossil botany, and whose classical "Traité de Paléontologie Végétale" is a systematic compendium of what was known of fossil plants up to the year 1874. But the volume now under notice is by a younger man, Gaston, Comte de Saporta, a Provençal, who has for fifteen years or more been investigating the rich tertiary deposits of Aix and vicinity, in the delta of the Rhone, the results of which have appeared from time to time in memoirs, mainly published in the "Annales des Sciences Naturelles." Besides these weightier and more technical publications, Count Saporta has contributed to the "Revue des Deux Mondes" and to "La Nature" subsidiary articles of a popular cast and of fine literary as well as scientific finish. These, now collected and re-edited, form a part of the volume before us, "Le Monde des Plantes avant l'Apparition de l'Homme," which has been published since the commencement of the current year. It is the most comprehensive and the most attractive, as well as the most recent, exposition of our subject, is a very readable book from beginning to end, of inviting typography, with abundant illustrations, both of woodcuts in the letter-press and intercalated plates. Although a popular, it is a truly scientific volume. The clear stream of the narrative is hardly at all troubled by the many technical terms which unavoidably strew its course, yet without obstructing its flow, for the author has the peculiarly French gift of happy exposition. As the volume is likely to be reproduced in English, let us hope that it may have a translator in whose hands it may lose nothing of its clearness, and as little as possible of its freshness and spirit.

To attempt a popular abstract of such a book would be like skimming the cream from the cream, and a critical review would cover too much or too technical ground. Still, we may give some general idea of the contents of the volume. The first part and the most discursive portion of the book is entitled "Phenomena and Theories." The introductory chapter discourses upon the introduction of life and the origin of the earliest terrestrial organisms; and the second chapter takes up the theory of Evolution or Transformism. We may skip

these chapters, yet without advising the reader to follow the example, unless he is already familiar with the topic — now a little threadbare — for, as a popular presentation, it is neat and sensible, though not profound. It hardly need be said that Saporta is an evolutionist, using the term in its general sense, and apparently a thorough Darwinian. A vegetable palæontologist who studies the later geological deposits cannot be otherwise: at least, he must needs be a "transformist." Saporta concludes that palæontology, if it does not furnish demonstration, yet gives irresistible reasons for a belief in evolution. The ground and the nature of this conviction appear in his rounded statement, that there is not a tree or shrub in Europe, in North America, at the Canaries, in the Mediterranean region, the ancestry of which is not recognizable, more or less distinctly, in a fossil state. This is too absolutely stated, no doubt, but the qualifications it may need will not invalidate the conclusion.

The chapter on ancient climates which follows, and forms a proper introduction to the second part of the book, is worthy of particular attention. It is prefaced by an elementary but very graphic exposition of the phenomena and laws of climates and their diversities, from the regular succession of equal days and nights under the equator to the contrasted condition toward the pole of a year composed of a day and a night season, separated by a season of twilight; the change so rapid in the high latitudes that, while the summer day at the North Cape is two months long, at Spitzbergen seven additional degrees of latitude lengthen it to four months. Let the author point the contrast between the two extremes, as affecting man, in his own language, here somewhat exceptionally ornate: —

"Il est vrai que dans ce dernier pays [Spitzbergen] le soleil s'élève au plus de 37 degrés au-dessus de l'horizon; il n'envoie que des rayons sans chaleur, *rolem inutile sine jure*; il éclaire de sa lueur pâle une terre glacée où frissonnent quelques plantes ensevelies sous les frimas, et qui ne sortent du sommeil qui les tient dix mois inertes que pour accomplir hâtivement leurs fonctions vitales et se rendormir de nouveau. Quel tableau, si l'on songe aux forêts

vierges du Brésil et de Java, aux vallées profondes du Népaul, aux savanes noyées de l'Orénoque, où la vie surabonde, où une lumière ardente, vive et dorée, ondule de toutes parts, soulève de tièdes vapeurs, joue avec l'ombre, et fait resplendir les formes des plus merveilleux végétaux! Sous les tropiques, l'homme se sent écrasé par une vie exubérante, il lutte incessament pour maintenir sa place au milieu de la nature, dout il est dominé; ses plus fortes œuvres sont envahies en peu de temps; les arbres immenses reprennent possession du sol, dès que celui-ci est abandonné à lui-même. Dans l'extrême Nord, la faiblesse de l'homme est encore plus évidente, mais c'est du poids de la nature inerte qu'il est accablé. Les éléments règnent seuls dans ces régions dévastées, où l'atmosphère se trouve livrée à d'épouvantables tourmentes. La neige dérobe les aspérités du sol, la glace couvre la mer d'un sol factice, souvent mobile et toujours dangereux; la confusion est partout, le calme nulle part; chaque pas est pénible, la vie elle-même devient un effort que l'energie la mieux trempée ne peut soutenir longtemps sans succomber." (p. 212.)

The modifications of this contrast through the actual distribution of land and water, winds and currents, are then considered. As these have a fixity secondary only to the fundamental elements of climate, — namely, the heat of the sun, the inclination of the earth's axis to the plane of its orbit, and the relative density of the atmosphere according to the elevation of the land-surface, — the climate of any part of the world might be supposed to have been constant, oscillations excepted, through the long periods that have elapsed since existing species or their immediate ancestors were introduced. It is not very long ago that Arago demonstrated, to his own and the general satisfaction, that there has been no appreciable change of the earth's climate in man's time. Plants are the thermometers of the ages, by which climatic extremes and climates in general through long periods are best measured. For at least five or six thousand years the Vine and the Date-palm have grown in proximity, and have furnished grapes and dates to the inhabitants of the warmer shores of the Mediterranean. Yet a very moderate change either way in the temperature would have excluded the one or the other. So Arago concluded that man had witnessed no sensible changes in the climate of Europe; a good conclusion, if re-

stricted within the limits of observation. But in Arago's life-time the evidence was already accumulating which has now proved that in earlier times man and the reindeer lived together on the soil of southern France, when if grapes could ripen in Syria and northern Egypt, dates doubtless could not. Then, at a still earlier day, Palms flourished in Switzerland and Vines in Iceland. Then Maples, Lindens, Plane-trees, Spruces, and Pines formed forests in Greenland up at least to the eightieth parallel; and, indeed, our own southern Cypress, or Taxodium — which now barely maintains its existence between the mouths of Delaware Bay and the Chesapeake — flourished along with the Silver Fir of Europe, within two hundred leagues of the North Pole. A climate in Greenland in which Sequoias, now confined to California, Magnolias, Persimmons, and Grapevines were mixed with Maples, Oaks, and Poplars, could not have been colder than, or much unlike, that of Indiana and Kentucky now.

As we have seen, the vegetable world has had an eventful history, and this history Count Saporta undertakes to reconstruct from ancient and authentic documents.

These documents have settled one point with certainty, namely, that the great changes in the temperature of the polar regions have not resulted from any change in the earth's axis of rotation, such as certain physical geologists have supposed. That this has remained steady throughout the whole period in question is as good as proved by the identity of the miocene and other tertiary fossil plants in all longitudes, from the Mackenzie River and Alaska round to Spitzbergen, Iceland, and Greenland — in part the same species, in all the same or equivalent combinations. Not indeed the same species in the same latitude any more than now, but such latitudinal distribution as to show that the curvatures of the miocene isotherms were quite analogous to those of the present age. Moreover, the monotony which characterizes the sub-polar vegetation of the present day — when the most of the species and the combinations of species occur all round the world — equally characterized it then when clothed with forest trees of a temperate zone.

Upon the data in hand, now much extended, and on the supposition that the same species of tree had not appreciably altered meanwhile in its relations to temperature, Heer long ago elaborately compared the miocene climates with those of our time, and Saporta corroborates his conclusions. For the northern regions the difference is said to be equivalent to 25 or 30 degrees of latitude — that is, we have now, say in Europe, in latitude 40° and 45°, and in Atlantic America, in latitude 38° to 40°, the temperature and the vegetation which then flourished at latitude 70° in Greenland. Grinnell Land, in latitude 82°, only two hundred leagues from the pole, had a forest of coniferous trees; among them — associated with a Poplar-tree, a Hazel, and a Birch — was the Silver Fir of Europe, and the Bald Cypress of the swamps of the southern United States. The same combination, minus the Cypress, must now be sought in the more elevated parts of central and southern Germany. The Sequoias and Magnolias and Persimmons of Greenland, in latitude 70°, mingled with Maples, Oaks, and Grapevines, have their representatives partly in Virginia and on the Ohio River, partly in California. The miocene of the southern shore of the Baltic had Laurels, Oleanders, and Camphor-trees, but no Palms, so far as is known, thus answering to the Mediterranean flora, but miocene Palms reached to Belgium and Bohemia; and Provence, on similar data, had then about the climate of the coast of Zanzibar.

Another conclusion which Saporta confidently reaches — and which indeed is reached from all sides — is that of a very moist quaternary climate. Looking back to this comparatively recent period from our own, everywhere the streams have dwindled. Through great river-beds shrunken streamlets now meander in insignificant channels; springs reach the surface much lower down the valleys than of old; and the "rivers without water" of Egypt and Syria, and the reduced level of the Dead Sea, are so many evidences of a dryness supervening upon a general humidity greatly in excess of the present. These fuller watercourses of themselves indicate a more temperate or mean climate, a more equal distribution of heat and cold through the year. Was this equable climate

frigid and rigorous or comparatively mild? Were Europe and the United States simply arctic and to be compared with the present Greenland and Spitzbergen, as the school of Agassiz maintains? Or should the comparison rather be made with southern New Zealand, where Tree-ferns almost overhang the terminal moraines of existing glaciers, in a climate which is neither cold nor warm? Saporta maintains the latter, and he is not alone. He insists that the high Alps and the Pyrenees are not the types of glacial Europe generally; that the arctic animals and plants, and the rigorous climate which we associate with these, belonged only to the close neighborhood of glaciers, but that the valleys below enjoyed a climate even milder than now, although vastly more humid. So, likewise, Mr. Ball (in a lecture recently delivered before the Royal Geographical Society of Great Britain) ventures to affirm that, even during the period of maximum cold, the highest ridges of the Alps were not completely covered with snow and ice; for we still see, by the appearance of the surface, the limit above which the ancient ice did not reach; and in the middle zone the slopes that rose above the ancient glaciers had a summer climate not very different from that which now prevails. And he concludes that the effect on the growth of plants in the Alps was to lower the vertical height of the zones of vegetation only one or two thousand feet.[1]

This would seriously affect the forests of Europe, but would not permanently disturb the alpine and sub-alpine vegetation.

Yet cold it must have been when the reindeer and musk-ox roamed over the plains of central Europe, and when the elephant or mammoth and even the rhinoceros which accompanied them, were equally clad with a thick coat of hair. But, says Saporta, with the remains of these very animals from which a frigid arctic climate is inferred, occur also, in the alluvia of the Somme and the Seine, those of an elephant nearly related to the Indian species, the hippopotamus of the African rivers, and the hyena of the Cape; and the vegetable

[1] But Mr. Ball is fairly astounding when he assumes that our arctic-alpine flora may have been the flora of high mountains at low latitudes in the carboniferous period.

remains include the Laurel of the Canaries along with the Vine. The trees of the same epoch farther north were Pines, Lindens, Maples, and Oaks. So, according to Saporta, even the glacial period formed only a seeming interruption to the general course, the steady and really unbroken diminution of terrestrial temperature from the earliest geological periods to the present. This must be admitted if the two classes of animals and plants — those adapted to cold and those to warmer climates — were really contemporaneous. Our geologists have maintained that they were not, but that climates have oscillated, and that warmer periods than ours intervened between the glacial epoch and the present, or were intercalated in the glacial period itself. But is not the distinction of periods an assumption for explaining the two kinds of fossil remains?

We need not enter here into the discussion of the cause of the higher temperature of ancient climates, and of that peculiar and temporary state of things attending and originating the glacial epoch, with which Saporta concludes his third chapter. And no space is left us in which to sketch even the outlines of the second part and main staple of his book, the history of the vegetable periods, beginning with the "primordial marine plants" of the Laurentian and closing with the pliocene, in which existing trees are everywhere identified. The general conclusion of these very rich, elaborate, and well-considered chapters is that the vegetation of the earth has been continuous through all ages, and that the explanation of the present is found in the past. The history of the genus Sequoia — of the two "big trees of California" — as recently sketched by Heer in a popular journal, "Das Ausland," is a fair illustration of this. The difference between these two trees is as notable as their resemblance and their isolation. They are the survivors of a numerous family, of wide distribution, which is first recognized in the cretaceous formation, in several species, and which reached its maximum in the middle tertiary, in fourteen recognizable species or forms. Almost from the first these separate into two groups, one foreshadowing the Coast, the other the Sierra, Redwood, yet with various intermediate forms. These intermediate species are

extinct, the two extreme forms have survived. The likeness of these two trees is explained by their genealogy, their marked difference by the extinction of the connecting forms which in earlier times bridge the interval.

SERENO WATSON ON NORTH AMERICAN LILIACEÆ.[1]

Mr. Watson, in preparing the *Monocotyledoneæ* for the "Botany of California," came upon the order *Liliaceæ*, which is well represented in Pacific North America; and he had to consider how the genera and higher groups should be disposed. This led to a wide study of the order and a strict scrutiny of the American species; and the present "Revision of the North American Liliaceæ," occupying the greater part of the "Contribution" before us, is the result. It is generally agreed that this order is to have the wide extension which was given to it by the present writer a dozen and more years ago; and the proper collocation of its diversified forms, with interlaced affinities, has been a problem of no small difficulty. Mr. Baker, in England, has attempted the task for the order generally, and has sedulously elaborated some of the North American, but more of the Old World and the South American, genera and tribes. His arrangement and his systematic views are in many respects satisfactory, in some unsatisfactory as respects North American botany. Mr. Watson has the latter primarily in view, but still has to adjust the American genera into the general system. The arrangement he has planned consists of three series, the first of which parts into two subseries, and includes sixteen tribes, some of them divided into subtribes. The great endeavor has evidently been to make natural groups — and this endeavor has been really successful. The next thing is to assign characters, and here comes the difficulty. Absolute characters of the lead-

[1] *Contributions to American Botany*, IX. By Sereno Watson; Proc. American Academy of Arts and Sciences. Boston, 1879. (American Journal of Science and Arts, 3 ser., xviii. 313.)

ing groups are not to be had, even when North American forms only are considered. Those who imagine they could do better than Mr. Watson has done should make trial before they criticise. The character of the pericarp, whether baccate or capsular, the nature of the stock, whether bulbous, tuberous, or rhizomatous, the nature of the seed-coat, the inflorescence, direction of anthers, union or separation of styles, are all good characters to a certain extent, and all fail to furnish unexceptionable marks to distinguish the higher groups when natural associations are sought. It is not easy to ascertain what diagnostic characters in this monograph are most to be trusted. But the nature of the bracts (on the one hand scarious, on the other foliaceous or none) takes the lead in the first two series, and is followed by the persistence or deciduousness of the perianth, the insertion of the stamens whether on the perianth or at its base, the dehiscence of capsule, — all matters of little physiological importance, but for that reason perhaps surer guides to affinity than the more prominent adaptive characters. However, it may be said that the first series answers to the *Asphodeleæ*, with Yucca and *Hemerocallideæ* added; the second to the true *Liliaceæ*, with *Uvularieæ* and *Trillieæ* added; the third to *Melanthaceæ*, with the tribe *Tofieldieæ* appended. Thus disposed, it is doubtless judicious to designate the three primary groups as "series," and not as suborders, and to throw the stress upon the tribes.

The Melanthaceous series, which in our view best divides into the *Colchiceæ*, *Veratreæ*, and *Tofieldieæ*, — the first not American, — is here divided into the *Veratreæ*, *Helonieæ*, and *Xerophylleæ* (which two we should combine), into the midst of which the *Tofieldieæ* are intercalated. This last tribe, which should end the series, is quite exceptional, and is well composed of Tofieldia, Pleea, and Narthecium. Its marks are the equitant leaves, introrse anthers with parallel cells, and caudate seeds; but to bring Narthecium under the remaining character of "styles distinct or none," it is defined as destitute of style, but with "the slightly lobed stigma sessile upon the attenuated apex of the ovary." This is really much

nearer the fact than would be supposed, as the cells of the ovary actually do taper up into the subulate style (as it has always and most naturally been termed), so that in the mature capsule the upper tails of the seeds reach up to within a short distance of the small stigma.

In a linear order it has not been practicable to approximate the *Convallariæ* of the first series with the *Uvulariæ* of the second. The division of Uvularia gives a gratifying opportunity of dedicating a New England genus to the memory of one of the best of New England botanists, the late William Oakes (*Oakesia sessilifolia*, with its relative of the southern mountains, *O. puberula*); but he would not have relished the dismemberment of the Linnæan genus upon the characters, good as they are, neither in fact do we. The formation of the tribe *Yuccæ*, of Yucca and Hesperaloë, strikes us as excellent; and it seems right not to adopt the supposed second species of Hesperaloë until it is better known. Its principal distinctions (longer anthers and shorter style) may indicate heterogone dimorphism, which would be a novelty in the order.

Tribe *Nolineæ*, of the first series, must be regarded as an excellent group, composed of Dasylirion and Nolina; and it is gratifying to find that the outlying genus Nolina, founded long ago on a single Georgian species, is the northern representative of a considerable Texano-Mexican group, named Beaucarnea. It were to be wished that the plan of this Revision had allowed more citation of generic synonymy, and that it had been more explicitly stated that Beaucarnea is only Nolina. This union, indeed, is one of the happy hits of the present monograph.

As has been suspected, the Californian *Schœnolirion album* of Durand proves to be quite distinct from the Atlantic species on which that genus was founded. So Mr. Watson has embraced the opportunity, here offered, to dedicate a peculiar Californian genus to Judge Hastings — a judicious patron both of botanical and legal learning. Except for his exertions, his own liberality, and his direction of the liberality of others, we could not have had the "Botany of California,"

which Mr. Watson may now soon bring to a completion. The reader finds no mention of this under the genus Hastingsia, p. 217, nor under the species *H. alba*, p. 242. But an appropriate reference is made on p. 286.

Leucocrinum, Nutt., was conjectured by Endlicher to be the Mexican Weldenia, but it has just now been ascertained at Kew that Weldenia is a Commelynaceous genus.

Our species of Allium as now worked out by Mr. Watson with great painstaking, are thirty-six in number, exclusive of the introduced *A. vineale*. Some characters might be made more of in living plants, such especially as those furnished by the so-called "crests of the ovary." In *A. stellatum* these crests are remarkably developed, radiating from around the base of the style and recurving, the notch at the end of each fitting over the base of the alternate filaments, and the under side is nectariferous and attractive to bees. The flowers are proterandrous.

In separating the two species of Maianthemum we should have unhesitatingly referred the large Pacific coast form to *M. bifolium*. We should not have distinguished *Lillium Grayi* as more than a form of *L. Canadense*, one which extends northward to the central parts of New York. In view of geographical range, size, and general appearance, we should never have thought of *Uvularia flava* as a synonym of *U. grandiflora*. Mr. Watson finds good characters in the shape and markings of the capsule to separate *U. grandiflora* from *U. perfoliata*. Has any one ripe fruit of the small, yellow-flowered *U. flava?*

Chamælirium Carolinanum, Willd. This specific name is properly restored. It was the original name under this genus; and the name *luteum* is a false one (though the plant was *Veratrum luteum* of Linnæus), the blossoms being white without a tinge of yellow, duller white in the female plant, pure white in the male, the pedicels equally of this color.

No space is left in which to notice the Notes upon the Affinities and Geographical Distribution of *Liliaceæ*, nor the Descriptions of some New Species of North American Plants, about fifty in number, which make up the second part of

this important "Contribution." Among them is a new Bolandra and a new Sullivantia from Oregon, both very much like (we fear too like) the original species. Here and in the Bibliographical Index, the name *Sullivantia Ohionis* is changed (perhaps accidentally) to *S. Ohioensis*. We knew of no law against genetive names of geographical more than of other places or stations, and such are not extremely uncommon. The name Ohionis was purposely chosen, and we hope may be retained.

The interesting new Erigoneous genus Hollisteria, discovered by the enthusiastic Mr. Lemmon (in San Luis Obispo County, east of the Coast Range), is of rather doubtful interpretation as to some points of structure. The inflorescence we suppose to be only seemingly axillary, the involucre is possibly a genuine trimerous one, and we take the two small stipule-like leaves to be real stipules, — a point which the published character does not decide, though it is implied in describing the leaves as alternate.

Being one of the most important of recent contributions to North American Botany, this publication deserves even a fuller notice than we can here give it.

DE CANDOLLE'S PHYTOGRAPHY.

Valuable as the present volume [1] is, it may very probably not be translated into English. So we propose to give a running account of its contents, adding here and there some brief comments, critical or otherwise. Treatises like this can be written only by botanists of long experience; and long experience, founded upon good training and accompanied by good judgment, gives the right to speak with a certain authority, particularly upon writing and publication in systematic botany, in which rules and method are most important. De

[1] *La Phytographie, ou l'Art de décrire les Végétaux considérés sous différents points de vue.* Par Alphonse De Candolle. Paris, 1880. (American Journal of Science and Arts, 3 ser., xx. 150, 241.)

Candolle is now one of our oldest systematists, one who as editor as well as author has had to consider every sort of phytographical question; and the volume he has here produced is a needful supplement to the "Philosophia Botanica" of Linnæus and the "Théorie Élémentaire" of the elder De Candolle, the two classical books which the serious botanical student should early and thoroughly master. Phytography has to do with form and method in botanical works; and natural history is nothing if not methodical. Its advancement by research and its educational value — which will be more and more appreciated as it is better taught — both depend upon correct morphology and upon well-settled method. Those who will not use its proper language and respect its customs, must not expect to be listened to, any more than is unavoidable. Observation and interpretation must go together, if either is to be of value; the naturalist must not only observe that he may describe, but describe if he would observe. In his preface De Candolle remarks upon the peculiar advantage of natural-history study in the combination of observation with judgment, and upon the importance to a student of acquiring a clear idea of what natural groups are, what a natural classification and the subordination of groups really mean, and how a naturalist arranges, names, and with precision defines the immensely numerous objects of his study. Men who have distinguished themselves in various professions and lines of life, have owed the advantage they have derived from this kind of training in youth, even though they never became naturalists.

De Candolle's book is in thirty chapters, many of them short and somewhat discursive, and generally abounding in recommendation and advice, rather than laying down positive rules.

The first chapter glances at "the evolution of botanical works" from Cesalpino, with whom scientific botany began in the middle of the sixteenth century, to Linnæus, whose rules and spirit still govern, and to our own times, noting the gradually increasing importance of herbaria as compared with botanic gardens. The second chapter touches upon the moral

and intellectual dispositions necessary in botanical work, and asks the question what manner of men botanists are or ought to be. As their pursuits do not lead to fortune, and professorships are neither numerous nor well paid, he concludes that botany is just the science for disinterested people to prosecute from pure love of knowledge and the pleasure of discovering something new; that it does not deal with questions of a very high order, nor require very difficult or absolutely rigorous reasoning. The faculties which it brings into requisition are the spirit of observation and of order, sagacity, and a certain good sense in the appreciation of facts; that, if it does not shine with great éclat, at least the faults of its cultivators are not likely to harm any one; that, equally with the other sciences, it tends to elevation of character in that it requires an ardent love of truth, reposing as it does upon the idea that the veracity of its cultivators is absolutely complete. He concludes: "Les sciences jouent dans le monde le rôle d'une école practique de bonne foi. D'après ces réflexions, il est permis de penser que les botanistes sont ordinairement et devraient être toujours des hommes paisibles, inoffensifs, indulgent pour les erreurs de leur confrères, et occupés bien plus de l'avancement de la science que de leurs intérêts ou de leurs petites glorioles. . . . Ne nous arrêtons pas cependant sur de rares exceptions. La presque totalité des botanistes est pénétrée du sentiment de la justice et des convenances. On en trouverait difficilement un seul qui ne reconnût le principe fondamental de ne pas faire à autrui ce qu'on ne voudrait pas qui vous fût fait."

Still, our author continues, sometimes the perfectly honest and right-minded botanist may have failings. He may, for example, neglect to cite his predecessors, or cite them inexactly, either from negligence (not to speak of calculated omissions, which show want of honesty and soon bring down reprobation), or from the want of literary resources. The latter case may be deemed a misfortune, and no fault. But, our author rejoins, if he has not the necessary books within his reach, why not go where they are and consult them? Or if unable to do that, why need he publish?

Some good advice follows about polemics and captious criticism; which we pass over, as seemingly superfluous, so long as the botanists are almost without exception such peaceable and good people. Something is said of the need of a right appreciation of the extent of the science; of the danger of exclusive devotion to a single branch of botany, in which one may lose all just perspective; and, finally, of what accuracy means in natural history as distinguished from mathematical exactness. Everywhere the naturalist has to judge as well as to measure.

The third chapter discourses upon the manner of preparing and editing botanical works, and the most advantageous modes of publication, considers the different degrees of publicity: for instance, complete and durable publicity is attained when a Monograph of an order or genus, a Flora, a Species, or a Genera Plantarum is published and placed on sale by the booksellers; or when an article or memoir is contributed to any leading and well-known botanical journal, or to the bulletin of a purely botanical society, which publishes with some regularity and indexes its volumes; or when printed in the transactions or bulletins of any scientific society, if separate copies in sufficient number are printed and fairly distributed or placed on sale. The usage in some learned societies of paging each memoir separately and placing it separately on sale is referred to, with implied commendation. Let us add that in all such separate issues, the original pagination of the volume should be scrupulously preserved; and it were better that there should be no other. Less complete, but durable publication is that of *ouvrages de luxe*, so limited in number of copies, and so high in price, that only a few libraries can possess them; also articles in journals without full indexes, or with indexes only to a series of volumes.

Incomplete publicity is given when papers upon botany are inserted in the voluminous transactions of general learned societies, of which few individuals can possess the series or find room for them; also articles in reviews, encyclopædias, and the like, treating of many or of all sciences. Even journals of natural history alone fall under the ban, unless divided

into separate parts for zoölogy and botany, — as is the long-continued "Annales des Sciences Naturelles" of Paris, and the "Journal of the Linnæan Society of London." A remedy or alleviation of these obstacles to publicity, and of others like them, is supplied by the catalogue of papers published by the Royal Society of London; which noble work was instigated by the late Professor Henry; yet, as this embraces all sciences and fills a goodly series of volumes, it can seldom be in the library of botanists.

In speaking of the obstacles to scientific publicity which are interposed by too limited editions, high prices caused by undue luxury in plates, and inopportune or inappropriate media of publication, De Candolle refers to customs in the book trade and in government patronage which need reform; and mentions incidentally what a botanical library costs. He says there should be by the side of every great herbarium and every considerable botanic garden, a special botanical library, without which it is impossible to determine exactly the plants of the one or the other, or to write any good Monograph or Flora. Such a library costs fifty or sixty thousand francs (ten or twelve thousand dollars), and needs about 12,000 francs for annual purchases. He asks how many such establishments there are in the world, and concludes that there may perhaps be between ten and twenty.

The section on the comparative superiority of certain kinds of works, sets forth the greater value of books or systematic works as compared with memoirs or articles.

The language to be employed in botanical publications is the topic of a special article. For descriptions, Latin, and the Latin of Linnæus. "Le Latin des botanistes n'est pas cette langue obscure et à réticences de Tacite, obscure et à périodes pompeuses de Cicéron, obscure et à grâces tortillées d'Horace, qu'on nous fait apprendre au collège. Ce n'est pas même le langage plus sobre et plus clair d'un naturaliste tel que Pline. C'est le Latin arrangé par Linné à l'usage des descriptions et, j'oserai dire, à l'usage de ceux qui n'aiment ni les complications grammaticales, ni les phrases disposées sens dessus dessous, ni les parenthèses enchâssées dans les phrases."

This for descriptions, except in local Floras, where popular use demands the vernacular; and we interpose the remark that English botanical language, freely incorporating as it does all Latin and Greek terms, comes next to Latin in convenience, compactness, and facility to all foreign botanists, who, being familiar with Latin, can seldom be at a loss. For discussions and reasonings, the botanists of each nation prefer their vernacular tongue; but De Candolle would restrict them to the four modern languages, one or two of which, beside his native tongue, every naturalist is nowadays supposed to be able fairly to read; English, German, French, and Italian. Indeed, De Candolle recommends Latin and the technically descriptive style even for *generalia*, on the ground of brevity; and he aptly suggests that the less capable botanists are of handling other than Linnæan Latin, the more brief, sententious, and strictly to the point their exposition will be.

Hints are given as to the best mode of collecting literary material, making and preserving notes (each upon separate slips of paper); upon the importance of adding clear explanations of drawings at the time they are made; and upon the desirability of refraining from publication until the work is thoroughly completed, but of then publishing as soon as possible. A manuscript work is said to have its maximum value at the moment of completion. Our author declares that second and third editions are seldom equal to the first. That depends. He objects also to posthumous publication, citing Roxburgh's "Flora Indica," published by Wallich, Plumier's plates, published by Burmann, and the wretched figures of Velloso; and he might have referred to the ill-advised printing of Griffith's rough notes and comments. But all depends upon the character of the manuscript and the length of time which has elapsed.

Chapters IV–XI traverse the whole subject of descriptions, under various aspects and a rather minute division of topics. As even a brief analysis would overpass available space, we will merely touch here and there upon certain points.

As to the relation of varieties to species, there are two modes of presentation, both of which have been followed by

Linnæus, and by most systematists, upon different occasions. Varieties are commonly designated by the small letters of the Greek alphabet, *α*, *β*, *γ*, etc., and also by names when they are pretty distinctly marked. Either the varieties, one or more, may be appended to the species (that is, to the form taken as the type, which usually must be the form originally described under the name), and therefore be treated as aberrant forms; or else the species is characterized as a group of forms, which forms are classified and defined just as species are under their genus. For instance, *Mentha Canadensis* is held to comprehend both a hairy and a smooth form, the two differing also somewhat in other respects. Linnæus founded the species on the former; and it is pretty well agreed that we are to refer the species back to him, however it be limited. Now we may either give a common character to the species, and then distinguish var. *α. villosa*, and var. *β. glabrata;* or we may characterize the species in general upon the originally named form, and append the variety *β. glabrata*. Either mode has its advantages and is likely to be employed in certain cases. The former classifies the varieties under the species, perhaps more naturally, and exhibits the polymorphous character of what we call a variable species; and De Candolle considers that it will prevail in proportion as the forms of a species come to be well known; the latter holds closer to the bibliography. There is danger of some misunderstanding when the two modes are used in the same work. In the "Synoptical Flora of North America," the former mode is invariably adopted, partly on the score of brevity. Either the originally described form, or a medium or common form is taken as the type, and the varieties are treated as departures from this. Even when the specific character is drawn so as generally to cover the varieties (as should be done as far as possible), some form, and the history of the species generally indicates what form, is kept in view as the norm or alpha. Of course, except for cultivated plants, there is no knowing and no pretence of determining which was the parent form, or in what order the several varieties may have diverged from a pristine stock.

As De Candolle points out, there is much ambiguity and looseness in the use of this word "type," which it would be well to avoid. Properly the type of a species is the species or genus, or the full idea of it, which no one individual or species may embody, which in the case of a group no single representative or member can fully exemplify. To apply the term to a form which well exemplifies the essential characters of the species or genus is quite natural, and hardly involves any confusion. But the term is also used in a historical sense, as referring to the particular form on which a species was founded, or the species on which the genus was characterized or which its founder had mainly in view, but which very often proves not to be the best representative of the group, sometimes not even a fair one. Finally a particular specimen which the original author described, or an authentic specimen, is said to be a type, or a typical specimen; and this De Candolle objects to. But after all, such terms can hardly he held to a single sense in technical any more than in ordinary language. Something must be left for the context to determine.

In drawing up the characters of groups, such especially as orders and genera are exceptions or what we call exceptions to be indicated in the character; or shall this express only what is generally true? De Candolle discusses the question, but leaves it, as must needs be, for practical judgment to determine. On the one hand the point or the usefulness of a character is blunted or dissipated by the intercalation of alternatives and exceptions, yet characters must be somehow made to correspond with the facts. The method of Bentham and Hooker, of a separate specification of the principal known exceptions, is commended.

Should outlying or anomalous groups be incorporated with the orders they most resemble, or be merely appended as "genera affinia," and the like? The latter was inevitable in the earlier days of the natural system; but increasing knowledge, as well as considerations of symmetry and convenience, more and more fix the place of these floating groups; so that their general incorporation into the orders by Bentham and Hooker in the Genera Plantarum of our day is in the natural

course of things. But botanists have to remember that many of them are still riddles.

De Candolle classes descriptions under the two general heads of *developed* and *abridged*. A developed description is a detailed account of the whole conformation, without regard to differentiæ. The type of an abridged description is the diagnosis, such as the specific phrase, or as Linnæus called it, the *nomen specificum;* what we now universally term the specific name being his *nomen triviale*. In the course of phytography both these have become rare or of special use as regards species, and a hybrid between the two has been engendered which is more serviceable than either. The long and independent descriptions of the olden time are now seldom written. Except for special cases, the development of the natural system in its subordination of groups in ever increasing numbers and definiteness, has rendered them superfluous. What was once stated in the developed description of a species in one formula, and a vast deal more, is now parceled out among the ordinal, tribal, generic, sub-generic, or sectional, sub-sectional, and other characters, each of which deals primarily, if not wholly, with *differentiæ*. The characters of each grade, being diagnostic, may be comparatively short; but taken together they become almost exhaustive. But to avoid going again over the same ground, subsidiary matters not diagnostical, yet needful or useful, are not rarely intercalated among the more essential points, instead of being collected in a separate paragraph. Consequently the specific diagnosis may be prolonged and get to partake of the nature of a developed description. The remedy for over-length is to multiply divisions and sub-divisions between the genus and the species. To do this well, to arrange the species group within group most definably as well as most naturally, tasks the powers and the patience of a systematic botanist, and tests his aptitude for discerning affinities, and solving practical difficulties.

Developed descriptions are in place in such general works as De Candolle's "Systema" (which was soon overweighted and crushed by them), and above all in monographs of orders or

genera. In his sixth chapter, devoted to this topic, the author cites, in order of date, the principal monographs of orders or tribes (excluding those of a single genus) which may be taken as models (about two dozen only), and points out some of their merits or defects. The subject of abridged descriptions is taken up in chapter VII.; and this connects itself with a great number of subsidiary questions and particular details, running on through twenty chapters more (individually short), and forming the most practically useful part of the book. There are so many points which it were well to call attention to, or sometimes to comment upon, for which space is now wanting, that we must defer the remainder of this critical notice to the next issue of the Journal.

It is to be regretted that, for the completeness of this work, the author did not comprehend in it the subject of nomenclature of groups — an important part of phytography — and reprint in it his opuscula, entitled "Lois de la Nomenclature Botanique," along with some further commentaries, such as his experience and some adverse criticisms from an opposing school may have suggested. This may still be desired, although the little treatise has already been widely disseminated in three languages, and although, as the author incidentally remarks, his own view is shared by an immense majority of descriptive botanists.

We proceed with our remarks upon this interesting volume, taking up certain points as they strike attention in turning the pages, but passing over many others of equal or superior importance.

There is a short chapter upon enigmatical descriptions or botanical riddles, and how they come about. The author has taken the pains to collect and tabulate the "species dubiæ" of the last four volumes of the "Prodromus," to see who is accountable for them, taking into account only botanical authors no longer living, and excluding those who have contributed no more than three. So good a botanist as Blume heads the list, one so indifferent as Siebold is accountable for the fewest; so not much comes from such a tabulation. The practical point is that Blume, as well as Miquel and Kunth,

who stand high on the list, have fallen much into the habit of founding species on fragmentary or quite insufficient herbarium specimens; instead of passing them over without mention, or at least without naming them. One is apt to suppose that a description of an incomplete specimen, say without flowers, may be readily eked out later by another hand supplied with the missing parts. This, as De Candolle says, is a mistake. The succeeding botanist is hindered more than he is helped by such work. And it is the same with species founded on figures, such, for instance, as those of Moçino and Sesse, upon which the elder De Candolle established species and some genera in the earlier volumes of the "Prodromus." As to "genera dubia vel non satis nota," very few can be laid at the door of first-class botanists. In the list of names of deceased botanists which are notable for their absence, the name of Torrey is inserted between that of the elder De Candolle and that of the elder Hooker.

In the chapter on the description of groups superior to species, the author enumerates and sketches the character of the six "Genera Plantarum" which have appeared within the 180 years of modern botany; the immortal works of Tournefort ("Institutiones," 1700), Linnæus (the first edition of whose "Genera" was published in 1737), A. L. Jussieu (1789), Endlicher (1836–1840), Meisner (1836–1843, which is much less known), and finally of Bentham and Hooker, which began in 1862, and is now three quarters finished. Tournefort fixed the rank and character of genera, Linnæus tersely and clearly defined them, Jussieu arranged them under natural orders, defining these, Endlicher and Lindley developed the hierarchy of groups superior to orders, also the tribes inferior to them, and the latter is deservedly praised for his sagacity in discerning affinities, the former for the perfection of his style; and to Bentham and Hooker is justly awarded the crowning merit of having, far beyond their predecessors in this century, verified or developed the characters of the genera by a wide and direct study of the herbarium materials.

Floras, or descriptions of natural groups, not in their entirety but so far only as represented in a particular country

or region, are discussed in chapter X. In Floras, as in more general works, abridged descriptions or diagnoses suffice, indeed are preferable in all cases where the region has been pretty well explored, and where the materials can be thoroughly elaborated. Formerly all considerable Floras were written in Latin, at least the characters. So they would continue to be if the convenience of botanists and the advancement of science only were to be considered. But Floras are used by many to whom even Linnæan Latin would be a stumbling-block. Fortunately the difference between good botanical English and botanical Latin is not wide, and will not seriously trouble a French or German botanist. The converse hardly holds. The greatest Flora written in English, we might say the best great Flora in any language which has ever been produced and completed, is Bentham's "Flora Australiensis," in seven octavo volumes. Touching upon works of special illustration, the "Botanical Magazine" is justly singled out for praise, for sustained botanical correctness under difficulties, and for its great influence upon the science.

De Candolle insists much on the importance of describing and well classifying the varieties of a species, and of distinguishing them as much as possible into grades, such as subspecies or races, varieties, subvarieties, etc. We suggest that this can be done with great advantage only when the forms are comparatively definite, or have been described as species. We think that only the more salient and definite varieties should be distinguished by names; otherwise the names and the groups will be limitless.

In the eleventh chapter, on partial descriptions of groups from the point of view of organography (a term which our author prefers to morphology), of physiology, botanical geography, etc., our author has some pertinent remarks upon the helps which all such studies are offering to phytography, which will gradually extend its domain over them; and upon the obvious advantage and great need of having results of histological researches expressed descriptively, under something like a common terminology, and with due regard to rules which have governed the more matured branches of

botany, — rules and practices which eliminate a deal of verbiage, facilitate comparison of views, and ensure mutual intelligibility. Of botanical descriptions for the purposes of systematic botany, it could be said that whatever is not clear is not botany. May such clearness be hoped for in the future of histological botany?

Chapter XII. treats of the unavoidable mixture of artificial with natural grouping. Truly natural groups are often artificially defined, that is, are indicated by single characters; or truly artificial characters are used for the sake of convenience in the division of natural groups. Of the latter sort are the divisions *Polypetalæ*, *Gamopetalæ*, and *Apetalæ* in Dicotyledons; also those founded on the mode of curvature of the embryo in *Cruciferæ*, introduced by Brown, who cautiously used them for genera, but raised to the rank of primary or subordinal characters by De Candolle. Hypogyny, perigyny, and epigyny are in the same category, and probably no one was more sensible of it than Jussieu himself, whose point and forte was the constitution of orders, not their collocation under these artificial heads. De Candolle suggests that, while to the more natural divisions are appropriated the terms of Class, Cohort, Orders, Tribes, Genera, and Sections; such names as Division, Subdivision, Series, etc., might be restricted to artificial divisions, and that these should take adjective names not of generic origin, such as *Ligulifloræ*, *Polypetalæ*, and the like.

Chapter XIII. relates to difficulties in phytography which have grown out of various methods or absence of method in the nomenclature of organs, and from the want of consideration of the law of priority in such matters. The result of which in some departments, such as histological morphology, is a state of anarchy not unlike that which prevailed in the names of groups before the days of Tournefort and Linnæus. We may hope that order and lucidity will some day dawn upon this chaos and a common language replace this confusion of tongues. Meanwhile De Candolle offers certain counsels, the utility of which, he says, is not doubtful nor the application very difficult.

(1) Hold fast to common and universally known names, whether in Latin or in modern languages. *Radix*, *caulis*, *folium*, *flos*, etc., with their vernacular equivalents, are not to give place to new-fangled substitutes. This, he thinks, will rid us of "such useless terms as *caulome*, *phyllome*, etc." Now these terms, along with *trichome*, seem to us legitimate and useful, as succinct expressions of a morphological idea; they are annoying only when pedantically ridden as hobbies over ground on which they are not wanted.

(2) Do not entertain the idea that a change in the mode of considering or defining an organ requires a change of name. Although Linnæus did take the leaf-blade for the leaf, and define it accordingly, that did not much hinder the coming in of a truer view, involving merely a change of the definition. But one may intimate that De Candolle here comes into conflict with another rule he insists on, namely, that terms should have unmistakably one meaning. When we say — as we ever shall — that leaves are ovate, we speak according to the Linnæan definition; when we say that their insertion is alternate, we use the word in a more comprehensive sense; when we have occasion to declare that cotyledons, bracts, petals, etc., are leaves, we use the word in the most comprehensive sense. All this involves considerable ambiguity; and the endeavor to keep the new wine in the old bottles causes no little strain. It is borne because it has been applied gradually. If Linnæus had started with, or even reached our ideas, we should happily have had a nomenclature to match. Now we must be content, for descriptive purposes, to employ some words both in a restricted and in a comprehensive sense, and let the context fix the sense, just as it must in ordinary language. Technical precision is only a matter of degree. But it is clear that the excellent rule here laid down need not forbid the introduction of terms to express our conceptions, such as *rhizome*, *caulome*, *trichome*, and the like. Yet these are ill-chosen terms, except the last. In particular, *rhizoma* has long ago been appropriated for something which is not of root nature, but the contrary.

(3) The third counsel is to change the name of an organ,

as we do that of a genus or species, only when it is positively contrary to the truth, or when it has been pre-occupied.

(4) Avoid giving special names for rare or ill-definable cases of structure. An epithet or short periphrasis is vastly preferable to a new and strange term, which will be seldom used and may be hardly understood. De Candolle truly remarks that after a great multiplication of terms and distinctions generally comes some good generalization, which does away with a crowd of particular names; that what has happened in carpology is likely to occur for microscopic organs; and he adds: "Nous assistons au 'feu d'artifice' d'une trentaine de noms de ces états des cellules" [in our vernacular, we have seen them "go up"]; "il en restera seulement quelques-uns généraux ou fréquents, qui seront toujours nécessaires."

(5) Between two or more names choose, not the most agreeable, or even the most significant, but the one best known and most widely recognized.

(6) Between names equally known and used, adopt the oldest. Which are the older names is not difficult to know in the case of common organs, but is very much so in modern histology.

(7) In this matter of priority or of usage, consider only names taken from [or in conformity with] Latin or Greek. As in systematic botany, scientific and not vulgar names are to be accounted in this regard. Those who like *spaltöffnung* for *stoma* or *stomate*, and *scheitelzelle*, must needs follow their own fashion; but the genius of our own and the French language resists their importation, while it adopts or adapts with ease technical terms from classical sources.

(8) Not to admit names contrary to these rules.

Chapter XIV. surveys some difficulties in phytography which arise from the variant, changed, or contradictory use of certain botanical terms, and from the employment of vernacular terms which cannot be latinized. The latter has just been referred to incidentally. Even the French describe the dehiscence of a certain kind of capsule as "en boîte à savonette." In English we do not attempt to say "in soap-

box fashion," and should not be understood if we did, but we adopt the Linnæan Latin "circumcissile." In general, De Candolle concludes that a vernacular term, whether the name of an organ or of a botanical group, which will not enter into a Latin text by a modification of its termination, is not scientific, and may give place to one which is.

A few terms are mentioned which have been more or less changed in meaning since the time of Linnæus; such as *lanceolate*, which has gradually varied more or less, and for a part of the change the present writer is held to account; also *glaucus*, which classically means sea-green in hue, but which has been generally used in botany to designate sometimes a certain whitishness, and sometimes a whitishness caused by a minute waxy exudation in the form of a powder: the latter is the same as *pruinosus*. Others may be as surprised as we were to learn that neither *glaucus* nor *pruinosus* are Linnæan terms.

Among the terms used ambiguously, it is surprising that De Candolle does not refer to *pistillum*, first introduced into botany by Tournefort, and used in the sense of the modern term *gynœcium*, therefore only one to a flower; modified by Ludwig to denote a female member of the flower (having ovary, stigma, and commonly a style), of which there may be several or many in a flower; and adopted in the latter sense by Linnæus, yet generally with a use that avoids contradicting the sense of Tournefort. Mirbel, Moquin-Tandon, and St. Hilaire among the French, have openly departed from Tournefort's use, and speak freely of pistils in the plural. Brown and De Candolle have used the word in the manner of Ludwig and Linnæus when they have used it at all, but have generally evaded its use; other botanists, especially British, have gone back to the Tournefortian sense of gynœcium. The present writer has a note on the subject in the new edition of his "Structural Botany" (1879 and 1880), p. 166.

Sinistrorse and *dextrorse* in the direction of ascent of climbing stems or the overlapping of parts in a bud, etc. — De Candolle had formerly insisted upon the desirability of following what he takes to be the authority and practice of

Linnæus in the use of these terms; and he here returns to the subject, reinforcing his former arguments. It is most desirable that these terms should not continue to be employed in contradictory senses, one party calling that sinistrorse which the other calls dextrorse; it is also fitting that the principle of priority should prevail and that the authority of Linnæus should be respected. Let us, therefore, in the first place give an abstract of the points which De Candolle here makes.

But first, we take it for granted that a stem or such organ, having no front or back, can have no right or left of its own: so when we say that it twines to the left or right, we can mean nothing else than the right or left of the observer. The contradiction comes from the different position which the observer is conceived to occupy. De Candolle supposes the observer to be placed within the coil or ascending helix, and that this is the more natural position. The other party supposes the observer to face the object from without; and from this position the Hop twines to the left, *i. e.*, turns in ascending from the observer's right to his left, while the Convolvulus turns from his left to his right; the first is sinistrorse, the second dextrorse; while to De Candolle, standing within the coil, the first is dextrorse, the second sinistrorse. Now, says De Candolle, Linnæus in the first edition (1751) of the "Philosophia Botanica," § 163, page 103, says: "Sinistrorsum hoc est quod respicit sinistrum, si ponas te ipsum in centro constitutum, meridiem adspicere; dextrum itaque contrarium."

De Candolle remarks that the phrase "meridiem adspicere" is of no account [but it indicates a certain confusion in Linnæus's mind], for it matters not in what direction you look. He adds — what we had all overlooked — that in the errata, on p. 360, Linnæus corrected the word sinistrum into dextram. But, inasmuch as two editions of the "Philosophia Botanica" were printed at Vienna in Linnæus's lifetime, and this correction was not introduced into them, he concludes that the correction was cancelled by the author of it. And he notes that the expression "sinistrorsum hoc est quod respicit

dextram" is a most awkward one for denoting the right-about change which the erratum had in view. Nevertheless the correction was so made in the edition of the "Philosophia Botanica" by Gleditsch in 1780, two years after the death of Linnæus, also in that of Willdenow, published ten years later. But De Candolle the elder, in the "Flore Française," and in all his writings, followed the original text, as also has the present De Candolle, who cites as maintaining the same view, Braun (who for a time gave way to the opposite), Bischoff, Mohl, Palm, Dutrochet, Nægeli, and even Darwin. But we should say that Darwin, noting the conflict of views, had carefully evaded both, using instead the expression "with the sun, and against the sun"; yet sometimes saying "from left to right," as equivalent to "against the sun" (as on p. 34), showing that he took the external position to be the natural one.

Among those who have used the terms sinistrorse and dextrorse and defined them in the way which supposes the observer to stand outside of the helix, are Aug. St. Hilaire, Duchartre, Bentham and Hooker, Eichler; and the present writer may be added, although our author appears not to be aware of it. While trusting that the younger botanists will follow the example of Linnæus and the majority of authors, De Candolle recommends that those who depart from it, and even those who adopt it, shall state their point of view by some convenient abbreviation, such as *extus vis.* or *intus vis.*; and thus lessen the danger of a misunderstanding. This is indeed essential.

De Candolle remarks that he can discover no reason for the *ab extra* point of view except a tacit but perhaps nowhere expressed assumption that it requires some effort to suppose one's self in the centre of a helix or spire. He thinks a moderate effort will accomplish this. The reply may be that, in the case of a stem climbing a hop-pole, or of the scales imbricated on the axis of a pine-cone, or of a flower-bud on the stage of a dissecting microscope, the contemplation of the object from without calls for no effort at all. So natural does this extraneous position appear to be that we found ourselves

describing these objects from that point of view without thinking of any other, — so natural, as we shall see, that Linnæus fell into it himself, and there remained. Yet, that the opposing view has also its fitness is obvious from the fact that the physicists and mathematicians are divided in usage, no less than the naturalists.

In the actual state of the case, the question which view ought to prevail in botany must be determined therefore on a balance of considerations: 1, Priority and authority, such as that of Linnæus; 2, Naturalness; 3, Prepònderant actual usage. We had maintained in this Journal (for March and for May, 1877) and in "Structural Botany" (6th ed., note on pp. 51, 52) that the *externe visum* view has decidedly the best case on the second ground, and, except in botany, on the third also. And now that De Candolle has drawn our attention to the matter, we are going to claim the remaining ground likewise, and to contend that the contrary usage in botany came in from non-attention to the teaching and practice of Linnæus himself.

On p. 39 of Linnæus's only own edition of the "Philosophia Botanica" he defines and illustrates the directions of twining thus: "*Sinistrorsum*, secundum solem vulgo: Humulus, Helxine, Lonicera, Tamus. *Dextrorsum*, contra motum solis vulgi; Convolvulus, Basella, Phaseolus, Cynanche, Euphorbia, Eupatorium."

Nothing is said about the position of the observer. But in every one of the examples of sinistrorse (Helxine being *Polygonum convolvulus*), the stem winds around the support passing from right to left of the observer confronting the coil; and in every one of the dextrorse examples (Eupatorium being Mikania) it winds in the opposite direction. That is, dextrorse and sinistrorse are used in the *externe visum* sense. On p. 103 the same is repeated, except that reference to the sun's apparent course is omitted and additional examples are added, most (but not all) of them accordant with the preceding. So far, it would seem that Wichura was not mistaken in his statement that De Candolle had followed a different method from that of Linnæus. And this appears to be the whole case as respects direction of twining.

But on the same page, to "Corolla sinistrorsum" is appended the foot-note which has made so much trouble, namely, "Sinistrorsum hoc est, quod respicit sinistrum, si ponas Te ipsum in centro constitutum, meridiem adspicere; Dextrorsum itaque contrarium." That is to say, in defining the direction of overlapping of the parts of a perianth, Linnæus took the open flower instead of the bud, and proposed to look down upon it from above or within. Now it may well be that Linnæus subsequently perceived the contradiction between his terminology for overlapping and that for twining; and that his brief erratum, on p. 310, "pro sinistrum lege dextram," was intended to bring the former into congruity with the latter, which it does, but in an awkward way. Perhaps he saw the incompatibility of the cited examples; in fact, about as many of them accord with the outside as with the inside point of view. Any way, the erratum is his own; it seems unlikely that he authorized its omission from the Vienna editions; and Gleditsch and Willdenow should not be blamed for heeding his behest in their editions. For, so far as it goes, it tends to render their author consistent with himself. If Linnæus had revised the page himself, he would have left out the "meridiem adspicere," which has nothing to do with the matter, and doubtless he would have completed his assimilation of the direction of petal-obliquity or overlapping with that of stem-winding; and so the whole confusion from which we are endeavoring to escape would have been avoided.

In adopting the external point of view — now fortified by original authority — it is well to note that we shall be in accord with the modern physicists and mathematicians, and also with common people. The ordinary screw, on which the thread winds from left to right of the confronting observer, and which is driven home by the semi-rotation of the hand and fore-arm from left to right, is everywhere known as the right-handed screw; and this, with the corkscrew, is taken as the norm and exponent of right-handed rotation by Clerk-Maxwell ("Treatise on Electricity and Magnetism," i. 23), and by Sir Wm. Thomson.

The analogies which have been adduced in favor of the

inside position are mostly drawn from objects which have a right and left of their own: a building, for instance, has a right and left side or wing because it has a front and a rear. The right side of an assembly presided over by an officer who faces the members is quite arbitrarily, but naturally, taken to be that on the right of the chairman. But the right-hand figures on a drawing or engraved plate are taken to be those on the right hand of the observer, notwithstanding that the plate, having face and back, has a right and left of its own.

Chapter XV. refers to certain difficulties which grow out of ambiguous terms of ordinary language: for example, the various meanings of the word (fin) end or purpose, and the ambiguities in the use of the terms nature, natural, supernatural (which lead off into philosophy, but are here treated rather in reference to style of exposition): also the change which has occurred in the scope of the word history in natural science.

Chapter XVI. is an interesting and pertinent one, upon the manner in which facts observed under the microscope are described, and on the great saving of space and advantage in clearness which would be gained by the adoption, for all matters perfectly capable of it, of the Linnæan descriptive style, and of Linnæan Latin. Extracts from the German of Schacht, the French of Payer, and the Italian of Gasparrini are given, and by their side a rendering in descriptive Latin; and the words and letters are counted. The German specimen so treated is diminished to considerably less than half the number of words and a little less than half the number of letters. The French simmers down to one third the number of Latin words and less than half the number of letters; and in the French of descriptive botany to less than one half. The Italian extract of 51 words and 256 letters is expressed in Latin of the Linnæan form by 21 words and 127 letters.

Style in botanical works is discussed in chapter XVIII., which all young botanists should study, especially the portion which treats of the admirable style of Linnæus. In speaking of botanical style in the modern languages, the author notices the great advantage which the languages of Latin stock have

inherited, and which the English-writing botanists have acquired, of ready and free use of Latin and latinized technical words by direct transference. Botanical French, English, and Italian, are contrasted with the German in this respect. Noting that the German of conversation inclines to be clear and sententious, while in botanical writings the words lengthen more and more and the sentences become badly involved, our author remarks that recently having read a couple of pages of Vegetable Anatomy, and feeling his brain somewhat fatigued with the frequency of such words as Sclerenchymfäsergruppen, Gefässbundentwicklung, and Entwicklungseigenthümlichkeit, he asked himself if that was good German style. He then recollected that Goethe, one of the very greatest of German literary writers, was also a profound naturalist. He opened his "Metamorphose der Pflanzen," read a page or so, and experienced a relief which he likens to that felt by a sea-tossed ocean voyager when the vessel suddenly glides into a quiet harbor.

Chapter XIX. discusses the propositions to employ letters and figures, chosen arbitrarily or otherwise, to represent specific and generic characters, — repulsive contrivances, to which our author lends no countenance.

Chapter XX. treats questions of orthography, abbreviations and signs, pagination, typography; the twenty-first chapter, of titles and indexes; both full of interesting details upon which we cannot touch, although we are longing to put in our oar.

Chapter XXII. animadverts upon the tendency of certain modern cryptogamists to set all botanical rules at naught. The next gives advice about articles in journals, dissertations, and the like; the next treats of translations; another, of figures, and has many noteworthy remarks; chapter XXVI., of auxiliary and bibliographical works; and chapter XXVII. is a chronological table of the progress of phytography, beginning with a Chinese encyclopedia 1000 years before Christ, and ending with Sachs's "Lehrbuch," 1868–1877. Botanical students will find it very interesting and instructive.

The remaining chapter begins the second part of the vol-

ume, "Preuves des Descriptions;" which is principally devoted to herbaria, their history, formation, and management; — a most important chapter, the analysis of which would form an article by itself. Last in order and not least in importance, a full enumeration is given of botanical collectors and authors who have formed herbaria, with an indication of the place where their herbaria or collections are preserved.

DARWIN'S POWER OF MOVEMENT IN PLANTS.

First let us congratulate the scientific community, no less than the author, that Mr. Darwin's experimental researches are seconded, and are we hope long to be continued, by the son whose name appears upon the title-page, and whose independent papers already published approve his worthiness for that honor. This volume[1] is from beginning to end a record of a series of researches and of the inferences which they directly warrant. Naturally it will not fascinate the general reader after the manner of "The Origin of Species" and some of the volumes which succeeded that epoch-marking production; nor has it the fresh charms of the treatises "On the Movements and Habits of Climbing Plants," and of "Insectivorous Plants," of which it is the proper continuation and supplement.

The organs of plants take certain determinate positions and execute certain movements, some of them universal or general, some of them special, some of them very striking and seemingly strange, most — but not quite all of them — evidently advantageous to the plant or essential to its wellbeing. Roots point toward the earth; stems point away from it; young stems bend toward the light, and the upper face of leaves is presented to it. Stems that twine "*circumnutate*" (a capital term), *i. e.*, bend successively to all points of the

[1] *The Power of Movement in Plants.* By Charles Darwin, assisted by Francis Darwin. With illustrations. London and New York, 1880. (American Journal of Science and Arts, 3 ser., xxi. 245.)

compass, and this wholly irrespective of external influences; and the twining around a support is the direct consequence of the circumnutation. Most tendrils freely circumnutate, and thereby are enabled to reach the object which they grasp. Most tendrils (and in certain cases some other parts) are very obviously sensitive to external contact or irritation, to which they respond by movement and change of form, and thus they grasp or do other advantageous acts. Some movements, especially of leaves, occur with regularity upon the access of light, others with its withdrawal; a few, such as the small leaflets of *Desmodium gyrans*, proceed irrespective of night and day. The specification need not be extended. The general facts in all their great variety are familiar to scientific readers. The inquiry of this volume is as to their ground and origin, or, as in this connection we should rather say, their development and history. For instance, circumnutation gives rise to twining and gives efficiency to other ways of climbing. But Darwin is bound to suspect, and even to show, that circumnutation is not a special endowment of the stems and tendrils of climbing plants, but rather a more developed manifestation of a general faculty. And the same is to be said of the movements of tendrils and leaves, or their appendages, whether automatic or in response to external irritation or stimulus. All this is what the experimental researches detailed in this volume undertake to ascertain and have satisfactorily made out.

An abstract of the volume might be somewhat tedious, and is certainly unnecessary for biological readers, who are sure to possess and study it. But the gist is readily to be gathered, without running through the iterated details or scanning many of the illustrative and curious figures which record the movements under investigation, by the simple perusal of the introduction and of the concluding chapter, in which the matter of the volume is summed up.

The sum and substance of the case is, that all these powers and faculties are manifested in the seedling immediately upon germination, and most of them are then remarkably exemplified. The caulicle or initial portion of stem below the

cotyledons (with the elongation and protrusion of which the germination of dicotyledonous seeds usually begins) circumnutates as soon as it comes out into the open air, and even earlier: this is the earliest manifestation of an automatic movement which is shared by all the succeeding portions of stem developed from it, in the early life of most plants, whether climbers or not. In the latter, and especially in twining plants, we see this general faculty at its maximum and in beneficial exercise. More remarkable and novel it is to learn that the initial root, growing from the lower end of the caulicle (not inaptly called by Darwin the *hypocotyl*), also shares in this faculty of circumnutation. As it penetrates the soil in its downward course, it cannot largely manifest this faculty, and indeed its power of circumnutation is always small; "but the circumnutating movement will facilitate the tip entering any lateral or oblique fissure in the earth or a burrow made by an earth-worm or larva; and it is certain that roots often run down the old burrows of worms. The tip, however, in endeavoring to circumnutate will (successively) press against the earth on all sides, and this can hardly fail to be of the highest importance to the plant" (being supplemented by another faculty, that of sensitiveness at the tip presently to be mentioned); for "when the tip encounters a stone or other obstacle in the ground, or even earth on one side more compact than on the other, the root will bend away as much as it can from the obstacle or the more resisting earth, and will thus follow with unerring skill the line of least resistance." Then, beside the almost universal heliotropic movement, by which each leaf or leaflet presents its superior surface to the direction of the greater light, Mr. Darwin shows that these organs also circumnutate, beginning even with the cotyledons or seed-leaves; although their sweeps generally form so narrow an ellipse that they move up and down in nearly the same vertical plane, a movement describing a circle being converted into one up and down.

These circumnutatory movements are of the most fundamental and therefore mysterious character. Although most commonly connected with growth, they are at bottom inde-

pendent of it. This — contrary to some German physiologists — we must conclude from both DeVries' and Darwin's investigations. They are produced by the changing turgescence of the cells on different sides of a stem or footstalk, which may or may not be fixed by some consequent growth or solidification. This Mr. Darwin, we presume rightly, concludes to be the faculty or susceptibility upon which heliotropism, geotropism, and the like (not to speak of *apheliotropism*, *apogeotropism*, *paraheliotropism*, *diaheliotropism*, *hyponasty*, *nyctotropism*, and other terms which the incautious student may take to be powers instead of abbreviated expressions) — in other words, upon which the solar rays and some occult influence of the earth — act, modifying the sweeps or converting them into forth and back or other special movements. Among these, that which has been termed the sleep of leaves, better and briefly designated by the word nyctotropism, is thoroughly investigated in this volume, and is shown to be far more general than has been supposed; and the conclusion is that the end subserved is a needful protection of the surfaces, mainly of the superior surface against cold from nocturnal radiation. *A priori*, looking at the structure of the leaf, one would have thought that the under surface had the greater need of such protection.

Not only are all these movements incipient in the seedling, but some of them are manifested more rapidly and extensively than in most mature plants. This should needs be, since, as Mr. Darwin states it, "Seedlings are subjected to a severe struggle for life, and it appears to be highly important to them that they should adapt themselves as quickly and as perfectly as possible to their conditions." Very properly, therefore, no small part of this volume is devoted to the seedling and to the behavior of its several parts. The most novel and unexpected results relate to the young root. Judging from its simplicity and from the medium in which it is developed, one would not look there for the endowment which Mr. Darwin finds in it. But this root-tip and the vegetable cells which compose it conspire to teach us that the most simple structures may be wonderfully gifted. The tiny root ex-

hibits three kinds of movement: first that of circumnutation, in which, endeavoring to bend in all directions, its tip "will press on all sides, and thus be able to discriminate between the harder and softer adjoining surfaces, . . . and to bend from the harder soil and follow the lines of the least resistance," so modifying advantageously its course from that to which geotropism constantly tends to give it. Moreover, the growing end of the root is sensitive to contact, and in a complex manner. If pressed above the tip, it bends there toward or around the impinging body, much as the end of a tendril bends around a support: thus it may follow, as roots do, along the unequal surface of a solid body. But, thirdly, if the tip itself be locally pressed, it exhibits different and more surprising sensitiveness, for it transmits an influence to an upper adjoining part, causing it to bend away from the affected side. This sensitiveness to contact is confined to a little more than one millimeter of the tip; the part which bends is 6 or 7 or even 12 millimeters above. So, when the sensitive tip in its downward growth strikes obliquely upon a stone or other obstacle, the part above at this distance, to which some influence must be transmitted, bends and carries the point away from the obstacle. Yet later, when a new portion of the side impinges upon the stone or other body, it will bend at that part toward instead of away from it, and so follow along its surface. It is the tip, likewise, that can discern that the air is moister on one side than on the other, and which thence "transmits an influence to an upper adjoining part, which bends toward the source of moisture." It is the tip only which is sensitive to gravitation. Well may Mr. Darwin affirm that there is no structure in plants more wonderful, as far as its functions are concerned, than the tip of the radicle. Also, that "it is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals." "But the most striking resemblance is the localization of their sensitiveness and the transmission of an influence to an excited part which consequently moves. Yet plants do not of course possess nerves or a central nervous system; and we may infer

that with animals such structures serve only for the more perfect transmission of impressions and for the more complete intercommunication of the several parts." The closing sentence of the book may be appended to this. "It is hardly an exaggeration to say that the tip of the radicle, thus endowed and having the power of directing the movements of the adjoining parts, acts like the brain of one of the lower animals."

The movements "excited by light and gravitation," as well as the nyctotropic or sleep-movements so called, are (as we have already stated) all referred by Mr. Darwin to modified circumnutation, "which is omnipresent while growth lasts, and after growth has ceased whenever pulvini are present," as in several classes of leaves. As respects the relation of external agents to the movements, note Mr. Darwin's remark: "When we speak of modified circumnutation we mean that light, or the alternations of light and darkness, gravitation, slight pressure or other irritants and certain innate or constitutional states of the plant, do not directly cause the movement; they merely lead to a temporary increase or diminution of those spontaneous changes in the turgescence of the cells which are already in progress."

Certain parts of plants turn or grow earthward. When this is attributed to gravitation, as it commonly is, the physicists have opportunity to complain of the misuse of a term. Although Mr. Darwin, like other writers, speaks of the influence of light and of gravitation in the same breath, without discrimination, we note with satisfaction his disagreement with those who "look at the bending of a radicle towards the centre of the earth as the direct result of gravitation," and note especially the closing dictum. "Gravity does not appear to act in a more direct manner on a radicle than it does on any lowly organized animal, which moves away when it feels some weight or pressure." Why, we would ask, need the word gravity or gravitation be used at all in this connection?

The introduction to this volume contains a short article upon the terminology which is adopted in it, chiefly as regards such words as epinasty and hyponasty, geotropism and

related terms, which is most convenient to employ, and also the names of these several parts of the embryo and seedling. This is, we believe, almost the first English book in which the axial part of the dicotyledonous embryo below the cotyledons (the radicle of the systematic botanist even of the present day) is distinctly recognized as hypocotyledonous or initial stem, although on the continent and in America this has long been taught and accepted. None the less so although the term radicle has been retained for it (until recently by the present writer, at least), in order not to break with the terminology of systematic works. Mr. Darwin, in this volume, shortens the expression of "hypocotyledonous stem" into the term "hypocotyl," — a fairly good English term, certainly better than the French *tigelle*. The objection to them both is that the words will not take the substantive Latin form, as all such terms should. Wherefore the better name — an old one which we have reverted to in the last edition of the Botanical Text-book (Structural Botany) — is *caulicle* or *cauliculus*. The initial root, which grows from the lower end of the caulicle (or "hypocotyl"), Mr. Darwin calls the radicle, following in this the ordinary English usage, except in very definitely distinguishing it from the cauline part above it. Being simply root, we have preferred uniformly to call it so, thus avoiding a word which the systematists have all along applied to the caulicle. Although initial stem and initial root are most clearly discriminated in the present volume, yet in the accounts of the germination or the ordinary Dicotyledons, it appears to be implied or stated, either that it is the root-part which first projects from the seed-coats and that the stem-part begins its development later, or that the axial part of the embryo conspicuously preëxisting in the seed is root and not stem. We take it to be quite otherwise, namely, that this axial part in the seed is cauline, and that ordinarily it protrudes or makes some growth in length before root-formation begins.

A few misprints of names of plants will in no wise mislead or trouble any botanist, except possibly in the case of *Apium graveolens*, which on p. 422 and 424, and in the index, is printed Apios.

DE CANDOLLE'S ORIGIN OF CULTIVATED PLANTS.

M. Alphonse De Candolle's "Géographie Botanique Raisonnée," in two volumes of nearly 700 pages each, was published in the year 1855, and has been for several years out of print. It is not surprising that the now venerable but still well-busied author should decline the labor of preparing a new edition, involving, as it would, the re-discussion of certain questions under changed points of view, and the collocation of a vast amount of widely scattered new materials which the last quarter of a century has brought to us.

Happily, the chapter on the geographical origin of the species of plants generally cultivated for food, and for other economical uses, could be detached. This, the author has sedulously studied anew; and the present volume[1] is the result. As yet we have it only in the original French; but it is said that an English translation is in preparation. So, if the work is not already in the hands of botanists and other scholars generally, we may expect that it soon will be; and, contenting ourselves with a mere mention of its plan and scope, we may proceed to remark, here and there, upon points which strike our attention.[2] We may expect this to be for

[1] *Origine des Plantes Cultivées.* Par Alphonse De Candolle. Paris, 1883. (Bibl. Scientifique Internationale, XLIII.) (American Journal of Science and Arts, 3 ser., xxv. 241, 370; xxvi. 128; with J. Hammond Trumbull.)

[2] To avoid repetition, it may be mentioned here that, in the following annotations, the "Relations of the Voyages" of Columbus are cited from Navarrete's "Coleccion de los Viajes," etc. (Madrid, 1858, and 1827–37); references to Peter Martyr d'Anghiera's first three Decades "De Rebus Oceanicis et Novo Orbe" (written before 1517) are to the Cologne edition of 1574; references to Oviedo's "Historia General y Natural de las Indias" — of which the first nineteen books, published in 1535, included a revised and enlarged edition of his "Relacio sumaria de la Nat. Historia de las Indias," printed in 1526 — are to the edition published by the Royal Academy of History of Madrid, 1851–55; Jean de Lery's "Histoire d'un Voyage faict en la terre du Brasil" (in 1557–8) is referred to in his revised edition in Latin, "Historia Navigationis in Brasilian" (Genevæ, 1586); Fr. Hernandez, "Nova Plantarum, etc., Historia," in the edition of

many years the standard work upon the subject, and to undergo revision in successive editions; and we are sure that the excellent author will welcome every presentation of discussion which may chance to throw any new light upon the sources or the aboriginal cultivation of certain plants which the Old World has drawn from the New.

The first part of the volume, of only 22 pages, is mainly occupied with a consideration of the means employed for the determination of the sources whence the various cultivated plants have been derived. The botanist enquires where a given cultivated plant grows spontaneously, or what was its wild original; and he has to judge, as well as he can, where it is truly indigenous or where a reversion from a cultivated to a wild condition. This, as respects weeds and the like, is a difficult matter, even in a newly settled country like North America, much more so in the Old World; but as respects the plants of agriculture, the case is usually simpler. The botanists resident in a country are not likely to be far misled by the occurrence of wilderings; but, in the case of travelers and collectors, perhaps too much has been made, even in this volume, of plants only once met with growing spontaneously, and inferred to be indigenous. Plentifulness is of no account, else the Century plant and Opuntia would be thought indigenous to the Mediterranean region, the Ox-eye Daisy to the United States, and certainly the Cardoon to the Pampas, where there is now probably more of it than anywhere in the Old World. Archæology and palæontology are often helpful, as by the identification of fruits and seeds in ancient Egyptian tombs, or of paintings upon their walls, or of fragments in ancient bricks; or the débris of lake-dwellings rescued from lacustrine deposits, as in Switzer-

Rome, 1651; "Rariorum Stirpium Historia" by L'Ecluse (Clusius), in the first edition, Antwerp, 1576; his "Exotica," including his translations of Monardes and Acosta, Antwerp, 1605, with his "Curæ Posteriores" (posthumous), 1611. — J. H. T.

Il va sans dire — yet should explicitly be said — that all the historical and philological lore, which gives this article its value, is contributed by my associate. — A. G.

land; and from the tufas of southern France, the kiökenmöddings of Scandinavia, the mounds of North America, and the ancient monuments and tombs of Mexico and Peru. Historical documents are also important for the date of certain cultures in particular countries; and here it is stated that the principal cultures have come from three great regions, namely: China, southeastern Asia and Egypt, and intertropical America. De Candolle also remarks that in the Old World agriculture was developed along rivers, in the New, upon plateaux, — a fact which he attributes to the primitive situation of certain plants worth cultivating. But this is not quite obvious. Linguistic learning may be turned to much account; as in tracing a plant toward its home by the name which has gone forth with it in all its migrations. Like other instruments this must be used with some knowledge and judgment. Blé de Turquie (maize) did not come from, and probably not by way of, Turkey, any more than did the animal of that name. Jerusalem Artichoke has naught to do with Jerusalem, but came from North America, and is no artichoke. Pomme d'Acajou, anglice Mahogany-apple, is neither an apple nor a pomaceous plant, nor has it anything to do with mahogany. New Zealand Flax came indeed from New Zealand, but is not a flax. Among errors from the careless transference of names from one plant to another, that of Potato, which belongs to the Batatas or Sweet Potato, is familiar. Of mistakes which have been made in the transference of a popular name from one language to another, De Candolle mentions the Arbre de Judée of the French, which in English has become Judas-tree. We may add that of Bois fidèle, of the French West Indians, which, taken up by their English successors as Fiddle-wood, has been perpetuated in the generic name Citharexylum.

The several lines of evidence — botanical, archæological, palæontological, historical, and linguistic — may be used to supplement or correct each other. How they may be brought to bear, and how their combination may give satisfactory results, is practically shown in Part II, — a study of the species as regards their origin, their earliest culture, and the principal

facts of their dispersion, — which makes up the principal bulk of the volume, namely, from page 23 to page 350.

This part is divided into chapters, *e. g.*, Plants cultivated for their subterranean parts, such as roots, bulbs, tubers, etc. Those cultivated for their herbage, whether for human food, for forage, for fibres, for stimulation, etc.; but the proper medical plants are left wholly out of view, as likewise plants cultivated for ornament. So the chapter on plants cultivated for their blossoms, or parts connected with these, is brief enough, treating as it does only of the Clove, Hop, Safflower, and Saffron. For the Rose, *Acacia Farnesiana*, and all plants however largely cultivated for perfume or for essential oils are left out of view. So also are the sweet-herbs of the kitchen-garden, and all condiments, except Horse-radish. Plants cultivated for their fruits and seeds occupy the closing chapters. Among the latter the Cotton-plant is placed. The arrangement matters little, and that adopted may be the most convenient. A good index makes ready reference to any topic.

In the order of the book we come first to *Helianthus tuberosus*, the Topinambour of the French, Jerusalem Artichoke of the English; in the United States the tubers simply called artichokes. Almost all we know of the origin and source of these esculent tubers has been recovered since the publication of De Candolle's earlier work, in 1855. Although the contemporary accounts specified its introduction from Canada, and Linnæus so cites it in the "Hortus Cliffortianus," the subsequent reference to Brazil was followed without question down to De Candolle's "Prodromus"; and the present author, in the work above mentioned, doubted the Canadian as well as the Brazilian origin. It now appears that Schlechtendal (in Botanische Zeitung, 1858) was the first to recover a part of the documentary history. Our own article on the subject — to which there is nothing of importance to add — was contributed to this Journal for May, 1877.[1] Singularly, it has

[1] In it reference was made to Lescarbot's mention of roots "grosses comme naveaux . . . ayans un goût retirant aux cardes," etc., and cited his "Histoire de la Nouv. France," in the edition of 1612 (p. 840). In a subsequent edition (1618), cited by M. De Candolle, Lescarbot adds that he

remained unknown to De Candolle, although it is referred to at the close of Decaisne's independent and exhaustive article, in the "Flore des Serres," 1881.

It can now be said that the wild plant to which *Helianthus tuberosus* has been traced is not *H. doronicoides*, Lam., although it was confounded with that species in Torrey and Gray's "Flora." Lamarck's plant is a sessile-leaved species. Decaisne's remark that *H. tuberosus* is the only species of the genus which is at all tuberiferous may be qualified. A form of what appears to be *H. giganteus*, but is not yet very well known, grows in Minnesota and the Saskatchewan region, has been mentioned by Douglas under the name of "Indian Potato" of the Assiniboine tribe, by Bourgeau as "*H. subtuberosus*," in herb. Kew, and by Dr. C. C. Parry in Owen's Minnesota Report, page 614, under the name of *H. tuberosus*. The scanty tubers which we have seen in dried specimens do not compare well with those of *H. tuberosus;* and that species has never been found wild so far north (that we know of), not even in the most southern parts of Canada West. The aborigines who cultivated it must have obtained it from the valleys of the Ohio and Mississippi and their tributaries, where it abounds.

had brought these roots into France, where they began to be sold under the name of Topinamboux, and that their Indian name was Chiquebi. On this last point, Lescarbot was wrong. Chiquebi was an eastern Algonkin name for the tubers of *Apios tuberosa*, the common "ground nuts," — not for those of *Helianthus tuberosus*. It is easy to see how Lescarbot was misled. Father Biard's "Relation de la Nouv. France" was printed in 1616, and in it (chap. 22) there is mention of certain "racines, appellées en Sauvage Chiquebi," which grow spontaneously under oaks: "elles sont comme des truffes, mais meilleures, et croissent sous terre *enfilées l'une à l'autre en forme de chapelet*," etc. Lescarbot doubtless caught the name from Biard, and misapplied it. Father Paul Le Jeune (Relation, 1634, chap. 7) mentions these ground-nuts, "une racine que nos François appellent des *chapelets*, pource qu'elle est distinguée par nœuds en forme de grains." Lescarbot's "Topinamboux" indicates a popular belief, in France, in the Brazilian origin of *H. tuberosus*. The Tupinamba Indians of Brazil — a division of the Tupi-Guarani family — had been allies of the French in the sixteenth century, and their name was probably well known in France through the relations of J. de Lery and other voyagers. Lescarbot (Histoire de la Nouvelle France, 1612, p. 178) follows Lery in writing the name "Tououpinambaoult." — J. H. T.

Helianthus annuus, L. — the history of which was almost equally confused, and which we had identified with a widespread species of the western United States — is omitted by De Candolle, yet might claim a place: for Decaisne, who has treated it at length in the paper above cited, informs us that a form of it (called "Russian Sunflower") is cultivated in Russia for the oil of its large seeds, and, if we mistake not, for fattening poultry. Our Indians also cultivated it for the oil of the seeds, which they used for greasing their hair, also for eating and other purposes. Champlain noted this (in 1610?), and Sagard about a dozen years later.[1] The latter says (Histoire du Canada, 1736, page 785): "Ils font estat du tournesol, qu'ils sement en quantité, en plusieurs endroits à cause de l'huyle qu'ils tirent de la graine," etc., piously adding: "Mais comment est-ce que ce peuple sauvage a pû trouver l'invention de tirer d'une huyle que nous ignorons, sinon à l'ayde de la divine Providence." The wild original of this Sunflower must

[1] Champlain's earlier record of the cultivation and use of the Sunflower is essentially like that of Sagard, and both relate to the same stations, namely, the Huron towns near the southeastern point of Georgian Bay. This Champlain reached by way of the Ottawa (R. des Prairies) and Lake Nipissing. The lamented Decaisne has here introduced some confusion into the history, which we hasten to rectify. In his article in the "Flore des Serres" (xxiii. p. 108, p. 2 of the pamphlet), he says, "Je trouve dans Champlain l'observation suivante (Voyage Nouv. France, réimpress. 1830, tom. i. p. 110):"

"En remontant le St. Laurent et avant l'arriver au Lac Ontario, je visitai cinq des principaux villages fermés de palisades de bois, jusqu'à Cahiagué," etc., and so on to the mention of the "grande quantité de bled l'Inde (Maïs) qui y vient très beau, comme aussi des citrouilles. Herbe des soleil, dont ils font de l'huile, de la graine de laquelle ils se frottent la tête."

This, the latitude of 44.50° being stated, would refer Cahiagué and the Sunflower cultivation to the neighborhood of Ogdensburgh and Prescott, far away from the actual place (the Indian town mentioned being the Huron name of the mission station of San Jean Baptiste, in what is now Simcoe Co.), and it introduces a palpable anachronism, Ontario having been an unknown name in Champlain's time. In fact, there is nothing answering to the early part of this pretended quotation, either in the original of Champlain or in the edition here cited by name and page. The excellent Decaisne could never have tampered with the quotation himself. He must have taken it at second hand and neglected to verify it.

have been obtained by the Canadian Indians from beyond the Mississippi, and some degrees farther south. Judging from the breadth of the flower-heads soon after its introduction into Europe, it must in aboriginal hands have assumed much of the abnormal development which distinguishes the cultivated Sunflower from its wild original of the western plains.

Solanum tuberosum, L. — The question of the Potato was fully discussed by De Candolle in 1855; and the present review of it only confirms the now generally admitted conclusions. These are summed up in the statements, that the plant is spontaneous in Chili under a form quite identical with the cultivated species, that its aboriginal cultivation had extended as far north as New Granada, but apparently no farther; that allied tuberiferous species, which our author regards as distinct (though others partly doubt it) are found along the Andes and through Mexico, and within the borders of the United States; that when known in Virginia and North Carolina in the second half of the sixteenth century, it was not derived from our Indians; and that it was carried to Europe first by the Spaniards between 1580 and 1585, and afterwards by the English.

Batatas vulgaris, Choisy, *Convolvulus Batatas*, L., the Sweet Potato, is one of a few cultivated plants which have attained to a very wide distribution over the warmer parts of the world in early times; and it is one which no botanist pretends to have seen in a truly wild state. The evidence inclines to an American origin; but it had reached the Pacific islands in prehistoric times, and was cultivated in China in the second or third century of our era. De Candolle states that —

"Clusius, one of the first to speak of the Batatas, says that he had eaten it in the south of Spain, where it was said to have come from the New World. He indicates the names of 'Batatas,' 'Amotes,' 'Ajes.'"

The testimony of Clusius (L'Ecluse) to the American origin of the Sweet Potato, though not of the highest value, might be more strongly stated. He visited Spain and Portugal in 1566. The first edition of his "Historia Rariorum Stirpium" was printed in 1576, and contains the description of Batatas,

which M. De Candolle cites from the edition of 1601. He gives a figure of the plant, of which, he says, he had observed three varieties growing in the south of Spain. He states their American origin, not as a doubtful matter or with a "l'on pretendait," but as a well-established fact: "Spontè nascitur in novo orbe, vicinisque insulis, unde primum in Hispaniam delata est." "Now," he adds, "it is planted in many places near the coast of Andalusia; but those grown at Malaga are preferred, and are transported to Cadiz and Seville. We sometimes have them fresh in Belgium, but they will not germinate here, the country being too cold." As to the name — he was as undecided as have been some botanists since his time: "the Spaniards call them 'Batatas,' and also 'Camotes' or 'Amotes'; some also 'Ajes'; yet, as they say, they differ among themselves, and the root of Batatas may be much the sweeter and the more tender."

This confusion of names dates from the time of Columbus, for Clusius was not, by half a century, the first to speak of the Batata. (It may be worth noting, in parenthesis, that Batatas, the scientific name adopted by Linnæus, and as the name of a genus by Choisy, is the Spanish plural of Batata, the aboriginal name.) Even Peter Martyr and Oviedo do not agree in all particulars as to the distinction between "Ajes" and "Batatas" — a distinction which both recognize. In the 9th book of his 2d Decade, written about 1514, Peter Martyr (ed. 1574, p. 191), describing the fruits, etc., of the province of Uraba, Darien, names, for the first time, Batatæ. "They dig from the earth," he says, "roots that grow spontaneously (*suapte natura nascentes*); the natives call them 'Batatas' [accus. plural], which when I saw I thought to be rapes of Lombardy [?'Insubres napos'] or great earth-tubers [*Cyclamen Europæum? Rapum terræ* and *Tuber terræ* of the old botanists]. In whatever way they are cooked, roasted or boiled, they yield in delicate sweetness[1] to no confectionery or other eatable whatsoever." They are, he adds, "also planted and cultivated in gardens." In his 3d Decade (lib.

[1] The sweet potato was an inspiration to Peter Martyr, who rarely indulged himself in such a flight as "dulcorata mollities."

4, p. 240) he mentions "Maize, Yucca, Ages, and Battatæ" as plants that grew in Honduras when Columbus landed on that coast in 1502; and in the same Decade (lib. 5, p. 261) he names the same four plants as the ordinary food of the people of Caramaira (east of Darien) "as of the others," and again takes occasion to name the battatas, as surpassing all else "mirâ quâdam dulci mollitie — especially if one falls on the better sort (nobiliores) of them."

Oviedo gives a good description of the Batata, which, when he wrote (1525–35), was commonly cultivated by the Indians in Hispaniola and elsewhere, and highly prized (Historia, lib. vii. c. 4). It resembles the "Ajes," he says, in appearance, but tastes better and is far more delicate. The leaf is more notched (*harpada*) than that of the "Age," in nearly the same fashion. Some varieties are better than others, and he gives the names of the five kinds which are most highly esteemed. [Peter Martyr (dec. iii. lib. 9, p. 302) included the same five names among the nine varieties of "Ajes" that he mentioned as distinct; but in this, as in other matters pertaining to natural history, Oviedo is the better authority.] "When the Batatas are well cured, they have often been carried to Spain, where the ships happened to make a quick passage, but more often they are lost on the voyage. Yet," adds Oviedo, "I have carried them from this city of Saint Domingo, in Hispaniola, to the city of Avila, in Old Castile."

The "Gentleman of Elvas" who wrote the "True Relation" of De Soto's expedition to Florida, in 1538, mentions Batatas, then growing in the Island of Terceira (belonging to Portugal).

Cieça de Leon, who was in Peru in 1547, speaking of the fertility of the valleys near the Pacific coast, and the plants cultivated by the Indians, names among these, Sweet potatoes (Chron. del Peru, c. 66). In the Quichuan language they were called "apichu"; in the dialect of Quito, "cumar." Mr. Markham, in a note to his translation (Hakluyt Soc., 1864, p. 234) mentions, on the authority of Dr. Seemann, "the curious and interesting fact that 'kumara' is also the word for Sweet potato in Tahiti, the Fiji Islands, and New Zea-

land." Garcilasso says these roots "which the Spaniards call 'batatas' and the Indians of Peru 'apichu,'" are of four or five different colors, etc. "The least good are those that have been brought from Spain."

Jean De Lery found them in Brazil in 1557, and described them under their Tupi name — "Hetich," as he wrote it — of which "the soil of Brazil is as prolific as that of Limousin or Savoy is of rapes." He describes the Indian method of planting; yet, "since these roots are the principal article of food of this country, and are met with by travelers in various places, I judge that they grow spontaneously" (Hist. Navig. in Brazil, p. 165). Montoya (Tesoro, 1639) gives the Tupi-Guarani name, "*Yetí*," and mentions numerous varieties.[1]

Monardes, in the third part of his "Simpl. Medic. ex Novo Orbe," published in 1574 (translated by Clusius, ed. 1593, p. 439), states that Battatæ "are now so common in Spain, that ten or twelve caravel loads are sent annually from Velez-Malaga to Seville."

De Candolle (who has elsewhere printed a short article upon the subject) calls attention to the fact, which ought to be familiar, that sweet potatoes are roots, not tubers, and that Turpin long ago published good figures illustrating this; also that while these roots are free from acrid or noxious qualities, all the *Convolvulaceæ* with tubers, of which there are many, and not a few of large size, are inedible and acrid, — mostly, as we know, violently purgative.

Manihot utilissima, Manioc, Cassava-plant. — De Candolle assigns good reasons for concluding (as did Robert Brown, without giving his reasons) that this important food-plant of the tropics is American, not African. But he leaves unnoticed the convincing fact that "Manioc" and "Manihot" are Brazilian names, slightly corrupted, of a plant cultivated in St. Domingo and Cuba before the landing of Columbus, and which became known to Spanish and Portuguese discoverers before 1500, by its Haytian name, "yuca," or "hiucca."

[1] Hans Stade, who was a captive in eastern Brazil in 1549, briefly mentions these "roots called '*Jettiki*,' of pleasant taste." (Captivity, Hakl. Soc. ed. p. 166.)

Peter Martyr (1493) describing the food of the islanders, names "Iucca, from which they make bread" (Dec. i. lib. 1, p. 7; ed. 1574); in the third book of his second decade (p. 148) he mentions Iucca, Ajes, and Maiz, as the three plants used by the natives for bread; in the third decade (lib. 5, p. 262) he describes the mode of the propagation by cuttings, of cultivation, and of the preparation of "Cazabbi" from the root; and he states that "there are many kinds of 'iucca'" (p. 263). Oviedo (Historia, lib. 7, c. 2) describes "the bread of the Indians that is called 'caçabi,'" which is "made from a plant they call 'yuca,'" and he distinguishes two species of the plant. Acosta (Hist. of the Indies, transl. by E. G.; Hakluyt Soc. ed., p. 232, 1588–90) gave a good account of the plant "yuca," and the kind of bread made from it, called "caçavi."

Peter Martyr (Dec. iii. lib. 9, p. 301) relates the Haytian tradition of the origin of the cultivation of "yuca" in their island. "They say that a 'Boitius' [*i. e.*, magus, or diviner], a wise old man, after the lapse of many years, saw, on the banks of a river, a plant that was like a cane; pulling it from the earth, he made this wild plant a cultivated one. He who first ate the 'Iucca' raw, quickly died. But because its taste was sweet, they determined that a way of using it should be diligently sought for. When roasted or boiled, it was less hurtful. At last they came to the knowledge of the latent poison in its juice," etc.

Gomara (Hist. gen., c. 71), Acosta (Hist. nat. y moral de las Indias, 1588–90; lib. 4, c. 17), Monardes (De Simplicibus medic., transl. by L'Ecluse, 1593, p. 437), and other writers of the 16th century gave good descriptions of the plant "Yuca," and of the "caçavi" or "cazabi" prepared from the root. By the blunder of European editors, in the last half of the 16th century, the Haytian name was transferred from the plant to which it belonged to one of another order, the Yucca of Linnæus and of modern botany. The mistake was pointed out by Lobel.

Jean de Lery (Hist. Navig. in Brasil. c. 9) describes the two species that were cultivated in Brazil in 1557 — under

their Tupi names, "Aypi" [*M. aypi*, Pohl] and "Maniot" [*M. utilissima*]. Marcgrav (Hist. plant. Bras., p. 65) mentions many varieties of both species, and gives "Mandioca" as the name of the root; "Mandiiba" or "Maniiba" for the plant. Of the products of the root, Cassava retains its Haytian name ("caçavi") nearly; Tapioca is a corruption of the Brazilian (Tupi) "tipioca" or "tipiocui."

Dioscorea sativa, alata, etc. Yam. — De Candolle informs us that these species, or their allies, are wholly unknown to botanists in a wild state; that, although cultivated in the East Indies, they have no Sanscrit names; that they seem not to have been widely cultivated in Africa, but that the authors of the 17th and 18th centuries speak of them as widely diffused over the south Pacific islands, from Tahiti to New Caledonia and the Moluccas. In the summary they are assigned to southern Asia (Malabar? Ceylon? Java?), and to the eastern Asiatic archipelago. Although a large part of the genus is indigenous to tropical America, it is thought that the cultivated species were probably introduced from the Old World. The following presentation of the evidence, as concerns America, may set the question in a different light: —

The natives of Cuba and St. Domingo, when Columbus discovered those islands, cultivated two kinds of plants for their roots. These were called in the language of the islanders of St. Domingo, "Ages" or "Ajes," and "Yuca." Neither of these plants was known to the Spaniards. About "Yuca" there is no question; it was the "Manihot," or "Manioc," of which we have already spoken. It is nearly as certain that the "Ages" was a species of Dioscorea, to which, in their ignorance of the language of the islands, the Spaniards at first gave the name of "Ñame," "Niame," "Inhame," or other corruptions of a foreign (probably African) name; and this name seems to have been occasionally misapplied both to the "Yuca" and the "Batata."

L'Ecluse, who had traveled in the south of Spain and in Portugal, in 1563, says that the Colocasia (*C. antiquorum*) "first brought from Africa, was common in many places in Portugal, near streams of water, that it was sought for by

negro slaves in Portugal, who ate it both raw and cooked," and that it was "called by the Portuguese, following the Moors, 'Inhame,' — by the Andalusians, 'Alcolcaz,'" etc. (Rarior Stirpium Hist., p. 299.) In a note to his translation of "Garcia ab Horto" (1574, p. 217), he says that "the plant called 'Inhame' by the Portuguese has very broad leaves, and grows near the water, or in water, — not spontaneously, but when once planted it propagates itself from the roots," etc.

Some of the companions of Columbus had seen the "Inhame" (or "Ñame") in Africa, and were ready to transfer its name to the first cultivated roots they saw in America. A few days after the discovery of Cuba (Nov. 4, 1492), Columbus saw fertile fields "full of 'Mames' ["these are 'Ajes' or 'batatas,'" notes Las Casas], which are like carrots ('Zanahorias'), and other plants, including Kidney beans and Beans (faxones y fabas) much unlike ours." (Navarrete, Colec., i. 200.) These "mames" are mentioned again Nov. 6 (id., 203) — in both places, probably by an error of the copyist, for "niames"; for, the next month, some natives of Hispaniola brought "bread of 'niames,' which are roots that grow as large as rapes ('rábanos'), which they plant and cultivate in all their fields, and on which they live; and they make bread of them, boil them, and roast them; and they have the taste of chestnuts, and no one eating them would believe they were not chestnuts" (id., 238). A few days later, the Spaniards learned the names of these roots — or of others with which they were at first confounded. The Admiral sent a present to a friendly cacique. The officer who carried it reported, on his return, that "all this island (St. Domingo) and Tortuga are cultivated like the country about Cordova. The lands are planted with 'Ajes' — which are little shoots (ramillos) that are planted, and at the bottom of each grow roots like 'zanahorias' which they use for bread," and these roots "are very savory, and taste like chestnuts." They have them here larger and better than he had seen in any place; for, he said, he had "[seen] such also in Guinea" (id., p. 242). Again, the natives "brought bread made of 'niames,'

which they call 'Ajes'" (id., 251); and, Dec. 26, they gave the Admiral a "collation, of two or three kinds of 'Ajes,' and of their bread that they call 'cazavi,'" etc. (id., 263). After this the name of "niames" gives place to "ajes" (or "ages"). On the second voyage of Columbus, the natives, near Isabella (in St. Domingo), brought great quantities of "ages which are like rapes (nabos) very excellent eating," and "this age, the natives of Caribi (the Caribbean Islands) call nabi, and the Indians [of Hispaniola?] hage" (id., 368, 369).

In two or three of the passages to which reference has been made — particularly those in which bread is mentioned — the Spaniards seem to have confounded the "ages" with the product of the "yuca" (Manihot), or to have included both under the general name of "niame" (or its equivalents, "Ñame, Igname," etc.) Amerigo Vespucci — or some one of the several translators through whom the relation of his first voyage comes to us — says, that in 1497, "the common food of the natives of Paria was the root of a certain tree (arborea radix quædam), which they reduce to a good enough flour, and that some call this root 'Iucha,' others 'Cambi,' but others 'Ignami'" (Navarrete, Colec., iii. 216).[1]

This confusion of names, in the first decade of discovery in America, was natural and unavoidable. The foreign name, "niame, igname," was applied without much discrimination to roots cultivated by the natives of the islands and the mainland — primarily, to "ajes," occasionally to "yuca" (Manihot), and perhaps to "batatas." In the relations of the voyages of Columbus only two cultivated roots are named — "Ages" and "Yuca." The first book of Peter Martyr's first decade (dated 1493, but probably revised before its publication in 1511) names only these two; and in the third book of his second decade he mentions the use of the same two roots by the natives

[1] It is to this passage that Humboldt refers, in "Nouv. Esp.," 2d ed., ii. 468 (cited by M. De Candolle, p. 63), as evidence that the name "Igname" was heard on the continent of America by Vespucci in 1497; but, as will be seen, Vespucci (or his copyist) does not say that this name was used by the natives.

of Comagra, in Darien (p. 148); but in a subsequent chapter (dec. ii. c. 9., p. 191) he adds — as has been mentioned in a preceding note — a third kind of roots, which the natives of the province of Darien call "Batatas," that grow in their country spontaneously. From this date to the middle of the 16th century the distinction between these roots, though occasionally lost sight of, is generally observed. Oviedo (Historia, l. vii. cc. 2, 3, 4, pp. 268–73) describes the "caçabi" and two species of the plant ("yuca") that yields it; "ajes"; and "batatas." The "ajes," he says, were cultivated in Hispaniola and in all the other islands, and on the continent; they were of various colors — white, reddish, inclining to mulberry, and tawny, but all white within, for the most part; the stem of the plant extends itself like that of "correhuela" (Convolvulus or Bindweed), but stouter; the leaves cover the ground, and are shaped much like "correhuela" and nearly like Ivy or panela, with some delicate veins ("unas venas delgadas"), and the little stems ("astilejos"), on which the leaves hang, are long and slender, etc. The leaf of the "Batata," he says (p. 274), is more toothed or notched (harpada) than that of the "Aje," but of nearly the same fashion; and the two plants are much alike, but the "Batatas" are sweeter and more delicate, etc.: some of the "Ajes" weigh four pounds each, or more. In some parts of Castilla del Oro (in Darien) there are "Ajes" that are small and yellow, etc. (p. 273). His description of the two plants permits no reasonable doubt that his "Ajes" were of the genus Dioscorea. Moreover, they were not identical with — though they resembled — the imported "ñame" or "yam": for Oviedo states (Historia, lib. vii. c. 19, p. 286), "that 'name' (called 'names') is a foreign fruit, not natural to these Indies, which has been brought to Hispaniola and other places, and is suited to this evil race of negroes, and a profitable and good subsistence for them. . . . These 'names' seem to be 'ajes,' but are not the same, and generally are larger than 'ajes.'" They had already multiplied greatly in the islands and on the mainland.

The distinction between "Ajes" and "Batatas," though

clearly apprehended, was sometimes lost sight of. Peter Martyr (dec. iii. lib. 9, p. 302) says that "the species of 'Ages' are innumerable — the varieties being distinguished by their leaves and flowers;" and he gives the American names of nine of the varieties; but five of these nine are named by Oviedo (p. 274) as varieties of "Batatas." [See Batatas, ante.]

The "Gentleman of Elvas," who wrote the narrative of DeSoto's expedition, mentions a fruit, at Santiago, Cuba, called "batata," the subsistence of a multitude of people, principally slaves, and which now (1538) grows in the island of Terceira, belonging to Portugal. . . . "It looks like the 'ynhame,' with nearly the taste of chestnuts" (Relaçam Verdadeira, ch. 5).[1]

Jean de Lery, who was in Brazil in 1557, though he gives a good description of the "Batata," does not mention the Yam; but it is figured and described by Piso (Hist. Nat. Brazil., 1648, p. 93), as "Inhame" of St. Thomas, called "Cara" by the natives of Brazil, and "Quiquoaquecongo" by the Congo negroes. Ruiz de Montoya has the name "Cará" in his Tupi dictionary, 1639, and mentions five varieties. As the Tupi name for the Virginia Potato (*Solanum tuberosum*), "Carati" (*i. e.*, white Yam), is formed from that of the "Inhame," it would seem that the latter was of earlier introduction. So, in the Mpongwe — a language of the Congo group — the Potato is called "mongotanga," "white-man's yam."

Portulaca oleracea, Purslain. — Botanists have taken it for granted that this weed of gardens and other cultivated grounds was transported to America from the Old World. But Nuttall found it apparently indigenous on the upper Missouri forty years ago, and Dr. James in Long's Expedition, along the eastern base of the Rocky Mountains in what is now the State of Colorado. From thence to Texas it grows wild

[1] In one Indian language of the south, the Choctaw, the Sweet Potato is now called "ahe"; while the Virginia Potato (*S. tuberosum*) takes the adopted prefix of "Irish," "Ilish ahe," or is sometimes called "ahe lumbo," "round ahe."

along with two other nearly related species. Moreover, the following evidence tends to show that its introduction, if introduced by human agency, took place before the landing of Columbus.

On their first sight of the New World, the Spaniards were much impressed by the strangeness of all forms of animal and vegetable life: "all the trees are as unlike ours, as day is to night" — wrote Columbus, Oct. 17th, 1492, six days after landing at San Salvador: "and so are the fruits, and so the plants, and the stones, and all things" (Navarrete, i. 183). On the 28th, on the north shore of Cuba, he saw — apparently for the first time — a familiar plant: "halló verdolagas muchas y bledos," — he found much "purslane" and "bletum" (id., 192). It seems hardly possible that the Admiral and his companions could mistake a strange plant for a salad herb so well known as "verdolagas" to Spanish eyes and palates. Again, Oviedo, writing about 1526, in a list of "plants in the island of Hispaniola which are like those of Spain, and which were before the Christians came to these parts, and are natives of this land, and were not brought from Spain," mentions "verdolagas or pertulaca," and "bledos or bletum" (Blitum).

In his description of "perebenequc," written in 1525, he says that plant grew in great abundance in Saint Domingo and in many places on the continent, in the woods and fields; even "purslane ('verdolagas') is not more abundant here" (id., lib. xi. c. 5, p. 378).

Jean de Lery, in Brazil in 1557, was as much impressed by the novelty of the flora as Columbus had been in the West Indies. "I declare," he wrote (Hist. Navig. Brazil., 168), "as far as it was permitted me to discover in wanderings through the woods and fields, that there are no trees or plants, or any fruits, that are not unlike ours, these three excepted, portulaca, ocymum, and filex" (in the original French edition, 1578, p. 217, "pourpier, basilic, et fougière").

Capt. John Smith, in Virginia in 1606, found "many herbes in the spring, commonly dispersed throughout the woods, good for broths and sallets, as Violets, Purslain, Sorrell, etc.; be-

sides many we used whose names we know not" (Smith's Gen. History, 1632, p. 26; and repeated by Strachey, Travaile into Virginia, p. 120). Smith's purslain was probably *Sedum ternatum*.

Sagard-Theodat, in the relation of his "Grand Voyage du Pays des Hurons," in 1624 (p. 331), says that the Hurons make little use of herbs, "although the pourpier or pourcelaine is very common there, and grows spontaneously in their fields of corn and pumpkins."

W. Wood, who was in New England from 1629 to 1633, names "Purselane" among plants growing "in the woods, without either the art or the help of man" (N. E. Prospect, pt. 1, c. 5). We doubt its growing literally in the woods, as unlike its natural habit, and place more confidence in the statement of Champlain, who, in his earlier voyages, 1604–11, found plenty of excellent "pourpier," for his salads, on the coast of New England, growing among the Indian Corn; "the savages making no more account of it than if it were a noxious weed" (Voyages, ed. 1632, p. 80).

Humulus Lupulus, Hops. — Although the matter has nothing to do with the introduction of Hops into cultivation, it is noticeable that De Candolle assigns the home of the plant only to Europe and western Asia. It is undoubtedly indigenous to North America also, and is mentioned as such in the American works. In Gray's "Manual," besides the printing of the name in the type appropriate to indigenous species, the plant is expressly stated to be "clearly indigenous." But, through some oversight, in the "Prodromus" (xvi. 29), it is stated, in connection with this very reference, that the plant was introduced.

Oca. — Considering that "Maté" and "Coca" find place in this volume, although perhaps rather employed than cultivated (at least the former), the absence of Oca (*Oxalis tuberosa* and *O. crenata*) is noticeable. This esculent root deserves mention, if only for the antiquity of its culture in Peru. The name, which is Quichuan, appears to have belonged, specially, to *Oxalis tuberosa*. Another root "like the oca in shape, but not in taste," called in Quichua "añus," was less esteemed.

Both were cultivated in Peru in the time of the Incas, and in the districts where no Maize grew, the crop of these tubers was of much importance (Garcillaso, Comment., b. v. c. 1; b. viii. c. 10). J. de Acosta, 1588–90, says "there are an infinite number" of roots used for food in the Indies, "but the Papas (Potatoes) and Ocas be the chief for nourishment and substance" (Nat. and Moral Hist. of the Indies, lib. iv. c. 18).

Our notes upon plants cultivated for their herbage, tubers, roots, etc., have run to such a length that the remainder, concerning some plants cultivated for their fruits and seeds, must be left for another article.

PART II.

THE fourth chapter relates to plants cultivated for their fruits; the fifth, to those cultivated for their seeds. Our present annotations concern a few species or forms of *Cucurbitaceæ*, the history of which has been involved in some obscurity and confusion.

A word, in passing, upon the Peach, upon the history of which this volume throws some new light. De Candolle had formerly suggested China as its home, and he has brought together additional evidence in favor of that view. He shows how this conclusion goes against an old idea that the Peach is a derivative of the Almond, which is indigenous to western Asia, and was unknown to the Chinese anterior to the Christian era, while they had peaches of various sorts long before. Upon Pyrus there is a note relating to botanical orthography, p. 183, which we append, as it has an application to a few other words.[1]

[1] "L'Orthographe Pyrus, adopté par Linné, se trouve dans Pline, 'Historia,' ed. 1631, p. 301. Quelques botanistes ont voulu raffiner en écrivant Pirus, et il en result, pour une recherche dans un livre moderne, il faut consulter l'index dans deux endroits, ou risquer de croire que les Poiriers ne sont pas dans l'ouvrage. En tous cas le nom des anciens est un nom vulgaire, mais le nom vraiment botanique est celui de Linné, fondateur de la nomenclature adoptée, et Linné a ecrit Pyrus."

Pears and apples were prehistoric in Europe, both wild and cultivated.

Lagenaria vulgaris. Bottle Gourd. — Although doubtless an Old-World plant (De Candolle attributes it to India, Molucca, Abyssinia), yet it is not quite certain that it had not reached the New World before Columbus. At least the following notes may be put upon record.

M. De Candolle mentions the case of the name gourd for pumpkin ("potiron") by English writers, as "an example of the confusion of popular names and the greater precision of scientific names." Such confusion becomes more perplexing when we have to deal with popular names of the 15th and 16th centuries. Parkinson — a good observer and a respectable botanist — complained, in 1640, of "our modern writers who confound Pepo, Melopepo, and Cucurbita so promiscuously that it is not possible to find out the distinct certainty of them all; for some make that Pepo that others call Melopepo, and others, Cucurbita." (Theater of Plants, p. 770.) Scientific names of the 16th century are as obsolete as popular names of the same period. They do not help us to distinguish Lagenaria from Cucurbita, or Pepo from Melopepo; or Citrouille from Citrullus. Early voyagers to America wrote cucurbita, calabaça, courge, or zucca, as a name for any gourd or pumpkin, and occasionally for a calabash which was not even a cucurbit. The relation of the first voyage of Columbus repeatedly mentions the "calabazas" used by the natives of St. Domingo and other islands for carrying water (Navarrete, Collec., i. 180, 188, and Dec. 3, 1492). Columbus saw, near the east end of Cuba, fields planted with "calabazas" and other productions of the country (id., p. 225). Yet we know from Peter Martyr that some of the gourds ("cucurbitæ") used in the islands grew on "cucurbiteas arbores" as tall as elms (Dec. i. lib. 3, and iii. lib. 4; pp. 38, 246). This tree, *Crescentia Cujete*, is described by Oviedo (Historia, lib. viii. c. 4) under its Haytian name, "Higuëro"; in Nicaragua it was called "Guacal"; and in Brazil, "Cuiete"[1] (Marcgrav, Hist. Nat. Brasil., 123). J. de

[1] Not "Cujete" — unless *j* has the German sound. The Tupi name is formed from *cuï* (*cou-in*, Lery) "the shell" or hard rind of a nut or fruit (and the bowl or calabash made from it) and *eté* "good, precious."

Lery (Hist. Navig. in Brasil., 154) describes the tree under the Brazilian name of "Choyne"; but elsewhere (p. 246) he says "the natives have *Cucurbitæ* (courges) and other kinds of fruits," from which "they make their bowls, called 'coui,' and other vessels."

It is certain that "calabaças," which were not arboreal, but genuine cucurbits, were abundant — and were believed to grow spontaneously — in the islands and on the main land, before 1526. Oviedo (Historia, lib. vii. c. 8) observes that "calabaças, in the Indias, were as common as in Spain, and of the same kinds (de las mismas), long and round, or banded (çeñidas), and of all the shapes they usually have [in Spain]." They were much used "in all parts of these Indias, both the Islands and the Main," and "are one of the common things that the Indians cultivate in their gardens." They were not cultivated for food — "for they do not eat them" — but for carrying water; "and they have other calabaças that are in all respects like the aforesaid, except that they are bitter to the taste; and there are many of these that grow of themselves without cultivation."[1] The same author (lib. xi., c. 1), in a list of plants introduced from Spain, names Melons and Cucumbers (pepinos), but not Gourds.

The relation of the voyage of Amerigo Vespucci, 1489, in a description of the Indians of Trinidad and the coast of Paria, says that "each carried, hanging at his neck, two small dried gourds (*cucurbitas*), one containing the plant that they were accustomed to chew, the other, a certain whitish flour," etc., and that each woman carried a "cucurbita" of water (Navarrete, iii. 252, 254).

The "Cucurbita lagenæ formâ," which Marcgrav found in Brazil, 1637–8 (Hist. Nat. Brasiliæ, 44), though "very probably *Lagenaria vulgaris*," yet, as M. De Candolle observes, "does not prove that the species was in that country before the voyage of Amerigo Vespucci in 1504;" but we

[1] M. De Candolle, p. 198, citing this passage from Ramusio's Italian translation of Oviedo's "Historia," has "zucche" for "calabacas" of the Spanish original, and takes no notice of what is said of their spontaneous growth.

know from Lery, above cited, that the natives of Brazil used *Cucurbitæ*, for bowls and drinking-vessels, at least as early as 1557. Moreover, the richness of the Tupi vocabulary in gourd-names suggests — if it does not absolutely prove — that several varieties of Lagenaria were known to the Brazilians long before the visit of Piso and Maregrav. The "Tesoro de la Lengua Guarani" (*o Tupi*) of Father Ruiz de Montoya was first printed in 1639. It gives for gourd ("calabaço"), the Tupi general name, "Ia" [which is a compound of *ĭ* "water," and *yá* or *á* "fruit"], and for the varieties — among others — it names *ĭaçĭ* "round gourd"; *ĭáyurumî* "narrow-mouthed gourd"; *ĭátĭ* "long-necked gourd"; *ĭáobá* "wide-mouthed gourd"; *ĭáquatiâ* "painted gourd"; *ĭácuîpé* "spoon gourd" (used for making spoons); *ĭápê* "small gourd, used for drinking"; *ĭáquâ* "great gourd"; *ĭácuî* "gourd like a great dish" or bowl, etc.: not including the derivatives of *cuî*, or the edible "calabaças" — to be mentioned hereafter.

"Acosta, too," says M. De Candolle, "speaks of Calebasses which the Peruvians used for cups or vases, but the Spanish edition of his book is of 1591, more than a hundred years after the conquest." (?) Acosta says more than this. After mention of the "Calebasses or Indian Pompions . . . especially those which are proper to the country" [Peru], he adds: "There are a thousand kinds of Calebasses; some are so deformed in their bigness that of the rind cut in the midst and cleansed, they make, as it were, baskets to put in all their meat for their dinner. Of the lesser, they make vessels to eat and drink in," etc. (Hist. nat. y moral de las Indias; translation, revised by Markham, lib. iv., c. 19, p. 238.)

Cucurbita maxima, *C. Pepo*, *C. moschata*. Pumpkin, Squash, etc. — In the "Géographie Botanique" not one of the cultivated *Cucurbitæ* is attributed to America, and a reference to Nuttall's record that the warted squash was grown by the Indians on the upper Missouri is the only mention of any aboriginal cultivation of squashes in North America. In the present volume there is merely a reference, in this respect, to Dr. Harris's article in this Journal (xxiv., 1857), and to Mr. Trumbull's note in the "Bulletin of the Torrey

Club" (1876), with the comment that: "Cela nous apprend seulement que les indigènes, un siècle après la découverte de la Virginie, 20 à 40 ans après la colonisation par W. Raleigh, faisaient usage de certains fruit de Cucurbitacées." Nevertheless *Cucurbita Pepo*, upon botanical indications solely, is attributed to temperate North America in the general table, to a Mexican or Texan origin in the body of the work. This rests upon the collection by Lindheimer, in Texas, of a form of this species "apparently indigenous." That was between thirty and forty years ago; no wild specimen has since been received from all that region (nor from any other); and it is wellnigh certain that the species was commonly cultivated in all that country by the aborigines. If ever found truly indigenous, it will probably be farther south than Texas. *C. maxima* is now set down as from Guinea, on the strength of a single finding of it "apparently indigenous" on the banks of the Niger. *C. moschata* (to which Vilmorin refers the Canada Crook-neck Squash) is in the list of species of completely unknown or uncertain origin.

In this state of the case, it is certainly worth while to present the evidence — gathered with much care and pains — which assures us that one or two, and perhaps all three, of these species, and many varieties, were largely cultivated throughout America, from the tropics to Canada, before the voyages of Columbus.

Allusion has already been made (under Lagenaria) to the difficulty of distinguishing the genera of *Cucurbitaceæ* under the names by which they are mentioned by voyagers and explorers of the first century after the discovery of America; and the question of species is particularly difficult. Yet we find abundant evidence — especially as respects North America — (1) that, in various parts of the country remote from each other, the cultivation of one or more species of Cucurbits by the Indians was established before those places are known to have been visited by Europeans; (2) that these species or varieties were novel to Europeans, and were regarded by botanists of the sixteenth and seventeenth centuries, as well as by the voyagers and first colonists, as natives or denizens of the

region in which they were found; and (3) that they became known only under American names; one of these names (Squash) becoming, in popular use, generic, and two others (Macock and Cushaw) surviving, as names of varieties, into the present century.

To present this evidence as nearly as possible in the order of time, we refer, first, to the relation of the first voyage of Columbus. December 3, 1492, entering a small river [the Rio Boma], near the eastern end of the island of Cuba, he found near it a populous Indian village, and saw large cultivated fields "planted with many things of the country, and calabazas, a glorious sight (que era gloria vella)!" See Navarrete, Colec., i. 225. It is not certain that these "calabazas" were not bottle-gourds (Lagenaria), but it is, to say the least, highly improbable that the enthusiasm of Columbus would have been so kindled by the promise of a harvest of little value to Europeans.

Oviedo (Historia, l. xi. c. 1) names among plants and seeds brought from Spain to Hispaniola "melones" and "pepinos" — of which imported varieties were already abundant in the island before 1535; the seed of "cogombros" brought from Castile had not succeeded so well.

In July, 1528, Cabeça de Vaca found near Tampa Bay, in Florida, "maize, beans, and pumpkins in great plenty, and beginning to be fit for gathering." In 1535–6, when passing through Texas, the Indians supplied him with prickly pears and, occasionally, maize; but after crossing "a great river coming from the north" — probably the Rio Grande — he and his companions came to a region having "fine dwellings of civilization, whose inhabitants lived on beans and pumpkins" — and, when the season was not too dry for raising it, maize (Relacion, 1542; translated by B. Smith, 1871).

In the summer and autumn of 1539, De Soto found the Appalachian country, in western Florida, well supplied with "maize, beans (fésoles) and pumpkins (calabaças);" the pumpkins of Uzachil were "better and more savory than those of Spain;" there were "fields of maize, beans, and pumpkins," not far from Tampa Bay, where he first landed

from Cuba; at Pacaha, on the Mississippi, the northernmost point he reached (1541), he found again "many pumpkins and much maize and beans"; and, still westward, at Coligoa, "beans and pumpkins were in great plenty; both were larger and better than those of Spain; the pumpkins when roasted had nearly the taste of chestnuts" (Oviedo, lib. xvii. cc. 24, 28; True Relation, etc., by a Fidalgo of Elvas; translated by Buckingham Smith, pp. 45, 47, 122, 285). Oviedo writes "calabaças," but the author of the Portuguese "Relaçam Verdadeira" (1557) has, in one or more of the places cited, "aboboras."

In 1535, Jacques Cartier, the first explorer of the St. Lawrence, found among the Indians of Canada "grand quantité de gros melons, concombres and courges" (Bref Recit de la Navigation, etc., 1545; reimpr. Tross, 1863, ff. 24, 31).

Sagard, whose "Grand Voyage du Pays des Hurons" was made in 1642, makes repeated mention of the native squashes ("citrouilles du pays") which the Hurons raised in abundance, and which he found very good, boiled or baked (pp. 85, 105, 140, 331). In his "Histoire du Canada" (283) he describes the method by which the Indians hastened the germination of the seeds of these "citrouilles du pays," and "raise them with great ease."

Lahontan (Nouv. Voyages, 1703, ii. 61) describes the "Citrouilles" of (southern) Canada — "sweet, and of a different kind from those of Europe, where," as several persons assured him, these would not grow. "They are of the size of our melons; the flesh yellow as saffron. They usually bake them in the oven, but they are better roasted under the embers, Indian fashion," etc. Lahontan had as little doubt as Sagard had, that these "citrouilles" (cultivated by the Indians of Canada from the time of Cartier, at least) were genuinely "du pays."

As to the *Cucurbitaceæ* of Virginia, M. De Candolle admits, "only, that the natives, a century after the discovery of Virginia, twenty to forty years after the colonization by W. Raleigh, made use of certain fruits of *Cucurbitaceæ*" (p. 201). Let us reëxamine the evidence. Captains Amidas

and Barlow, in the first vessels sent by Sir Walter Raleigh to the New World, landed on an island in Ocracoke Inlet (now within North Carolina) in 1584. While the vessels remained there, and while they were at Roanoke Island near by, the Indians entertained them kindly, and "sent them, commonly every day, a brace of bucks, conies, etc., sometimes melons, walnuts, cucumbers, pease, and divers roots" (J. Smith's Gen. Hist. p. 3).

What these "melons," or some of them, were, we learn from later explorers and the first colonists of Virginia (proper).

Captain John Smith says that the Indians of Virginia (1606–8) "plant amongst their corn Pumpions, and a fruit like unto a musk-melon, but less and worse, which they call Macocks," etc. (Gen. Hist. p. 29). Strachey, who was in Virginia in 1610, describes these "macock gourds" in nearly the same words (Trav. into Virginia, p. 72); elsewhere, he says the "macokos is of the form of our pumpions — I must confess, nothing so good, — 't is of a more waterish taste," and he mentions also the "pumpions" planted by the Indians, and "a kind of million" which they "seeth and put into their walnut-milk, and so make a kind of toothsome meat" (p. 119). "The Indian Pumpion, the Water-melon, Musk-melon," etc., are named among fruits introduced into Bermuda, by the English, before 1623 (Smith's Gen. Hist. p. 171).[1]

[1] L'Ecluse (Clusius) heard of these Macocks in 1591 or earlier. In his "Exotica" (1605; lib. iii. c. 2) he describes a fruit — "Macocqwer Virginiansium, forte" — which had been sent him from London by James Garet, brought from "the province of Wingandecaow, which the English call Virginia." He conjectured that this might be "the fruit which the natives of that region call Macocqwer" — but his figure and description do not favor this identification. The fruit, he says, is nearly orbicular; four inches in diameter; with a hard rind, yellowish on the outside; many seeds, flat and heart-shaped ("cordis, ut vulgo pingitur, formam referentia"). L'Ecluse thought it might be one of the gourds which the natives used for rattles, as the Brazilians used their Tamaraca, etc. His specimen was old and dried, the pulp blackened, the rind covered with a dark membrane, "per quam sparsæ quædam fibræ à pediculo ad summum." This must have been a fruit of *Crescentia cucurbitina*, a calabash, which is a native not only of the West Indies, but also of southern Florida.

Among Johnson's additions to Gerarde's "Herball," 1636, there is a description of "Macock Virginiani, sive Pepo Virginianus; the Virginian Macock or Pompion" (pp. 919, 921). The description is dated 1621, and signed by John Goodyer. The plant has "great broad shrivelled yellow flowers, like those of the common Pompion." The fruit, "somewhat round, not extending in length, but flat like a bowl, but not so big as an ordinary bowl, being seldom four inches broad and three inches long; of a blackish green color when it is ripe. The substance or eatable part, of a yellowish white color. . . . Seeds like the common Pompion, but smaller." The "small round Indian Pompion," and "the cornered Indian Pompion" — the latter resembling our common "scolloped Squash" ("*Pepones lati*, Broad Melons or Pepons" of Lyte's Dodoens, p. 588) — are described and figured in "Johnson's Gerarde," p. 920.

Beverley's "History of Virginia," 1705, p. 124, mentions the Macocks, "a sort of Melopepones, or lesser sort of Pompion or Cashaw," which he identifies with the "Squash or Squonter Squash" of New England. "The Indian name," he says, "is still retained by them." Professor Schele de Vere (of Virginia) states that it still "survives in its anglicized form of Maycock" (Americanisms, 1871, p. 60).

The "Cushaw" ("Ecushaw," Hariot) is described by Beverley (Hist. of Virg., p. 124) as "a kind of Pumpion, of a bluish green color, streaked with white when they are fit for use. They are larger than the Pompions, and have a long narrow neck. . . . The Cushaws and Pompions they lay by, which will keep several months good, after they are gathered" (p. 152). Bartlett, "Dict. of Americanisms," notes the name Cushaw, "sometimes spelled Kershaw," as "Western" for a pumpkin. Beverley's description makes it nearly certain that the variety so named was the (New England) winter "crookneck" squash — which, five and twenty years ago, might have been seen hanging, by its necklace of flannel "list," in every New England farmer's kitchen, from early harvest time till wanted for Thanksgiving or Christmas pumpkin-pies.

The Rev. Francis Higginson, who came to New England in

1629, wrote from Salem, a few weeks after his arrival: "Here are stores of pompions, cowcumbers, and other things of that nature which I know not" (N. E. Plantation, 1630); and, again: "We abound with . . . sundry sorts of fruits, as musk-melons, water-melons, Indian pompions, Indian pease, beans, and many other odd fruits that I cannot name" (Young's Chron. of Mass., 265). William Wood, who was in New England from 1629 to 1633, says of the Indians of Massachusetts: "In summer, when their corne is spent, 'Isquoutersquashes' is their best bread, a fruit like a young Pumpion" (N. E. Prospect, p. 76). Roger Williams, 1643, names these "*Askútasquash*, their vine-apples, which the English from them call squashes, about the bignesse of apples, of several colors, a sweet, light, wholesome refreshing" (Key to the Language of America, 103). Again, Josselyn (1638–71, N. E. Rarities, 57) mentions these "squashes . . . more truly Squontersquashes, a kind of melon or rather gourd, for they oftentimes degenerate into gourds; some of these are green, some yellow, some longish like a gourd, others round like an apple, all of them pleasant food boiled and buttered, etc. But the best yellow squash, called an Apple squash, because like an apple, and about the bigness of a Pome-water, is the best kind: they are much eaten by the Indians and the English." But he distinguishes these from the "Pompions [of which] there be several kinds, some proper to the country; they are dryer than our English Pompions, and better tasted; you may eat them green" (p. 91). The last words (here italicized) give a nearly literal translation of the Algonkin-Indian name of Cucurbits, — in the dialect of New England, "asq," plural "asquash," "green things," or (to be eaten) "immature." Eliot, in his version of the Bible (1663) names three kinds of asquash: askoot-asquash [= Askùt-asquash, R. Williams, Isquoutersquash-es of Wood, Squontersquash-es of Josselyn, ut supra], for "cucumbers"; quonoo-asquash "gourds" [literally, "long *asquash*"]; and monas-koot-asquash "melons."

Squashes were first known to the Dutch by their Algonkin name. Van der Donck, after speaking of the pump-

kins of New Netherland (1642–53), adds: "The natives have another species of this vegetable peculiar to themselves, called by our people 'quaasiens,' a name derived from the aborigines, as the plant was not known to us before our intercourse with them. It is a delightful fruit, as well to the eye on account of its fine variety of colors, as to the mouth for its agreeable taste. . . . It is gathered early in summer, and when it is planted in the middle of April, the fruit is fit for eating by the first of June. They do not wait for it to ripen before making use of the fruit, but only until it has attained a certain size. They gather the squashes and immediately place them on the fire without any further trouble. . . . The natives make great account of this vegetable." (Descript. of N. Netherlands, 1656; transl. in N. Y. Hist. Soc. Coll., 2 ser., i. 186.)

Thus far we have cited, with one or two exceptions, American authorities. M. De Candolle, after mentioning "the three forms of Pepones figured by Dodoens, edition of 1557, to which a fourth, *P. rotundus major*, was added in the edition of 1616," and a figure of *P. oblongus*, in Lobel. "Icones," 641, observes, that "the names given to these plants indicate a foreign origin; but the authors can affirm nothing in this regard; the less so, because the name Indian signifies, either, of southern Asia or of America" (p. 204). A collation of the descriptions of Pepones or Cucurbitæ, given by European botanists of the sixteenth century, does away with this ambiguity.

Tragus (Hieron. Bock), "De Stirpium Nomenclaturis," etc., 1552, p. 830, described and figured Melo, Pepo, Cucumis, and Citreolus; and (p. 832) named, also, *Cucumis sylvestris*. In the next chapter (p. 834) he wrote "De Cucumere seu, ut vulgo loquuntur, Zucco marino" — with a figure. "Many kinds of strange plants," he says, "have been brought from remote parts into Germany, in the last few years." Among others, these "poma æstiva," of which some are large, some small, some round, some oblong, some sweet, others bitter, of various colors. "Some call these 'Cucumeri,' and assert that they are 'Turkish Cucumeres,' with which opinion I cannot agree. . . . I call them *Mala æstiva & Indica*," of

which he distinguishes four kinds, *M. Indica crocea*, *lutea*, *citrina*, and *nigra*. "Commonly," he says, "they are called Zucco marina, because they first came to us from parts beyond the sea, some from Syria, some from India, which the names given them attest; for they are commonly called Zucco de Syria and Zucco de Peru."

The figure of *Cucumer marinus*, Ital. *Cocomere marino*, etc., in the "Effigies Plantarum" of Fuchs, 1549, is a reduced copy of Bock's, and substantially agrees with that of *Pepo rotundus* in Lyte's "Dodoens," p. 587, which was "called also *Cucumis marinus;* of some, *Zucco marino;* in French, *Concombre marin*, *Pompons Turquins*," etc.

Matthioli of Padua (Comm. in Dioscor., ed. 1559, p. 292) is more explicit. "There are," he says, "various kinds of cucurbits foreign to Italy, which can be kept fresh far into the winter. They say that these came into Italy from the West Indies, whence they are called by many Indian. Their taste is sweetish, not so insipid as ours," etc.; and his figure of *Cucurbita Indica* agrees with that of Bock's Zucco marinus (or "Zucco de Peru") and with Lyte's *Pepo rotundus*.

It is certain, then, that the botanists of the 16th century to whom M. De Candolle refers, used Indian — when applied to varieties of Cucurbita — in the sense of American. In the 17th century, the evidence is not less direct. Parkinson (Theatrum Botanicum, 1640, pp. 769, 770) figures and describes (1) *Cucurbita lagenaria major*, the greater Bottle Gourd; (2) *C. longa*, the long Gourd; (3, 4) *C. clypeiformis & verrucosa*, and *Anguria Ægyptiaca*, the Simmel [Scallop Squash], and the rugged Gourd [warted Squash, orbiculate depressed], and the Egyptian Citruell or Watery Million; (5) *Cucurbita Indica*, *ovalis*, *pyriformis*, & *fere rotundus*, Indian Gourds, oval, pear-fashioned, and almost round. Of these Indian Gourds he says: "There is very great variety of these Gourds (or Millions, as some call them, or Pompions, as I may call them) that came out of America or the West Indies, from sundry places, both farther south among the Spanish colonies, and nearer hand, in our own of Virginia, New England, etc." He notes the great variety

of size, shape, and color, "some as great as our pompions, some as small as an apple, some discolored on the outside, green with whitish or yellowish stripes, . . . some also reddish, spotted, or striped, and some of a deep yellow."

Piso and Marcgrav (Hist. Nat. Brasil., 1648, p. 44) describe and figure a plant called "Iurumu" [=Yurumu] by the Brazilians, and by the Portuguese, "Bobora." M. De Candolle, p. 201, is inclined to agree with modern botanists in referring this to *C. maxima;* but, as he remarks, it appears to have been a cultivated plant. If introduced from abroad, the name given it by the Tupis was probably formed, by prefix or affix, from that of some native (or naturalized) species to which it had some resemblance. In Montoya's "Tesoro," 1639, we find "Yuruá" "calabaçillos silvestres," small wild calabazas; but the name "Yurumu" did not yet appear. Almost a century before the visit of Piso and Marcgrav, Jean de Lery saw in Brazil (1557) "certains citrouilles rondes, fort douce à manger," called by the natives "Maurongaus" (Voyage, ed. 1578, p. 217). The Tupi name "morangá" (the first two vowels nasal) denotes a "handsome fruit."

Lycopersicum esculentum, Tomato. — We have only to note an oversight in respect to the Mexican cultivation of the *Mala Peruviana*, as it was named by some botanists of the 16th century. De Candolle refers to Humboldt's statement that the cultivation of this esculent was ancient in Mexico, but adds that there is no mention of it in the earliest work on the plants of that country, namely: Hernandez, "Historia." But Hernandez (ed. 1651, p. 295 ff.) actually has a chapter "De Tomatl, seu planta acinosa vel Solano," and describes several sorts under their Mexican names.[1]

[1] I find only one writer in the 16th century who gave the Tomato a name indicating a Peruvian origin — namely, Anguillara, whose treatise "De Simplicibus" was first printed in Italian, at Venice, 1561. On his authority the name *Poma Peruviana* is introduced in the synonymy by C. Bauhin, in his annotations on Matthioli, 1598 (p. 761), and the "Hortus Eystettensis" (attributed to Besler), published in 1613, is referred to by the same writer (Pinax, 1771, p. 167) an authority for the names, *inter alia*, "Poma amoris fructu rubro & Mala Peruviana." The "Hortus Eystettensis" is the only authority cited for *Mala Peruviana* in the work to

Persea gratissima, Alligator Pear of the English, l' Avocat of the French; a singular corruption of a native name, as De Candolle remarks, which had no more to do with an alligator than with a lawyer. Our author does not carry back the native Mexican name quite to its original, which was "Ahuacahuitl" corrupted by the Spaniards into "Aquacate," "Avogade," etc. Champlain, who saw it in Mexico in 1599 or 1600, calls the fruit "Accoiates" and "Acoyates"; "Voy. to the W. Indies" [Hakluyt Soc., 1859], p. 28.

Oviedo described "the wild pear-tree of the main land," in 1526. It grew "in the province of Castilo del Ora (Panama), in the sierras of Capira and the country of the cacique of Juanaga," etc. In the revision of his first work, in 1535, he adds, that he had, some years before, seen these trees cultivated by the Indians in Nicaragua (Historia, lib. ix. c. 22). It was still a tree of "Terra firma" — not yet introduced into the Islands. Clusius saw it in a garden in Valencia — "said to be brought from America" — thirty-five years earlier than the date (1601) mentioned by De Candolle. He described the Persea in the first edition of his "Historia rariorum Stirpium," 1576 (lib. i. c. 2), published five years after his journey in Spain.

Passiflora. — This genus is wholly omitted by De Candolle; unaccountably so, considering how much Granadillas have been cultivated and prized in tropical countries. A note on the subject may not be out of place, as a species was cultivated by our own Indians.

which M. De Candolle refers — the "Historia Stirpium," attributed to J. Bauhin, but published long after his death (in 1551), with large additions by his son-in-law Cherler, and by Chabræus and Graffenreid. Guillandinus, of Padua, in a treatise "De Papyro," 1572, named the "Tumatle Americanorum" as a species of "*Pomum Amoris* or *Solanum pomiferum*"; and earlier, Matthioli had described it (Comment. in Dioscor., ed. 1559, p. 537) as a "kind of Mala insana" [*Solanum Melongena*], which was "beginning to be imported" into Italy, and which was "popularly called *Pomi d'oro*, that is, *Mala aurea*."

Anguillara may have confounded the Tomato with another of the American *Solanaceæ*, introduced at about the same period — the Thorn Apple (*Datura Stramonium*) which Guillandinus (1572) named *Mala Peruviana*, and the French called "Pomme de Perou." — J. H. T.

As to early history and aboriginal nomenclature, Monardes (De Simplicibus Medic., c. 66) says that the Granadilla was spontaneous in Peru, and that the fruit was highly esteemed by the Indians and by the Spaniards. Cieça de Leon (Chron. del Peru, c. 28), about 1550, saw it growing in the valley of the Rio Lile, near Cali (now in Colombia), and in the country about Pasto. The flower and fruit are mentioned by J. de Acosta (Nat. and Moral Hist. of the Indies, b. ii. c. 38) — "the fruit sweet, and too sweet, in the opinion of some." Lery (1557–8) does not appear to have found it in Brazil, but it was common there before the middle of the 17th century. Piso and Marcgrav (Hist. Nat. Brasil., 1648, pp. 70, 106) reckon nine species or more, of which four were cultivated — two especially for their fruits. The Tupi (Brazilian) name was "Mburucuîa" ("Montoya," 1639), which Piso and Marcgrav, *l. c.*, wrote "Murbcuia"; the species which was generally cultivated for its fruit was "Mburucuîa-guacu" (*i. e.*, great "Murucuya"). Father R. Breton (Dict. Caraibe, 1665) gives "Merécoya" as the Carib name of the fruit; but this seems to have been adopted from the Tupi — for in that language "Mburucuîa" denotes the "fruit of a vine."

It is remarkable that the Tupi (and Carib) name went with the plant and its fruit to the country of the Algonkins before the coming of Europeans. One species (*Passiflora incarnata*) was cultivated by the Indians of Virginia. "They plant also Maracocks, a wild fruit like a lemon, which also increase infinitely" (Capt. John Smith, Gen. Hist., p. 29): and, again (p. 25), Smith mentions the "fruit which the inhabitants call 'Maracocks' — pleasant, wholesome fruit" — among "things which are naturally in Virginia." Strachey (Travaile into Virginia, 72, 119) describes the "fruit called by the natives a 'maracock,' which the Indians plant," etc.

Although no longer planted, the fruit of the spontaneous plant is still eaten in the southern Atlantic States; and its popular name, "May-pop," is probably the last stage of the Tupi original.

Now our *Passiflora incarnata* is so like *P. edulis* (well

known in cultivation), the home of which is in Brazil,[1] that botanists have been unable clearly to distinguish the two, except by the fact that ours, dying down to the ground at approach of winter, remains herbaceous. It occurs in a rather narrow geographical range; and Dr. Masters, in his elaborate study of the order (Trans. Linn. Soc., xxvii. 641; see also Flora Brasiliensis), says that "being so far separate from the remainder of its allies of the same subgenus, [it] may be considered as an outlier." Altogether we may infer that the fruit and the name were originally derived from the same South American source.

Musa, Banana. — The author concludes, as did Robert Brown, that Banana and Plantain are varieties of one species; also that this species is of the Old World; that in all probability it was not known in the West Indies when discovered by Columbus; but that in respect to the western side of South America there is some evidence which is not easily ruled out, especially the statement of Garcilasso that the Peruvians had the Banana before the conquest, and of Stevenson (Trav. in S. Amer.), who is said to have seen in the ancient Peruvian tombs beds made of Banana leaves. This is discredited because the author found beans in the same tombs, "et que la fêve est certainement de l'ancien monde." But if Stevenson wrote beans, without doubt he meant the seeds of Phaseolus, not of Faba. It would rather seem that the Banana, like the Sweet Potato and Cocoa-nut, had early been transported over the Pacific.

Phaseolus vulgaris, Kidney Bean. — Three weeks after his first landing in the New World Columbus saw, near Nuevitas in Cuba, fields planted with "faxones and fabas very different from those of Spain," and two days afterwards, following the north coast of Cuba, he again found "land well cultivated with these fexoes and habas much unlike ours." "Faxones"

[1] Not Mexico, although indeed said to have been brought from New Spain to the garden of the Farnese palace, in Rome, as early as 1619. It is described, under the name of "Maracot" — with excellent plates showing the plant, the flower, and the fruit — by Tobias Aldinus, in "Rarior. Plant. Horti Farnesiani" (Romæ, 1625), pp. 49–59.

or "fəxoes" were — as Navarrete notes, "Colec." i. 200, 203 — "the same as frejoles or judias," Spanish names for Kidney beans, which the Portuguese call "Feijaos." Oviedo (1525–35) speaks of the "fésoles, as the Spaniards call them, of which there are many kinds in the [West] Indias." These fésoles, he says (lib. vii. c. 18), "are called by Pliny 'fagivoles': in Aragon we call them 'judias,' and the seeds of those of Spain and of this country are properly the same." The natives of Hispaniola raise these fésoles, but they are much more abundant on the main land, especially in New Spain and Nicaragua. "I have, in the province of Nagrando in Nicaragua, seen them gather a hundred hanegas (bushels, nearly) of these fésoles: and they also, in that country and other parts of that coast, have many other kinds of fésoles, besides the common sort: some have yellow seeds, others spotted," etc. In another place (lib. xi. c. 1), Oviedo, mentioning plants that had been brought from Spain to Hispaniola and other parts of America, "in the beginning," names "Fésoles, called in Aragon 'Judias,' and in my country [Castile] 'Arvejas luengas': " but "of these, there is no need of bringing more seed, for in this island and on the main many bushels are harvested every year, and in the province of Nicaragua they are indigenous (naturales de la misma tierra), and a great number of bushels are produced yearly of these and of other fésoles of other sorts and different colors," etc.

From this time (1535) onward, nearly every writer who mentioned plants cultivated by the Indians named, together or in close connection, maize, beans, and pumpkins. Reference to several of these writers has been made in our notes on *Cucurbitæ*. Cabeca de Vaca found beans cultivated by the Indians of Florida in 1528, and again near the western limit of his wanderings (in New Mexico or Sonora) in 1535. De Soto, at his landing in 1539, found "fields of maize, beans, and pumpkins," near Tampa Bay; and at Coligoa (west of the Mississippi) "beans and pumpkins were in great plenty; both were larger and better than those of Spain;" and so, at other places, on his travels to the west and north.

Jacques Cartier, the discoverer of the St. Lawrence, on his

first voyage, 1534, found that the Indians near the mouth of that river on the Bay of Gaspé had abundance of maize, and had "beans (febues) which they name 'Sahu,'" or (as spelled in the vocabulary printed with his Discourse du Voyage) "Sahe."[1] The "Bref Recit" of his second voyage, 1535–36, mentions the use of corn and beans by the Indians of the St. Lawrence — "bled & febues & poix, desquels ilz ont assez" (f. 24).

Father Sagard in his "History of Canada" and in the account of his journey to the country of the Hurons, 1625, mentions the cultivation and use of "fezolles" by the Indians. The Hurons used in their succotash ("neintahouy") "a third or a quarter part of their fezoles, called ogaressa" (Grand Voyage, 83, 138).

Lescarbot, 1608, says that the Indians of Maine, like those of Virginia and Florida, plant their corn in hills, "and between the kernels of corn they plant beans marked (féves riolées) with various colors, which are very delicate; these, because they are not so high as the corn, grow very well among it" (Hist. Nouv. France, ed. 1612, p. 835; see also p. 744).

The relation of the voyage of Captains Amidas and Barlow to Virginia, 1584, mentions pease, melons, etc., at Roanoke Island, but does not name beans; but Harriot, who accompanied them on this voyage, includes both "Wickonzour, called by us pease," and "Okindjier, called by us beans," among the productions of that country. Capt. John Smith, who was in Virginia in 1607, and Strachey, who was there in 1610, describe (in nearly the same words) the Indian manner of planting corn and beans: "they plant also pease they call assentamens, which are the same they call in Italy fagioli: their beans are the same the Turks call garvances, but these they much esteem for dainties" (Smith's Gen. Hist., 28; Strachey, Trav. in Virginia, 117). Evidently, these names

[1] The language spoken by these Indians was a dialect of the Huron-Iroquois group, and we trace the name sahe (as Cartier caught it) in the Mohawk *osahe-ta*, "fésoles" of Bruyas (17th century), and the Onondaga *ousahèta* and *hôsahèta*, "poix, féve" (Shea's Onondaga Dictionary).

are confounded. Garvance was the French name of the Chick Pea (*Cicer arietinum*), the Spanish garbanzo; and it is not probable that the Turks gave this name to any kind of beans; while fagiuoli was the Italian equivalent of Latin phaseoli. Strachey's Virginian vocabulary gives assentamens (and otassentamens) for "pease," and peccatoas, peketawes, for "beans."

It must be remembered that at the beginning of the 17th century kidney beans — as well as vetchlings (Lathyrus) — were popularly regarded as a kind of pease, or "peason." Turner, in his "Names of Herbes," 1548, says that "Phasiolus otherwyse called Dolichos, may be called in English 'long peasen or faselles;' . . . in French phaseoles:" and "*Smilax hortensis*, . . . in French, as some wryte Phaseole . . . may be called in English Kydney beane," etc. (Eng. Dial. Soc., ed. 1881, pp. 62, 74). Lyte's "Dodoens," 1578, follows Turner for the English names of Phaseolus, "Kidney beane and Sperage; of some they are called Faselles, or Long Peason," etc. (p. 474): his "common Peason" and "middle Peason" are Ervilia (*Ervum Ervilia* L?) and *Pisum arvense* L.; while *P. sativum* is distinguished as "Great Peason, Garden Peason, and Branche Peason, because, as I thinke, they must be holpen or stayed up with branches" (id. 476).

So, on the continent, the Spanish names for fésoles was "arvejas luengas" (Oviedo), *i. e.*, long vetches, and the garden pea (*P. sativum*) was "un corto genero de Arvejas" (Calepin's Diction., ed. 1616), a short kind of vetches. The confusion of names is frequent in writers of the 16th and first half of the 17th centuries, in French as well as English and Spanish. "Magno sane labore" — as Tragus found — "Phasioli, Orobi, et Pisa, nec non Cicer arietina, maxime agrestia, secernuntur," (Stirp. Hist., 1552, p. 613).

Champlain uses "poix" and "fèves," interchangeably, as names of the American Phaseoli. In Breton's "Diction. Fr.-Caraibe" (1666) the Carib name, "caláoüana," stands for "pois de Brésil" and "febue de Brésil"; "pois rouges, dit Anglois, míbipi;" and "Pois, mancónti" — this being, probably, the (introduced) *P. sativum*.

Sagard (1624–5) says that the Hurons called the coarser part of their pounded maize — after the meal had been sifted from it — "Acointa, c'est à dire Pois (car ils lui donnent le mesme nom qu'a nos pois);" and in his "Dictionnaire Huronne," he has "Pois, Acointa," "Fezolles, Ogaressa;" whence we infer that French pease [*P. sativum*] were already cultivated by or known to the Hurons. The Abnakis of western Maine, in the 17th century, called pease, "awennootsi-minar," *i. e.* "French (or foreign) seeds." Tanner, 1830, gives as the Chippeway name of the "Wild Pea-vine" [*Phaseolus diversifolius ?*] "Anishemin," *i. e.* "Indian (or native) seeds." In nearly all North American languages, the names for kidney-beans (Phaseoli) are of earlier formation than those for garden pease. The latter are usually formed on the former: *e. g.* Chahta, *tobi*, "bean"; *tobi hullo*, [wild] "pea"; *tobi hikint ŭni*, "garden pea" (Byington); Dakota, *o^{n}mnicha*, "bean"; *o^{n}mnicha hmiya^{n}ya^{n}* [*i. e.* "round bean"], "pea."

Without multiplying citations — we may assume that the "pease" and "poix" which early voyagers found cultivated by the American Indian were species of Phaseolus — not Pisum.

Five and twenty years before the settlement of Virginia, the Indians of Carolina and Florida had "fine citroüilles and very good beans" (Lescarbot, Nouv. France, 778). Lawson, 1700–1708, gives a more particular description of the southern beans cultivated by the Indians. "The Kidney-beans," he says, "were here before the English came, being very plentiful in Indian corn-fields." "The Bushel bean," a spontaneous growth, very flat, white and mottled with a purple figure, was trained on poles [*P. multiflorus ?*]: "Indian Rounceval, or Miraculous Pulse, so called from their long pods and great increase; they are very good, and so are the 'Bonavies, Calavancies [= Garvances ?], Nanticokes,' and abundance of other pulse, too tedious to mention, which we found the Indians possessed of when we settled in America" (Voyage to Carolina, pp. 76, 77).

In the northern States, we find little difficulty in establishing the identity of *Phaseolus vulgaris* with the beans cultivated by the Indians at the first coming of Europeans. These were from the first distinguished as "Indian beans," from the Garden beans (*Vicia Faba*) introduced by the English. In 1609, Hudson, exploring the river which bears his name, saw at an Indian village — in the vicinity of Schodac and Castleton, Rensselaer County, N. Y. — "a great quantity of maize or Indian corn, and beans of the last year's growth" (Hudson's Journal, in De Laet, 1625, b. iii. ch. 10, and Juet's, in Purchas: N. Y. Hist. Soc. Coll., 2 Ser., i. 300, 325).

1631–42. The Indians of New Netherland "make use of French beans of different colors, which they plant among their Maize. . . . The Maize stalks serve, instead of the poles which we use in our Fatherland, for the beans to grow upon" (De Vries, Voyages, transl. in 2 N. Y. Hist. Soc., iii. 107).

1653. Van der Donck, in his "Description of the New Netherlands," distinguishes the beans cultivated by the Indians before the coming of the Dutch, and the Turkish beans which had been introduced: "Of Beans there are several kinds; but the large Windsor bean [*Vicia Faba*] . . . and the Horse-bean will not fill out their pods. . . . The Turkish beans which our people have introduced there grow wonderfully. . . . Before the arrival of the Netherlanders [1614] the Indians raised beans of various kinds and colours, but generally too coarse to be eaten green or to be pickled, except the blue sort, which are abundant," etc. He then describes the Indian mode of planting beans with maize, *ut supra* (N. Y. Hist. Soc. Coll., 2 Ser., i. 188–9). This is the only reference we have found to the introduction of any species of Phaseolus into North America. Van der Donck's book was written more than forty years after Hudson's coming, and the author first arrived in New Netherland in 1642. His statement as to the introduction by the Dutch of the best kind of "Turkish beans" for "snaps," salad, or pickling, is not to be accepted without reserve; but the fact that Turkish beans "grow wonderfully, fill out remarkably well, and are much

cultivated," while the imported Windsor beans [*Vicia Faba*] and horse-beans proved failures, is to be noted.

Wood, who was in Massachusetts from 1629 to 1633, says that the Indians "in winter-time have all manner of fowles, Indian beanes, and clams" (N. E. Prospect, pt. 2, ch. 6). Roger Williams, 1643, gives the Indian name of these beans in the Narragansett dialect: "Manusqussed-ash" (plural); Cotton's Massachusetts vocabulary (1727–8) has (sing.) "Mônasquisset, an Indian bean;" President Stiles, about 1760, heard the name in the Pequot dialect as "Mushquissedes" (MS. Vocab.); Zeisberger, 1776 and 1803, wrote it in the Delaware, with dialectic modification, "Malachxit;" and we can trace it in the modern Shyenne "Mónisk" (Hayden's Vocab., 1862) and "Monchka." In the Chippeway, the kidney-bean has received — probably from some local variety — a different name: "Miskodissimin," *i. e.* "red-dyed seed (or fruit);" and this name, modified as "M'skochī-tha," was used by the Shawanees of Ohio.

To return to New England, Josselyn, who was in this country, 1638–9, and again, 1663–71, in his catalogue of "plants proper to the country," names "Indian beans, falsely called French beans:" "the herbalists call them kidney-beans, from their shape and effects. . . . They are variegated much [in size and color]; besides your Bonivis and Calavances, and the kidney-bean that is proper to Roanoke: but these are brought into the country: the others are natural to the climate" (N. E. Rarities, p. 56; Voyages, p. 73–4). Here is reference to at least two species of American beans, one "proper to New England," the other from Roanoke — perhaps *P. multiflorus*.

Besides the names already mentioned — "Mônasquisset," with its variants — there is another, in northern Algonkin languages, for kidney-beans, which must have originally belonged to some high-twining variety. Eliot used it, in the plural, for "beans" in 2 Samuel, xvii. 28, *tuppŭhquam-ash* — which literally signifies "twiners;" and Rasles (1691–1700) gave, in the "Kennebec-Abnaki" of Maine, for "faséole," *a'teba'kwé* — from the same root. A modern Abnaki vocabulary shows that this name is still in use — as "ad-ba-kwa."

As to the American origin of *P. lunatus*, the Lima bean and its varieties, there seems now to be no question. "It is evidently," says M. De Candolle (p. 276), "a Brazilian species, dispersed by cultivation, and perhaps long ago naturalized, here and there, in tropical America."

But as to the origin of *P. vulgaris* he is not free from doubt. He finds "(1) that this species was not cultivated in ancient times in the East Indies, the southwest of Asia, or Egypt; (2) that we are not absolutely certain that it was known in Europe before the discovery of America; (3) that at that epoch the number of varieties suddenly increased in the gardens of Europe, and all authors began to speak of it; (4) that the majority of species of this genus are found in South America; and (5) that seeds which apparently belong to this species have been found in Peruvian tombs [at Ancon] of a date somewhat uncertain, mixed with many other species, all of which are American." (p. 275.)

The proof that *P. vulgaris* (and *P. nanus*), in varieties almost innumerable, were cultivated by the natives of America before the coming of Europeans, seems to be conclusive. The resolution of M. De Candolle's doubts as to the American origin of the species must depend chiefly on the identification of the species known as Phaseolí (Phaselí, Fagiuolí, Fésoles, etc.), in Europe, before the discovery of America. This identification may not be impossible, but the space at our disposal will not permit us to attempt it in this article, or even to reëxamine the authorities on which M. De Candolle admits the probability "that the Dolichos of Theophrastus was our pole bean (*haricot à rames*), and the Fasiolos our cultivated bush bean (*haricot nain*)," p. 271. At present, we have only to offer one or two notes.

1. The distinction indicated by Galen (De Alimentis, lib. i. cc. 25, 28) between the Phasiolos (φασίολος) of Dioscorides and Phaselus (φάσηλος) — presumably the "vilis faselus" of Virgil — if well founded, seems to have been lost sight of in the middle ages. In Italy, the Greek and Latin names Phasiolos, Faseolus, Faselus, Fasillus, etc., passed into the modern Fagiuoli. Piero de' Crescenzi, of Bologna, whose

treatise on agriculture was written near the beginning of the 14th century, in Latin, and translated into Italian about 1350, mentions, among field plants, Faseoli (Fagiuoli), as well known; "some of them are red, some white. . . . They are planted conveniently among panick, millet, and chick pease; they are also planted in gardens, among cabbages and onions." [1] It is not certain that the red and the white were of the same species, or genus, or that either was a species of Phaseolus, L. In the first half of the sixteenth century the white Phaseoli were the more common and less esteemed. The young and tender pods were eaten, with the included seeds, in salads, or boiled with other vegetables.

Two other early figures show that the Faseoli were not so "well known" to the herbalists of the 15th and beginning of the 16th century as to Crescenzi in the 14th. One is from a Venice edition of the "Hortus Sanitatis," 1511; the other from the "Tacuini Sanitatis" of Elluchasem Elimithar, Strasburg, 1531, p. 49. They are equally unlike the modern Phaseolus, the earlier figure in Crescenzi, and each other. The second may have originally been intended for a Teasel (*Dipsacus sylvestris*), the *Virga pastoris* of the herbalists. Calepin's Dictionary (ed. 1616) says, s. v. Faseolus, that the name Fasilli is now given by the common people to "a species of Cicercula." This "vulgaris Phaseolus" of Matthioli [2] and other Italian botanists of the period is figured and described in the later editions of his commentary on Dioscorides,[3]

[1] "De Agricultura," lib. iii. c. 10 (Italian, Ed. Venice, 1504). The Latin text was first printed at Strasburg in 1471, and with figures, 1486; the Italian version was printed at Florence, 1478. The figure of Faseolus in the earliest (Latin) edition we have seen, without date, but probably of Louvain, about 1480, has little resemblance to the Phaseolus of modern botany.

M. De Candolle remarks (p. 272) that "authors of the 15th century say nothing of Faseolus, or any analogous name," and that "this is the case with P. Crescenzi," — referring to a French translation of Crescenzi, printed in 1539, which we have not seen.

[2] "Vulgares Phaseoli, quibus passim in cibis vescimur, dum satis in campis virent, non repant," etc., Matth. "Apologia adv. Amathum," 1559, p. 33; "Vulgaris usus Phaseolus." *Ibid.* 31.

[3] Ed. C. Bauhin, 1598, p. 341. In the earlier edition (Venice, 1559, p.

as having an erect stem, ternate leaves, white flowers, kidney-shaped seeds, white, "except the umbilicus, which grows black." It may be worth noting that "a black spot in the place of the cotyledon" was a characteristic of the Faseoli described by Albertus Magnus (13th cent.), which appears to M. De Candolle "to be the dwarf Haricot (*P. nanus*) of our epoch." This black spot is more strongly suggestive of Dolichos than of any known variety of *P. vulgaris: e. g. Dolichos unguiculatus*, L. (French, *D. Mongette*, *Banette*, *Haricot cornille*) — not mentioned by M. De Candolle, but much cultivated in Italy, and of which there are a great number of varieties — which has seeds "marked by a prominent black spot about the umbilicus."[1]

2. M. De Candolle (p. 272), with a reference to Delile and to Piddington's "Index," remarks that though "no Hebrew name corresponding to the Dolichos or Phaseolus of the botanists" is known, yet "a name less ancient, because it is Arabic, namely 'Loubia,' is found in Egypt for the *Dolichos Lubia;* and in Hindustani, under the form 'Loba,' for *Phaseolus vulgaris*." This name seems to be clearly referable to the Greek. It has not been traced earlier than to Jahia ebn Serapion — an Arabian physician of the 9th or 10th century — whose work "De Simplicibus," compiled chiefly from Dioscorides and Galen, was translated into Latin in the 15th century.[2] In a chapter (lxxxi) on "Lubia," *i. e.* "Faseoli," he quotes from Dioscorides the description of *Smilax hortensis* (χηπαία σμίλαξ) "whose seeds some call Lobia;" and it is evident that the name Lubia (as it was transliterated from the Arabic text by the translator) was transferred to the

264), Phaseolus is figured as a low, bushy, and spreading, but not twining plant.

[1] Several other species of Dolichos (*e. g., D. sesquipedalis* L., Ital. "Fagiuolo Sparagio," Engl. "Asparagus bean") are similarly marked. Vilmorin-Andrieux et Cie., "Les Plantes Potagères," 1883, p. 280. Other names for this species are: Germ. "Ostindische Riesel-Spargel Bohne," "Nagelische Fasel;" Ital. "Fagiuolo dell' occhio;" Span. "Garrubia," "Moncheta," "Judia de Careta."

[2] Milan, 1473, and Venice, 1479; but better known to botanists of the 16th century in the Strasburg edition of 1531, edited by Otho Brunfels.

Arabic from the Greek of Dioscorides. It is probable, to say the least, that it has been rightly appropriated to *Dolichos Lubia*, Forskal (De Candolle, 278), rather than to any species of Phaseolus.

The length to which our annotations have extended forbids all notice of the third part of this book. This, however, is very brief. It contains a tabulation of the plants of cultivation, and of the results of the preceding discussion of them; also an article on the regions in which the principal species have originated or have been brought into cultivation, in which it is stated that of the 247 species under investigation the Old World has furnished 199, and America 45, leaving three which are doubtful in this regard; and the extreme poverty of the southern hemisphere beyond the tropics is a striking feature. An article on the number and nature of species cultivated at different periods is noteworthy. So, also, is the enumeration of the cultivated plants which are unknown in a wild state; from which it is gathered that 27 species have never been found wild by any botanist, 27 more are doubtful in this respect, while 193 are of recognizable origin.

Of the "Reflexions diverses," at the close, we note only the final one, that "In the history of cultivated plants I have found no indication of communications between the inhabitants of the Old and New World anterior to the discovery of America by Columbus. The Scandinavians, who had carried their expeditions to the northern United States, and the Basques of the Middle Ages, who had extended their whaling voyages perhaps to America, would appear not to have transported a single cultivated species. The Gulf-stream has equally been without effect. Between America and Asia two transportations may have been effected, one by man (the Batatas), the other either by man or by the sea (Cocoa-nut)."

Perhaps the Banana should be ranked with the Sweet Potato in this regard. And we may merely conjecture that the Purslain came to our eastern coast with the Scandinavians or the Basques.

BENTHAM AND HOOKER'S GENERA PLANTARUM.

THE completion of this great work[1] marks an era in systematic botany. As the senior author is nearly eighty-three years of age, we may say that, essentially, it brings to a worthy close the long course of scientific labors of the most accomplished, sagacious, and continuously industrious phytologist of our day, almost the only survivor of those who personally knew Antoine Laurent Jussieu, and were associated with the elder De Candolle; one whose line of authorship, begun in the year 1826 (antedated only by that of the venerable Roeper), comes down to the present year, apparently, in almost unabated vigor, and is as remarkable for sustained importance as it is unparalleled in length. Upon the junior author, who should not yet feel the burden of years, however weighted with official cares, and upon such specialists as can be mustered, may devolve the responsibility of such *addimenta* as may be needed to bring the earlier volumes up to the date of the last, and even to supplement the work from time to time, as the "Genera Plantarum" of Linnæus was supplemented by successive editors. But it is not probable that the task which has now been happily accomplished will be undertaken again, still less executed, before the twentieth century is well entered upon. Useful compilations we may expect, and monographs which may here and there better represent advancing knowledge of particular groups; but a production like this, covering the whole field of phænogamous botany, ordinally and generically, and with the uniformity of treatment and scale thus secured by two close associates in one continuous work, cannot be looked for again for a long while.

It has few predecessors. The first "Genera Plantarum" in fact (though not exactly in name) was that of Tournefort, published in the year 1700. Then and there, as Linnæus de-

[1] *Genera Plantarum ad Exemplaria imprimis in herbariis Kewensibus servata definita.* Auctoribus G. Bentham et J. D. Hooker. Londini, 1862–1883. (The Nation, No. 942, July 19, 1883.)

clared, genera of plants, in the sense of scientific botany, were first established. There would probably be more recognition of this dictum if the present work were to be planned anew, and the genera which Linnæus himself admitted as of Tournefort, along with those which modern botanists have restored, could have been attributed to this real founder, without thereby compromising the proper position to hold in respect to herbalistic and ancient names. The second "Genera Plantarum" was that of Linnæus, in 1737, of which the last edition revised by the author himself was that of 1767. The third was that of Jussieu, "secundum ordines naturales disposita," which appeared in the year 1789. That of Endlicher — a monument of literary or bibliographical erudition rather than of botanical research — was brought out in the main between 1836 and 1843, at about the same time with the more unpretending synoptical compilation of Meisner. These were important in their way. But the "Genera Plantarum" of Bentham and Hooker, which began to be issued in the year 1862 and was finished in the spring of the present year, is the lineal successor of the three classical works above mentioned, that of Tournefort representing the botany of the close of the seventeenth century; that of Linnæus the first half, and that of Jussieu the latter part, of the eighteenth century. The present work — increased from the one small octavo of Linnæus to three thick imperial octavo volumes of nearly 1200 pages each — stands in like relation to the nineteenth century, and is based, like them, or even more than they, upon actual investigation, and upon the comparison of a vastly greater number of original types than was formerly possible.

Unlike its predecessors, however, — and in this respect agreeing with the other great botanical work of the century, the "Prodromus" of De Candolle, — the whole of cryptogamic botany is omitted. This vast field must be left to specialists. Fries, of Upsala, who died a few years ago at a ripe old age, was the last phænogamic botanist who was at the same time master of one or two cryptogamic orders; and now even the best of cryptogamists can hardly aspire to more

than a general and superficial acquaintance with any other department than the one to which he devotes himself. This inevitable state of things has its disadvantages. The reasons for it do not really apply to the Ferns and their allies, and it was naturally expected, as it is much to be desired, that these should enter into the present work. May we hope that this still may be?

Some idea of the progressive enlargement of the field may be had by a comparison of the number of genera characterized in these successive works. The phænogamous genera of

Linnæus, "Gen. Pl.,"	ed. 1, A. D. 1737,	were . .		887
" "	ed. 6, A. D. 1764,	" . . .		1,189
Jussieu,	". . . . A. D. 1789,	" . . .		1,707
Endlicher,	". . . . A. D. 1843,	" (about)		6,400
Bentham & Hooker,	". . . . A. D. 1883,	" . . .		7,585

If the last had been elaborated upon the scale of Endlicher, or with the idea of genera which is still common if not prevalent, the number of genera would have amounted to at least ten thousand. An estimate of the number of known species of each genus and higher group has been made throughout the work — a rough approximation only, mentioning first the number in the books, and the number to which, in the opinion of the authors, these may probably be reduced by botanists who adhere to the Linnæan view of species; from which it appears that upon the very strictest estimate their number, as now known to botanists, is at least 95,620. In round numbers, it may fairly be said that about 100,000 species of phænogamous plants are in the hands of botanists. The five largest orders, as well for genera as for species, are the following, and in this rank: *Compositæ*, *Leguminosæ*, *Orchideæ*, *Rubiaceæ*, *Gramineæ*. The high standing of the Orchid family in the list will be a surprise to many. Linnæus knew only a hundred species; five thousand is now a moderate estimate — about half as many as there are of *Compositæ*, which hold to their proportion of one tenth of the whole. In both families every country and district is largely peculiar in its species and types. The far greater prominence of *Compositæ*

over Orchids is owing to the vast number of individuals in the former, their paucity in the latter.

Those who desire to know the respective parts which the two authors have taken in the elaboration of the "Genera Plantarum" may be referred to a short article on the subject in a recent number of the "Journal of the Linnæan Society of London." Great thanks from all botanists are due to them both.

BOTANICAL NOMENCLATURE.[1]

SIXTEEN years have passed since M. De Candolle laid before the International Botanical Congress held at Paris, August 16–26, 1867, a body of Laws of Botanical Nomenclature, which he had drawn up for consideration by that assembly. The code was discussed by a special committee, afterward by the congress in full session, some modifications introduced, and it was then all but unanimously voted, "by about one hundred botanists of all countries:" "That these laws, as adopted by this assembly, shall be recommended as the best guide for nomenclature in the vegetable kingdom." The adopted code, with an extended commentary, was published by De Candolle early in the autumn of the same year; and an English translation, made by the lamented Dr. Weddell, appeared early in 1868. The "Laws," but without the more voluminous explanatory commentary, were reprinted from the English translation in this Journal in July of that year, occupying only twelve pages; and some remarks and suggestions by the present writer were appended. As was then said, the code did not make, but rather declared, the common law of botanists. It announced principles, systematically and perspicuously, and indicated their application in leading cases; but many practical questions, as well as conflicts of

[1] *Nouvelle Remarques sur la Nomenclature Botanique.* Par M. Alphonse De Candolle. Geneva, 1883. (American Journal of Science and Arts, 3 ser., xxvi. 417.)

rules in particular instances, which would inevitably come up, were necessarily left to be settled when they arose. No small discussion upon certain details has indeed ensued, in which our author, naturally appealed to, has taken an active part.

In the first part of the present publication (of 79 pages 8vo), the discussions of the intervening years are summed up and reviewed; and a few changes, which experience has shown the need of, are proposed. In the second part the author takes up certain questions which the Paris Congress left untouched, such as the nomenclature of organs (which he treated in his recent "Phytographie"), the nomenclature of fossils, and the rules according to which names and authorities should be cited when old genera are combined or reconstituted; also some matters of orthography and punctuation are briefly considered. Finally, in the third part, the laws adopted by the Paris Congress are reprinted, with the suggested changes. The alterations and additions are printed in italic type, so that they may be seen at a glance.

We are not sure of an English edition; and in any case it is desirable to make so important a publication as this generally known to our own naturalists. So our abstract and comments may run to some length. They are intended partly to illustrate and reinforce the author's doctrine, in respect to matters upon which there is still diversity of opinion and practice, perhaps occasionally to offer a criticism or suggestion; also as helps and guides to botanists in our own country, who are beginning to take interest in such matters and to feel (or at least to show) the need of giving attention to them.

As respects all the weightier matters of the law, the most experienced phænogamous botanists are in general agreement. Having accepted the code of 1867, they will be ready to accept proposed modifications and interpretations which are in accordance with its principles. Among them all, perhaps no one is so well qualified as De Candolle, by the bent of his mind and course of his studies, his opportunity of leisurely consideration, and his long editorial experience — combining, as it may be said to do, that of two generations, — for declaring what the present consensus of authority is, and for

discussing the cases which still need adjustment, some of them of considerable consequence in phytography.

At the head of the publications upon nomenclature emanating from or sanctioned by associations or committees of naturalists which have appeared since the year 1867, De Candolle places the Report to the American Association for the Advancement of Science, at the Nashville meeting, 1877, on Nomenclature in Zoölogy and Botany, prepared by Captain Dall, after much conference with leading American naturalists. The differences between these rules and those of the botanical code relate mainly to questions mooted by the zoölogists, whose systems and views had been comparatively loose and variant. The first alteration in De Candolle's revised draft is adopted from Dall's report, with high commendation, namely: —

"Art. 3. The essential point in all parts of nomenclature is: 1. Fixity in names; 2. Avoidance or rejection of forms or names which may create error or ambiguity or introduce confusion into science." The "fixity in names" is taken from the American code, and is said to supply a real omission. Although merely declaratory, some practical consequences flow from it. The same idea dominates in the report made by Douville, chairman of a committee of the Geological Congress at Bologna in 1881, and which concerned itself with nomenclature in palæontology. This report insists that "The law of priority being fundamental in nomenclature, it appears to be necessary to apply it with all possible generality and to suppress derogations and exceptions to this law. . . . Contradiction between the signification of a name and the characters of a genus or species is no sufficient reason for changing such a name," etc.

Another code referred to is one by a committee of the Zoölogical Society of France, in 1881, M. Chaper, chairman, in which this principle of fixity is said to be less prominent, more exceptions being allowed.

On reviewing the whole field, De Candolle assures us that the tendency during the last sixteen years has been: 1, to an increasing agreement of the zoölogists with the botanists;

and 2, an increasing recognition of the law of priority as the fundamental principle of nomenclature, and as prevailing over considerations of elegance, linguistic purity, and precise meaning. But some recent publications of individual botanists look the other way; notably so two publications by Saint-Lager, "Réforme de la Nomenclature Botanique," and "Nouvelles Remarques sur la Nomenclature Botanique," published at Lyons, in the Annals of the Botanical Society of that city; of which it may be said, that the reflex effect of the multitudinous changes proposed in the view of making old and accepted names better, more classical, or more significant, has greatly strengthened the hands of those who contend for the absolute fixity and unalterableness of published names. Whether and to what extent misspelled or otherwise wrongly formed names may be corrected, and whether in some cases the principle of fixity should not prevail over absolute priority, are matters which may be discussed further on. We take up in the author's order the points which we wish to specify or to comment on.

Article 6 of the code declares that "scientific names should be in Latin. When taken from another language, a Latin termination is given to them except in some cases sanctioned by custom." Here our author asks, "But what is Latin?" He concludes that the Latin of Linnæus should be the model. It is the classical language of botany, and is much more precise than the Latin of antiquity, in which very many words bear two, three, or half a dozen senses, either in the same or in different ages; while in the technical language of botany each word has but one meaning, and each idea or object is expressed by a single term. De Candolle elaborated this point in his "Phytographie," to which he refers for illustrations; and he returns to it in special applications when commenting on article 66, as we shall see. But, in fact, even technical language cannot always avoid ambiguities and the use of words in senses which have to be determined by the context. *Folium*, for example, in botanical description, may mean the blade of a leaf only, or this along with its petiole, or blade with petiole and stipules together, or it may even mean any homologue of the ordinary leaf.

"Art. 15. Each natural group of plants can bear in science but one valid designation, the most ancient, whether adopted or given by Linnæus, or since Linnæus, — provided it be consistent with the essential rules of nomenclature." De Candolle now adds an article 15bis, which is purely explanatory, but has a bearing upon subordinate questions. It is: "The designation of any group, by one or more names, has not for its object the enunciation of its characters or history; it gives merely the name by which we are to call it." He comments upon the tendency which is often shown to mix up the question of name with other considerations. Before Linnæus introduced the binomial system, the names of species were at the same time names and characters. In separating these two things, Linnæus rendered a great service, and we should be careful to preserve this advantage. "A name is a name; characters are characters; the succession of names is synonymy. To mingle such different ideas leads to confusion. In these days there is a disposition to attribute too much importance to the meaning of names, and also to intermix the synonymy — *i. e.*, the bibliographical history of the groups — with the names, at least with the indication of the author, which being commonly annexed comes to be almost a part of the same. Such complications are contrary to the general principle that different ideas should be expressed separately. If this rule is neglected, we may be led into attempts to express in the name, or with the name, the phytogenitic history of a group, that being just now one of the ideas in vogue." Names that have an appropriate meaning are very well, and botanists always endeavor to make such; but experience shows that meaningless names are in some respects better — are generally better than names founded on ideas, which in the progress of knowledge often become false.[1]

De Candolle's main remarks upon article 15 relate to the

[1] Or may be essentially false from the beginning. One of our common Maples has two names, *Acer dasycarpum* and *A. eriocarpum*, both signifying that the fruit is woolly, whereas it is perfectly glabrous; only the ovaries are woolly, yet no botanist has ever proposed to change the received name, — which is remarkable.

point of departure for the law of priority in botany. The names of the two great classes, Monocotyledones and Dicotyledones, are of Ray in 1703, who scientifically distinguished and named them. The counterpart, Acotyledones, is of Jussieu (1789), and to all three names our author would apply the law of priority. That the name Acotyledones fails to express the true character is a small objection. This is only an example of the disadvantage of significant names, which may lose aptness in the advance of knowledge. Far better are such names as Aves, Pisces, Vermes, etc., which time and discovery can never falsify. Cryptogamia as the name of a class (which for meaning is hardly so good as Acotyledones) was introduced by Linnæus in 1735, and has maintained its place.[1] The counterpart, Phanerogamia (or Phænogamia), is not of Linnæus, but much later. Natural Families or Orders date from Jussieu's "Genera Plantarum," in 1789; Cohorts and Tribes, from A. P. De Candolle's "Systema," 1818. Subgenera begin with R. Brown, in 1810, according to our author. There are a few instances (without the name) in the "Prodromus Floræ Novæ Hollandiæ." But it is in the Oudney and Denham-Clapperton paper, in 1826 (p. 16), that their use is discussed, and the mode of designating them in citation by interpolation between generic and specific name in parenthesis, is introduced. Species (as distinguished from varieties) and the actual binomial nomenclature date from 1753, and the first edition of the "Species Plantarum." There is substantial agreement among botanists as to this point of departure; and the fact that the specific phrase of earlier authors is occasionally of a single substantive does not militate against it. *Galega vulgaris*, *Lappa major*, and *Trifolium agrarium* of the old herbalists were in good binomial form; but the adjectives are phrases, not specific names.

Generic names bring in a question of interpretation and usage. In his table De Candolle makes the point of departure for priority, Linnæus, "Genera Plantarum," ed. 1, 1737. All

[1] On p. 58 De Candolle has collected nineteen synonyms of the name Cryptogamia, all of later date, and specified the objections which may be brought against each of them, besides that of want of priority.

agree, or should agree, that no anterior name has right of priority to a Linnæan name or to a name adopted by Linnæus. But as respects generic names adopted by him, are we to follow Linnæus or are we not? He says, "Tournefortius primus characteres genericos ex lege artis condidit." And in the "Genera Plantarum": "Ipsi non immerito inventionis gloriam circa genera concedere debeam," — and so he uniformly accredits to Tournefort the generic names adopted from him; and the same as to "Plumerius, . . . Vaillantius, Dillenius, . . . Michelius et pauci alii," "qui ejus vestigia presserunt." De Candolle remarks that Tournefort had the merit which Linnæus ascribes, but that "he kept a good many adjective names for genera (Acetosa, Bermudiana, etc.)." Since Linnæus did not adopt these, they are out of the present question. Moreover, not to speak here of a score or two of really adjective generic names, Linnæus himself adopted two which Tournefort had discarded, Mirabilis and Impatiens, and deliberately made another, Gloriosa, in place of a proper name, Methonica, of a sort which, though not of the best, is now regarded as next to the best. But it is completely understood that Linnæus is not to be corrected; so Gloriosa, Impatiens, etc., remain.

Are we equally to follow Linnæus in regard to names which he adopted from Tournefort and a few later authors, some of them his own contemporaries? If so, we shall continue to write Salicornia, Tourn., Corispermum, A. Juss., Olea, Tourn., Justicia, Houst., Dianthera, Gronov., Lycopus, Tourn., Linnæa, Gronov. The practice of the leading botanists has been essentially uniform in this respect, from Jussieu down to De Candolle, father and son, even to the latest volume of the "Monographia," published during the current year. It seems perfectly clear therefore — although we believe that the question is not raised in this revision — that such genera are expected still to be cited as of their respective founders. And, as hardly any one doubts that Tournefortian genera suppressed by Linnæus but restored by modern botanists (such as Fagopyrum) are to be cited "Tourn.," it follows that only in a restricted sense do genera begin with Linnæus in the year 1737. This case, indeed, is governed by the principle in

citation, so well insisted on by M. De Candolle, that no author is to be represented as saying the thing that he does not say. The alternative course is to write "Linnæa, Linn.," which is certainly what Linnæus has not said. The only authors we know of who have on principle followed this alternative — and a notable exception it is — are Bentham and Hooker in the new "Genera Plantarum," against which a protest was made in this Journal when the first part was issued. They have followed the rule that botanical genera began with Linnæus so strictly as to cite even authors as recent as Gærtner for Tournefortian genera and to ignore botanists like Gronovius, contemporary with Linnæus, and publishing since the year 1737; and it is only by an infraction of their rule that they have avoided writing Linnæa, Linn.

No change of rule 15 seems actually required to bring it into unison with the almost universal practice in citation. We have only to understand that genera adopted by Linnæus from Tournefort, etc., and so accredited, should continue to be thus cited; that the date 1737 (Linnæus, "Genera," ed. 1) is, indeed, the point of departure from which to reckon priority, yet that botanical genera began with Tournefort; so that Tournefortian genera which are accepted date from the year 1700. That is the limit fixed by Linnæus, and it definitely excludes the herbalists and the ancients whose writings may be consulted for historical elucidation, but not as authority for names.

Upon articles 21 and 22, which give rules for the names of orders and other supra-generic groups, our author offers no new remarks. We venture to offer two. It being the general rule that *aceæ* is the proper termination for ordinal names which take their appellation from a typical genus, it is desirable to conform to it as fully as well may be.[1] Since *Saxifragaceæ*, *Myrsineaceæ*, *Styracaceæ*, *Gentianaceæ*, *Nyctaginaceæ*, and even *Lauraceæ* and *Juglandaceæ* were adopted

[1] *Cruciferæ*, *Leguminosæ*, *Umbelliferæ*, *Compositæ*, *Labiatæ*, and the like, are no exception to the rule, rightly stated, as they are not named from typical genera. We shall not have any more of them, but the old ones in use are among the best.

in the "Prodromus," it seems to us retrograde and unadvisable to have gone back in the new "Genera Plantarum" to *Saxifrageæ*, and the like; and this upon no obvious principle, as we have *Samydaceæ*, *Cornaceæ*, etc., brought into harmony with the rule. And if in the "Prodromus" we have *Styracaceæ*, why not also *Salicaceæ?* And if, in the "Genera Plantarum," Styrax could take the *aceæ* termination as *Styraceæ*, why not as *Styracaceæ?* If it be objected that some such terminations have an unclassical aspect, this objection applies all the more to the cases under our second remark: Namely, that too rigid adherence to the rule that names of suborders, tribes, etc., shall end in *-eæ* and the like, gives us nearly unpronounceable words of four or five consecutive vowels, or, when the diphthongs are printed in separate letters, according to a prevalent fashion, one or two more. Of these — the diphthongs written out — *Saurauieae*, *Spiraeeae*, *Catesbaeeae*, *Jaumeeae*, *Thymeleeae*, and *Moraceae* are the worst instances, and would justify any infraction of rules.[1] The last, and one of the worst, would have been avoided by writing the ordinal name *Iridaceæ* when that of the tribe would have been *Irideæ*. Names are to be spoken as well as read, and botanists who have to teach think more of these things than those who only write.

At the head of his remarks upon generic names (art. 25 et seq.), our author commends to other naturalists the very clear directions given in the rules for Zoölogical Nomenclature, edited by Dall, for rendering Greek letters into Latin in the construction of generic and specific names. He notes, however, that the rendering of η by *e* is not in full accordance with Latin usage, as witness *bibliotheca*, *dialectica*, *Hecubea*, etc.

[1] Far better to write *Spiræaceæ*, with De Candolle. The use of this termination for tribal names need not be objected to by those who take little pains to use it for orders. And those of us who are careful so to employ it, would prefer its occasional use for tribes and suborders to the concatenation of vowels, which it is not easy to write and almost impossible to pronounce. Some quite unnecessary tribal names in *aceæ*, such as *Vernoniaceæ* and *Eupatoriaceæ*, adopted by De Candolle from Lessing, are kept up, although exceptional.

Article 28 of the code, which is reprinted without alteration or comment, consists of a series of recommendations of points to be attended to in the construction of generic names. As such names are to be "in Latin," it would have been well to recommend that in their formation from the Greek, from which most of them are nowadays drawn, the principles of Latin prosody should not be wholly ignored. Such names as Trichóclădus and Ancistróclădus would have been euphonious as Trichocladia and Ancistrocladia, and very little longer. Acanthópănax, Didymópănax, Dimorphóchlămys, Trigonóchlămys, Aulacócălyx, Pellácălyx, Micróchăris, and the like, are harder to pronounce than the makers probably thought; and so of many others.

One of the actual recommendations is "Eviter les noms adjectifs." This in Weddell's version is translated, "To avoid adjective nouns:" doubtless a wrong translation. Adjective nouns we take to be substantives which are directly formed from adjectives. Not many such are likely to be made for genera; but if such good ones can be constructed as those we already have in Nigella, Amarella, Flaveria, Chlora, Rubia, Leucas, and Hyptis, they will not be objected to. Clearly the recommendation is to avoid adjective names for genera. That may be done for the future, but has not been done in the past. They are contrary to the rule of Linnæus, but not to his practice. Not to refer again to Gloriosa, Mirabilis, and Impatiens, at least two score of obviously adjective names for genera may be counted in the first edition of the "Genera Plantarum," — such as Arenaria, Stellaria, Utricularia, Dentaria, Asperula, Angelica, Trientalis, Pedicularis, Digitalis, and from the Greek such as Polycarpon. Amphicarpum and Mitracarpum are recent names of this kind. To conform the rule to the fact it were better to state that: generic names are either substantives or adjectives which may be used as substantives, the latter mostly feminine in gender. Angelica is understood to be *Planta Angelica*, Sanguinaria, *Planta sanguinaria*, etc.

It is recommended "to avoid making choice of names used in zoölogy." But it has become nearly impossible to follow this advice, nor is it now thought to be important.

Article 33 is suppressed. It was no more than a statement of the custom that personal names for species were to be nouns in the genitive (*e. g.*, *Clusii*) when the person commemorated was a discoverer, describer, or an illustrator of the species, but were in adjective form (*e. g.*, *Clusiana*) when the name was merely complimentary. The rule sometimes worked awkwardly; for many personal names do not take kindly to latinization in the genitive form, which are sufficiently euphonious as adjectives in *-ana* or *-anum;* and it is well to do away with a needless restriction.

Article 34, which recorded the fact that many names of species are substantives, is the subject of a few remarks, called out by the publication of Saint-Lager, in which it is proposed to change all these into adjectives, — a proposition which botanists are not likely ever to adopt. Some of them are among the best of descriptive names, *e. g.*, *Stellaria nemorum*, *Convolvulus sepium*, *Rhus vernix*, *Chamærops hystrix* (and such should be written without a capital initial); those which are proper names, either old names of genera or of the herbalists, are rightly said to be significant either of the absorption of a former genus, or of a transference, or as preserving a native appellation, or as indicating a likeness. "*Digitalis Sceptrum* means a Digitalis which had been called *Sceptrum;*" *Ardisia Pickeringia*, a species of this genus which, mistaken by Nuttall for a new one, had been named Pickeringia; *Rudbeckia Heliopsidis*, a *Rudbeckia facie Heliopsidis*, from its resemblance to a Heliopsis. Linnæus gave us many such names; and no sufficient reason appears either for discarding these, or for forbidding the discreet adoption of new ones. But we cannot commend such a name as *Senecio Bhot* for a species indigenous to Bhotan.

Article 36 consists of a series of recommendations for the formation or adoption of specific names. Its fifth sub-article we may refer to in another connection, namely, along with article 48. The recommendation to "name no species after one who has neither discovered, nor described, nor figured, nor studied it in any way," should be respected; yet there are occasions for departing from it, especially in case of new species

in very large genera. Excellent and sometimes needful is the advice to "avoid names designating little known or very limited localities." We are obliged to cite — happily as a synonym — *Helenium Seminariense*, published by a Professor who thought he had discovered a new species of Helenium in the vicinity of the "seminary," in one of our southern States, where he taught botany.

Article 40 suggests that names of varieties originated in cultivation, and still more half-breeds and sports (so important for horticulturists to distinguish), should have only fancy names, generally vernacular, and in some form as different as possible from the Latin specific names of botany, — names which, when needful, may be appended to the botanical name of the species, when that is known, *e. g.*, *Pelargonium zonale*, Mrs. Pollock. This has been seconded by the editor of the "Gardeners' Chronicle" and other judicious experts, and is slowly making its way.

Article 42, treating of the conditions of publicity, is the subject of additional remarks. The rule is, that "Publication consists in the sale or the distribution among the public of printed matter, plates or autographs. It consists, likewise, in the sale or distribution, among the leading public collections, of numbered specimens, accompanied by printed or autograph tickets, bearing the date of the sale or distribution." De Candolle now remarks that distribution among the members of an exchange club, of collections not offered to the public, does not come up to the rule; also that, as Dr. J. Müller states, the distribution of specimens without characters or any indication of the reason for calling it new, is nearly tantamount to announcing a species or genus in a publication, but without characters; which article 46 declares is not publication. But the cases are not quite alike. The possession of the named specimen enables a botanist to ascertain its distinctions. A published description without access to specimens may or may not serve the same purpose, very often does not. Unfortunately an insufficient or even a misleading description — and we have many such to deal with — claims the same right of priority that a good one does.

It is well, therefore, that publication by sufficient distribution of named specimens should be recognized. But the remark is true that, in fact, very few distributed collections fulfill all the requirements of article 42.

Article 47, sect. 2, recommends botanists "to publish no name without clearly indicating whether it is that of an order or of a tribe, of a genus or of a section, of a species or of a variety — in short, without giving an opinion as to the nature of the group to which the name is given." Unless this is attended to, and unless citations are equally precise, — for instance, unless subgeneric names are cited as such and not as generic, and *vice versa*, — much confusion in synonymy and in indexes will ensue.

Article 48, on the citation of the authority for generic and specific names, and matters herewith connected, involves questions which have been more disputed than any other. In the revised article the phrase printed in italic type is interpolated: "For the indication of the name or names of any group to be accurate and complete, and for the ready verification of the date, it is necessary to quote the author who first published the name or combination of names."[1] The statement might

[1] M. De Candolle appears to insist upon this verification (or incipient verification) for higher groups as well as for genera and species, and would deprecate the not unusual custom in compendious Floras, Catalogues, etc., of omitting to cite the authority for orders, sub-orders, tribes, etc. We should agree with him if the omission was held to signify that the names of the groups in question, when thus simply given, were proposed as new. But in fact, authority is omitted, not because the groups are new, but because they are old and entirely familiar. Nobody will ever suppose that *Ranunculaceæ*, *Clematideæ*, etc., nakedly written, are novelties. In this regard, the nature and plan of the publication are to be considered; what is necessary in a Systema or a Genera Plantarum may be superfluous in a local or a compendious work. Indeed it may be nearly impossible to assign the authority for the name of an order correctly, without explanation and extended references. One would wish to write, succinctly, *Cornaceæ*, DC., as De Candolle founded the order, but it was in the form *Corneæ*. A well-known order was instituted as *Onagræ*, Juss., "Genera Plantarum," which the founder altered to *Onagrariæ*; for reasons referred to in this article we may wish to adopt the form *Onagraceæ*. As all this is most familiar matter, yet may not be correctly

be simplified by omitting "or names" and "or combination of names," on the ground that the name of a plant is one, that it has a name not names, its name being the generic followed by the specific appellation. *Ranunculus bulbosus* is one name of two words. Our veteran botanist, Bentham, has insisted upon this; and it has a bearing upon the mooted question of mode of citation of authority.

The governing principle for the citation of authorship, etc., is well declared by De Candolle: "Never make an author say that which he does not say." It is difficult to go wrong when this principle is kept in mind, and when it is also understood that the appended name of an author, or its abbreviation, makes no part of the name of the plant, but is only the initial portion of its bibliography. Those who take a different view seem to have fallen into it by failing to distinguish strictly between name and history, and especially by mixing the history of a preceding with the statement of an actual name. A single example may illustrate this. When we write "*Mathiola tristis*, Brown," we give the name of a certain kind of Stock and the original authority for it; and we may, when needful, complete the citation by adding the name of the book, with the volume and page, where it was first published. If, with some, we write "*Mathiola tristis*, Linn.," we make an untrue statement. Linnæus had a wholly different genus *Mathiola*, and no *M. tristis*. If we add "sp.," and somewhere explain its import to be that the latter half of the name was given by Linnæus, the other half remains unaccounted for. And we have still to seek in the synonymy for the name of the genus under which Linnæus knew the plant, and also for that of the author who transferred it to Mathiola. If, with others, we write "*Mathiola tristis*, Linn. (*Cheiranthus*)," or "*Mathiola tristis*, Linn. (*sub-Cheirantho*)," our longer phrase still wants

stated without somewhat detailed exposition, why not in a local or condensed botanical book write simply *Onagraceæ*? The proper exposition is in place in a Genera Plantarum; and it would have been better if Bentham and Hooker had critically attended to this, instead of referring merely to the preceding work of Endlicher. It would have added somewhat, yet not very much, to their great labor.

the essential part of the citation. If, to secure this, we write "*Mathiola tristis*, Linn. (*Cheiranthus*, Brown)," our name, if it may be so called, now extended to five words and two signs in print, or of seven words when spoken, is still ambiguous and confused. It is a jumble of synonymous names and authorities, which become explicit and clear only when we translate it into "*Mathiola tristis*, Brown (*Cheiranthus tristis*, Linn.)," that is, into name and synonym, with respective authorities. This is clear and literally truthful; the injection of the synonymy into the name is neither. Linnæus reformed nomenclature by freeing the name from the descriptive phrase. The school in question would deform it by rebuilding, in another way (as De Candolle observes), ante-Linnæan phrases, only making them historical instead of descriptive.

The practice of appending the authority to the name whenever the species is mentioned has been so strictly and pedantically adhered to that many take the former to be a part of the name. To obviate this impression, it might be well to treat the names of common plants as we do those of genera; that is, to omit the reference to authorship in cases where there is no particular need of it. Not, however, so as to cause any confusion with the cases referred to in the following paragraph: —

"When a botanist proposes a new name . . . it is impossible for him to cite an author; consequently the absence of such citation suffices to show that the name of the species or other group is new. Linnæus, Lamarck, De Candolle, R. Brown, Martius, etc., followed this course. It is then a useless complication of many modern naturalists to append 'mihi,' 'nobis,' 'sp. nov.,' 'gen. nov.,' etc., to a new name. A large majority of species, genera, and families were published without these wholly personal indications." This is good as a general rule; but the "gen. nov." and an indication of the order or tribe are often needful.

No new comments are made upon article 49, probably because the practice of botanists generally is conformed to it. The article reads: "An alteration of the constituent characters, or of the circumscription of a group, does not warrant the

quotation of another author than the one that first published the name. . . . When the alteration is considerable, the words 'mutatis char.,' or 'pro parte,' or 'excl. syn.,' 'excl. sp.,' etc., are added," etc. The translation would have been better worded, "does not warrant the quotation of another author in place of the one that first published the name." For, in fact, the addition of the reforming author's name to the citation is often warranted and helpful, sometimes is almost a necessity, in the case of genera. It appears that R. Brown began, in an oblique way, the practice objected to, and for which there is often a plausible excuse; and the elder De Candolle sometimes followed it. It was only when the practice was systematically carried out by one or two authors that the consequences became apparent; for few genera or species have now their Linnæan limits or signification, and the new rule was practically proved to be a necessity.

Among the recommendations contained in article 36 was the following: "Readily adopt unpublished names found in travellers' notes or in herbaria, unless they be more or less defective." Guided by the practice of the elder De Candolle and his contemporaries, it used to be thought a duty, or at least a part of common courtesy, to do this, in all cases in which the author's approval could fairly be supposed. But certain inconveniences and misunderstandings have resulted, especially as to mode of citation, which have suggested its withdrawal or modification. In this Revision, M. De Candolle only adds the restriction, "or unless the author has not in advance approved the publication." This does not alter the case except for living authors: their approval ought to be obtained or counted on; and in respect to authors no longer living a botanist takes up only such names as in his opinion ought to be published, and which he supposes the posthumous author would have approved. On the whole it were probably better not to take up names left unpublished by a deceased botanist; and De Candolle assigns good reasons for letting them alone. If he had modified the article decidedly in this sense he would have more fully expressed his own view, and probably have been sustained by prevalent opinion. More-

over, he might have distinguished this practice from an essentially different one, namely: —

The case of plants sent under manuscript names by a discoverer or an investigator to some botanist engaged in publication, and with a view to their publication; of which the sending by Nuttall of new *Umbelliferæ* to the elder De Candolle when elaborating that order for the "Prodromus," is a marked and not unusual instance. For this is a practice that need not be discouraged. Any small inconvenience that may arise as to mode of citation is counterbalanced by the greater concentration of publication, new genera and species thus appearing in monographs, floras, or in the papers of leading botanists, which otherwise would have dimly seen the light in obscure or local periodicals. And they are more likely to have proper characters assigned to them, instead of vague descriptions, by incompetent or unpractised hands, such as often try a botanist's patience.

Article 50 treats of the mode of dealing with such names as the above mentioned after they have been published, *i. e.*, "names published from a private document, an herbarium, a non-distributed collection, etc." It declares that such names "are individualized (Fr. *precisés*) by the addition of the name of the author who publishes them, notwithstanding the contrary indication that he may have given." This is found to mean that, although the elder De Candolle gives us "Eulophus, Nutt.," as the name of a genus communicated by Nuttall, with a specimen, for the purpose of its being so published in the fourth volume of the "Prodromus," yet subsequent writers, looking only to the work it was published in, are to cite it as Eulophus, DC. And that the genus which Linnæus published as "Linnæa, authore Clariss. Dr. Gronovio," we are to cite as Linnæa, Linn. This is not only quite contrary to the practice of botanists from Linnæus down to De Candolle and later, but is also contrary to the golden rule of citation, already referred to, never to make an author say something different from or opposed to that which he does say.

Appreciating this, the author of the code has now recast

article 50, so as to read, "When an inedited name has been published (by another botanist), in attributing it to its author, those who afterwards mention it ought to add the name of the person who published it; for example, Leptocaulis, Nutt. in DC.; *Oxalis lineata*, Gillies in Hook."

This is reasonable, and in the first instance such names will almost of necessity be so cited, must always be so cited when work, volume, etc., are specified. But De Candolle remarks that the addition will soon vanish; for instance, "*Cynoglossum ciliatum*, Douglas, Mss.," published by Lehmann in "Pugillus," etc., and in Hooker's "Flora Boreali Americana," will soon come to be quoted simply as "*Cynoglossum ciliatum*, Dougl.," that is, just as other names are quoted. And why not? Because, it is said, the name dating only from the publication, it is necessary to know when and where this vicarious publication was effected. For this "Nutt. in DC." may fairly serve, nearly all names published by De Candolle being contained in the "Prodromus." Not so, however, with "Gillies in Hook." Sir William Hooker published very widely, in periodicals, in the "Botanical Magazine," and in numerous independent works. In such cases the double citation gives little help. The experienced botanist may know where to look; the inexperienced must turn to indexes at once; for both these must be the final and the usual resort; and in them the double has little if any advantage over the single citation. Moreover, if this principle is fully applied, the number of double-cited names may be inconveniently numerous. The first volume of Torrey and Gray's "Flora of North America" abounds in species and genera published by them for Nuttall. If these have all to be permanently quoted "Nutt. in Torr. & Gray," why not also the many species published, say by Bentham in De Candolle's "Prodromus," in the "Flora Brasiliensis," etc., and even the species published by Brown in the second edition of the "Hortus Kewensis," and elsewhere? On the whole it seems probable that these double citations will be used only in first or in early quotations, or in special instances; that it will not be deemed necessary to retain them when the names become settled in Floras or

general works, except in the bibliography or full reference; when of course the "*Leptocaulis inermis*, Nutt. in DC. Coll. Mem. v. 39, x. 10, et Prodr. v. 107," will fully appear. But so long as the abbreviated citation of the author and publisher together is requisite, the mode or citation recommended by De Candolle is the one to be employed.

A quite different case is that of citing, as authority for a genus or species, the name of a botanist which is not upon the record. There is reason to believe that L. C. Richard edited the "Flora Boreali-Americana" of Michaux, and drew up the excellent generic and specific characters of the new plants in it. There is equal reason to believe that he purposely withheld his name. Upon no just principle of citation, therefore, can the name of Richard be quoted, as the younger Richard and Kunth essayed to do. The same holds for the work of Solander in the first edition of Aiton's "Hortus Kewensis." And if it does not hold for the contributions of Brown to the second edition, it is because he claimed them in his lifetime, rather than because they have been collected and republished under his name since his death. Only confusion will come from the admission of hypothetical constructive authorship. The old rule, that what does not appear is no better than non-existent, must apply to all such cases.

In the comments upon article 52, the duty of abbreviating authors' names in the normal way is insisted on, and the bad practice of doing so by leaving out the vowels is deprecated. "Michx." for Michaux, which is partially shortened in this way, was a necessity on account of the ancient botanist Micheli. But "Crn." for Crouan is intolerable. Such a name need not be abbreviated at all. Monosyllabic names should rarely if ever be curtailed. "R. Br." has so long been used for Robert Brown that it may continue to be used, although "Brown" is better. In the other form, it may be counted among the few cases in which initial letters are used instead of the first syllable and first consonant of the second, — cases which should probably be restricted to the "L." for Linnæus, "DC." for De Candolle, "H. B. K."

for Humboldt, Bonpland, and Kunth. We are not sure that De Candolle would favor the latter.

A series of remarks is made upon articles 29–66, taken together, — relating to names which are to be rejected or modified, and those which are to be maintained notwithstanding certain faults. As already mentioned, the tendency among working naturalists is to preserve names in spite of faults; while a few linguistic reformers, such especially as M. Saint-Leger, propose changes which would affect 733 recognized names of species in Europe alone, and ten or twelve thousand in the vegetable kingdom at large, and "this after all the endeavors of botanists for the past half century to establish the law of priority and to have more stability of names. *Sagittaria sagittifolia* and *Psamma arenaria* must be changed, forsooth, because they are pleonasms; all substantive specific names, because a great majority of specific names are adjectives, and many others because they are not sufficiently classical." For instance, Dianthus he would change to Diosanthus, Mentha to Minthe, *Hydrocotyle brevipes* to *H. brevipedata*, Cactus to Cactos, Arum to Aron, and so on. De Candolle adds, that Cicero was not so particular in Latinizing Greek words, as witness "barbarus," "machina," "emporium." As to Pirus, our author insists that even if Pyrus is not Latin, it is the botanical name of the genus as adopted by Linnæus (from Tournefort and from all the herbalists, and it is old enough to be entered as an alternative form in the dictionaries), and so is to be preserved under the law of priority. There is little danger that the reform of Saint-Leger will prevail. There is some danger that the reaction will so stiffen the rule of priority as to forbid the correction of obvious mistakes. See, for instance, the form in which article 60 is now recast by De Candolle: "A generic name should subsist just as it was made, although a purely typographical error may be corrected. The termination of a Latin specific name may be changed to bring it into accordance with its generic name." From this it would seem that a slip of the pen and a mistaken orthography of a man's name may not be corrected. We trust that, when the change would not

sensibly affect the place of a name in an index, such obvious corrections as of Wisteria to Wistaria may prevail. We may assume that the error was typographical; for Dr. Wistar was at the time too well known in Philadelphia for Nuttall to have been ignorant of the orthography of the name. The correction of Balduina into Baldwinia brings it into accordance with the rule that personal names used for genera should be written as near as may be with the original orthography of the person's name. "*Astragalus aboriginorum*" is neither a typographical nor a clerical error. It is a hard rule that forbids us to write "aboriginum," still retaining Richardson's name as authority.

Botanists may take more kindly to the rule when applied to such names as Eleocharis and Aplopappus, in the formation of which the Greek aspirate was neglected. We cannot well suppose this to have been a typographical or clerical oversight on the part of Robert Brown or of Cassini. Perhaps a majority of botanical authors have preserved the original orthography, on the ground that the right of priority, like that of a certain king, is *super grammaticum*, — while the remainder have written Heleocharis and Haplopappus; whence some confusion in the indexes. The requirement to preserve the original form of the generic and specific names and to abide by the Latin of Linnæus and his contemporaries, notwithstanding classical faults, enables us to retain such familiar names as *Ranunculus acris*, *Lathyrus palustris* and *sylvestris* (instead of *R. acer*, *L. paluster* and *silvestris*), and to keep up "laevis" for smooth, — probably to the disgust of classical scholars.

M. De Candolle has a note on Diclytra of Borckhausen, changed into Dielytra to make it conformable to a conjectured meaning, and then into Dicentra that it might agree with the etymology given by Borckhausen himself: he gives it as a case in which an excess of erudition has loaded the genus with three names in place of one; and he concludes, as do we, that it were better to have kept the original orthography, and have treated it as a name which had, through some mistake, failed of meaning. But the name having been changed into

Dicentra on the ground that the right word κέντρον, and not the impossible word κλύτρον, must have been intended by Borckhausen, we think it should now be maintained, although it might have been left in the original form. Moreover, the doctrine that names must not be mended and that sense is unimportant, however good and needful, is so recent that it must not be too rigidly applied to long-standing cases.

This consideration should not be wholly overlooked in the case of old and long-established genera, especially those of numerous species for which some obscure older name has come to light. Since it is impossible to make rules for the infraction of a rule, such cases must be left to sound discretion. In our opinion such discretion would forbid the transference of the name Stylidium from Swartz's genus to Marlea, and the revival of Labillardière's transient first Candollea for Swartz's Stylidium.

The fourth section of article 60, which enjoined the rejection "of names formed by the combination of two languages," is now suppressed. Nothing is put in its place; but let us hope that we shall not be driven to the acceptance of the specific name "acuticarpum" which one of our fellow-botanists has recently perpetrated. Although hybrid names are to be avoided, yet, as De Candolle remarks, they cannot consistently be outlawed by people who accept "centimetre," "decimetre," "bureaucracy," "terminology," and the like, nor by botanists who raise no objection to "ranunculoides," "scirpoides," "linnæoides," "bauhinioides," etc.

Names of identical meaning but of different orthography, as our author insists, may well enough co-exist. In a vast genus it might be neither inconvenient nor harmful to maintain species named respectively "fluviorum," "fluvialis," and "fluviatilis," at least if they belonged to different parts of the world.

We pass to some brief annotations upon the second part of the publication before us, which deals with questions not taken up by the congress of 1867.

The first topic is that of the nomenclature of organs, which was treated with some fullness in the "Phytographie." The

remark is here repeated that the greater part of the so-called names of organs are only terms, that is, names indicative of the condition of organs or parts of the plant. For some of these substantive names are necessary or highly convenient, yet most were better provided with adjective terms only, which belong to terminology, not to nomenclature. Doubtless principles of fixity and the rule of priority should apply to these, both to names and to terms. But it seems unlikely that the phytotomists will at present heed the counsels of the phytographers in this matter. Yet the latter may insist that established names used in descriptive botany shall not be displaced on the pretense of getting more appropriate ones. For instance, the long recognized name "testa" for the outer seed-coat is to be discarded, because, forsooth, this covering is not always or even not generally a shell, or of the texture of earthenware. As well ask the French to discard the word "tête" (or "teste"), because the human head, or the skull which gave the name, does not really resemble a brick or earthen pot.

The second is upon the nomenclature of fossils. And the rule is that they are named according to laws which apply to living plants. The Bologna congress of palæontologists ordained that, to secure priority for specific names of fossils, they should not only be described but figured. De Candolle, after consultation with Heer (whose recent death we have to deplore), concludes that this rule is too absolute. It seems to us that so long as a large part of the names of fossil plants are merely tentative and provisional, we should be content with a general approximation to the received rules of botany.

The nomenclature of groups inferior to species (varieties, sub-varieties, variations and sub-variations) is considered; but no new rules are proposed; nor is the question of sub-species discussed.

Although it is not exactly a matter of nomenclature, we should have liked that our author had considered the two modes of disposing of varieties, and had expressed an opinion as to whether the character of the species should or should not completely cover the variety or varieties assigned to it. In

the former case there is a variety α, followed by β, etc. In the latter, the species is defined upon its type, without any special regard to the appended variety or varieties, which are then characterized as to the points in which they differ from the type. We prefer the latter method, as being on the whole clearer, and as a saving in names; avoiding the awkwardness or the superfluity of a varietal name for the type of the species.

Some noteworthy observations are introduced in respect to the plight which systematic botany is threatened with by what De Candolle would call micromorphic botanists, like Jordan and Gandoger, who abandon the Linnæan idea of species altogether, and give this name and rank to what ordinary botanists take for sub-varieties. For example, we are informed that M. Gandoger divides the Roses of Europe and North America into 4600 species, or groups provided with names similar to those of species, under numerous subgenera, which in effect take the place of genera. Mentha has already undergone a similar micro-metamorphosis. If this goes on, and the names should be written every one, I suppose that even the world itself could not contain the books (or indexes) that should be written. The obvious and only remedy is to relegate this kind of botany to a world of its own, with which the legitimate science need have nothing to do.

Questions having been raised as to the proper use of capital initials in certain specific names, M. De Candolle has devoted two or three pages to this and related topics. Linnæus used capital initials only for substantive names; Lamarck employed them for personal and some geographical names, seemingly without system. A. P. De Candolle used the initial capital systematically for all three, and even for "Alpina" when used to designate a plant of the Alps. His example has generally been followed until recently; and this is in accordance with the custom of the English language. To the objection that it is contrary to the customs of the Latin language, our author replies at some length, substantially as follows. He finds that in the matter of orthography, etc., classical writers distinguish nine phases or periods of the Latin language, of which the most classical is the seventh period, that of Augustus; and

there is no foundation in classical Latin for either punctuation (the points distinguishing words, not phrases) or accentuation by signs, and that the distinction between capitals and small letters was made since the dark ages by scholars whom a purist of our day might tax with ignorance of the proper way of writing Latin; that the object and result of all these and other innovations was greater clearness and precision; that the question is not at all one of ancient latinity, but of modern usages, both of the philologists and the naturalists; and these have happily modified classical Latin into a medium of greater precision and clearness and better adapted to the needs of science.

Finally, we have a brief discussion of the question: "When an author has comprehended one genus in another without naming a species, can he be cited for the names of the species which implicitly result from this union?" The answer he decisively gives is: "This would be neither right nor possible, nor practically convenient. To be correct, one should attribute to an author exactly what he has published. When it is said that the genus B should be united with the genus A, this is not saying that the species of the genus B should be called by such and such names in the genus A. To name them correctly it is necessary to examine them one by one. A glance at the Genera of Bentham and Hooker, or at the works of Baillon, will show how impossible it is to attribute the designation of the species to the authors who have changed the names of the genera, without an explanation under each species." This is illustrated by the supposed case of three genera combined into one, each of which has a species "lanceolata"; by the case of a species "minor" transferred to a genus of which it may be the largest species; and by reference to the state of all large and many small genera, full of obscure, misunderstood, or debatable species, the arrangement and naming of which can be effected only by patient and prolonged study. When this work has not actually been done by the reformers of genera, it should be left to monographers and the editors of Floras. If, by article 45, "a species is not looked upon as named unless it has a generic name as well as a specific one,"

neither is it named unless a specific as well as a generic name is assigned to it. Besides the instances in which the old specific name is impossible under another genus, there are very many in which it would be improper or questionable, and in respect to which particular consideration is required. Between these cases and the plain ones in which implied naming could not go wrong, who is to draw the line? Perhaps it might be drawn at monotypic new genera with old specific names. But how to do even this upon recognized principles is a problem.

A fatal objection to the principle of names by implication is that all such names, if they are existent, must be indexed in the new "Nomenclator Botanicus" now in preparation. To transcribe under Senecio the specific names pertaining to all the genera which Bentham has referred to that already vast genus is no small matter, and a part of the work will prove superfluous if — as we suppose to be the case — some of these genera, such as Cacalia, ought to be maintained. But that is only the beginning. A more recent author, Baillon, has reduced the genera of *Compositæ* nearly one half. For example, to Helenium he has referred Gaillardia, Actinella, Cephalophora, etc.; to Tagetes he has referred Dysodia, Nicolettia, Hymenatherum, and others; to Helianthus, a greater number of genera, most of them prolific in species. In all probability, most of these reductions will not be approved. Yet, if the principle of constructive naming is adopted, the "Nomenclator" must burden its columns with these hosts of inchoate specific names of Baillon, either as received names or as synonyms. It is plain that the principle referred to, besides its incongruity with the leading ideas of received nomenclature, breaks down with its own weight. There are, nevertheless, taking arguments in its favor, which need not here be recapitulated; and the common system has its disadvantages and liability to abuse; yet it appears to be the only workable system. As already intimated, the right assignment of specific names in reconstructed genera requires particular knowledge and careful investigation. And the botanist who reconstructs genera should himself adjust and state the specific names as far as he can.

BALL'S FLORA OF THE PERUVIAN ANDES.

THE personal observations and the collections upon which this essay[1] was founded were made in April, 1882, in an excursion by railway from Lima up to Chicla, which although only seventy-five miles in distance, is at the elevation of 12,220 English feet. Much to his surprise, Mr. Ball found that at this elevation he had not yet reached the alpine region, which really begins about 2000 feet higher. This is three or four thousand feet higher than Grisebach had placed it, on the authority of Tschudi and Humboldt; yet is only what we should expect, since the proper alpine vegetation of the Rocky Mountains in lat. 40° N. hardly descends below 10,000 feet, and the oscillations of temperature in the Peruvian Andes are small.

Equally mistaken, Mr. Ball suspects, must be the common view that the flora of the tropical Andes is scanty in species as compared with high-mountain floras in general. He makes some comparisons from which he infers that the paucity is apparent rather than real, and may be attributed mainly to the paucity of collections in the Andes, since these vast regions have been visited at very few points and far between.

About a quarter of the Andean phænogams is of *Compositæ*, which is double their ratio in North America, which again is greater than that of any other continent. The characteristic and the most abundant Andean *Compositæ* are the *Mutisiaceæ*. Mr. Ball, referring to Bentham's indication of the complex affinities of this group, ventures "to believe that under *Mutisiaceæ* are included very many different lines of descent, but that among them there are some minor groups distinguished by very high relative antiquity." And in another connection he opines "that the arguments that have led some distinguished botanists to consider the great family

[1] *Contributions to the Flora of the Peruvian Andes, with Remarks on the History and Origin of the Andean Flora.* By John Ball; Journal Linnæan Society, xxii. London, 1885. (American Journal of Science and Arts, 3 ser., xxxi. 231.)

of *Compositæ* as of comparatively recent origin to appear to me altogether inconclusive. When I consider the vast variety of forms which it includes, the degree in which some large groups are localized in different regions of the earth, while others, such as Senecio, have representatives in every zone, I shrink from the conclusion that their origin can be, even in geological language, at all recent. It is, of course, not inconceivable that plants which we class together under the name *Compositæ* may have come into existence by different lines of descent through gradual modification from different ancestral types. But when we consider the general agreement in the structure and arrangement of the essential organs, I think that the balance of probability inclines decidedly toward the belief in a community of origin of all the various existing forms. Be that as it may, we are, I think, justified in looking to the mountain region of South America as the original home of many large groups, such as the genus Baccharis, most of the *Mutisiaceæ*, and many genera of other tribes."

As to these two suggestions, although it is practically convenient, and perhaps necessary, to bring all the Labiatiflorous *Compositæ* under one tribe, as Bentham has done, it seems to us altogether probable that the existing forms are descended from different lines of ancestry. Indeed, by such a conception we can more naturally understand their diverse affinities. But as to the great order they belong to, if there is any large group in which the structure suggests community of origin, it is the *Compositæ*. And we suppose that systematic botanists of large experience would entirely agree with Mr. Ball, that the wide differentiation and distribution of this vast order indicated its high antiquity. Our author has assigned some strong reasons for this opinion. The only argument to the contrary that we know of is an ideal one, based upon two suppositions: one, that Dicotyledons culminate in the *Compositæ* and in such-like orders; the other, that the highest ideal type of plants must be of the latest evolution. But, indeed, the vegetable commonwealth shows no tendency to culminate in any one group or set of groups;

and it is a questionable morphology which would promote the capitulum of a Thistle or a Dandelion to the head of the class.

We remember an interesting lecture, in which, recognizing the dominant part which the northern hemisphere and its boreal lands, with their favorable configuration, have undoubtedly played in floral distribution, it was inferred that the rôle of the southern had always been comparatively insignificant. But a great deal may have happened in the austral regions before this boreal supremacy was established. Mr. Ball several years ago brought forward his doctrine of the very high antiquity of our actual temperate and alpine floras, of their coexistence in highly elevated regions of low latitudes even in early times. So now, applying his former conclusions to the southern hemisphere, and to "a period remote even in geological language," he notes that "the special generic types of the antarctic flora" "belong without exception to the great groups or natural orders which are now almost universally diffused throughout the world; and the ancestral types from which they originated were probably carried to that region at a remote period, when the physical conditions of the earth's surface were widely different from those now prevailing." "Various considerations tend to the conclusion that the dispersal of the chief cosmopolitan genera of plants may have coincided with the period of the older secondary rocks; and at that period physical agencies far transcending those of our experience prevailed throughout the earth. If the ancestors of the antarctic types of vegetation were then established in a south polar continental area, and were developed from them by gradual modification, I see no difficulty in believing that they may have maintained themselves through successive gradual changes of physical conditions within the same region, and even that some may still survive within the Antarctic Circle."

Whether or not one accepts the idea of such high geological antiquity which Mr. Ball claims for what he calls Cosmopolitan types, we must wholly agree with him in his use of this name for them, in preference to that of the Scandinavian. The latter term was used by Hooker before the relation of

the present flora of our temperate zone to a former high-northern vegetation was made clear, and before the types in question could "with more reason be referred to North America than to Scandinavia." Mr. Ball's remark that, as to many of them, "the balance of evidence points to an original home in the high mountains of lower latitudes" chimes in with his favorite and original doctrine. And this indeed seems likely to gain ground the more it is considered and applied, as he is applying it, to the explanation of actual distribution.

The interesting problem is to discriminate, as well as may be, the two commingled elements of the northern temperate floras, one of arctic, the other of more endemic mountain origin. An interesting presentation, as concerns central Europe, is made in Heer's "Nival Flora of Switzerland," a posthumous work published by the Société Helvetique des Sciences Naturelles, of which a summary is given in "Nature" for December 31, 1885.

The following idea is extremely suggestive. "In a zoölogical as well as a botanical sense Brazil is one of the most distinct and separate regions of the earth. It is in large part a granitic region, from which vast masses of superincumbent strata have been denuded, and where the granite itself has undergone a great amount of decay and ablation. We there see the ruins of one of the greatest mountain masses of the earth, where a very ancient flora and fauna were developed, of which portions were able to migrate to a distance, while others have been modified to adapt themselves to the gradual changes of the environment. Many vegetable groups, which are but slightly represented in the higher region of the Andes, such, for instance, as the *Melastomaceæ*, probably had their origin in the mountains of ancient Brazil."

We are now only beginning to reach some conception of the rôle which the Andes and their prolongation through Mexico have taken in determining the character of no small part of the North American flora. Following up some ideas which were touched upon in this Journal (vol. xxvii., Nov., 1884, p. 340) and elsewhere, Mr. Ball writes: —

"When we consider that, although subsidence has probably at various times separated the two portions of the continent, the highlands of Mexico and Central America have, in all probability, served during long periods as a bridge over which some portions of the mountain vegetation may have been transferred from north to south, and *vice versa*, we are led to feel surprise rather at the separations now existing than at the presence of many genera and of a few identical species in the flora of the Andes and that of the Rocky Mountains. It is true that I have reckoned as Andean genera and species many that extend northward as far as Mexico; and it may well be that that region, so rich in varied forms of vegetation, is the original home of some that now appear to be more fully developed in the mountain ranges of western North America. Among the widespread American types we must note two natural orders whose original home may with some confidence be placed in the north-western part of the continent. The *Polemoniaceæ*, of which about 140 species belong to that region, are represented in the Andes by five species of Gilia, one of Collonia, and by the endemic genus Cantua. They have sent to the Old World two or three species of Phlox in northern Asia [we believe only one, and that not far over the border], and a single emigrant which has reached Britain, — the Jacob's Ladder of old-fashioned gardens, — which maintains a struggling existence in several isolated spots in Europe. The other specially American family is that of the *Hydrophyllaceæ*, of which 12 genera are known in North America, but which is represented in the Andean chain by only four species of Phacelia." The *Loasaceæ* illustrate the opposite course of migration.

A list of the plants which Mr. Ball collected in the upper valley of the Rimac in the Peruvian Andes, with various annotations and the characters of some new species, concludes the present interesting contribution to Andean Botany. We believe that a second paper upon the subject may be expected. Two or three comments upon individual plants of the list will bring our review to a close.

Erodium cicutarium. — Although Mr. Ball notes the wide diffusion of this Old World species in South America, and that it attends the distribution of cattle, he seems at a loss to account for its presence in the Peruvian Andes at the height of 12,500 feet. He supposes that it has not shown the same readiness to establish itself in North America. This is true of the Atlantic but not of the Pacific side. In California and the adjacent districts the Alfilaria, as it is popularly called, has taken such full possession that we can hardly convince even the botanists that it is an introduced plant. The authors of the "Botany of California" speak of it as "more decidedly and widely at home throughout the interior than any other introduced plant, and, according to much testimony, it was as common throughout California early in the present century as now. . . . It is a valuable and nutritious forage plant, reputed to impart an excellent flavor to milk and butter." At Santa Barbara and other parts of southern California it is used for lawns around dwellings, and it seems to be the only resort. It makes a passable substitute for grass so long as the rainy season lasts or irrigation is kept up. It must have been brought in with the earliest cattle, and have found on the Pacific coast a perfectly suitable climate.

Caldasia of Lagasca, Mr. Ball shows us, must be restored as the name of the genus named Oreomyrrhis by Endlicher.

Relbunium, upon a general survey of the species, will in our opinion be found quite untenable as a genus.

Phacelia circinata, which extends almost from one end to the other of the American continent, is said to be singularly constant, exhibiting no marked varieties. But we have in North America a remarkable diversity of forms, the extremes of which, by themselves, no botanist would refer to one species, although intermediate forms inextricably combine them.

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OF

ASA GRAY

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CHARLES SPRAGUE SARGENT

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ESSAYS.

EUROPEAN HERBARIA.[1]

The vegetable productions of North America, in common with those of most other parts of the world, have generally been first described by European botanists, either from the collections of travelers, or from specimens communicated by residents of the country, who, induced by an enlightened curiosity, the love of flowers, or in some instances by no inconsiderable scientific acquirements, have thus sought to contribute, according to their opportunities, to the promotion of botanical knowledge. From the increase in the number of known plants, it very frequently happens that the brief descriptions, and even the figures, of older authors are found quite insufficient for the satisfactory determination of the particular species they had in view; and hence it becomes necessary to refer to the herbaria where the original specimens were preserved. In this respect, the collections of the early authors possess an importance far exceeding their intrinsic value, since they are seldom large, and the specimens often imperfect.

With the introduction of the Linnæan nomenclature, a rule absolutely essential to the perpetuation of its advantages was also established, namely, that the name under which a genus or species is first published shall be retained, except in certain cases of obvious and paramount necessity. An accurate determination of the Linnæan species is therefore of the first importance; and this, in numerous instances, is only attained with certainty by the inspection of the herbaria of Linnæus and those authors upon whose descriptive phrases or figures

[1] American Journal of Science and Arts, xl. 1. (1841.)

he established many of his species. Our brief notices will therefore naturally commence with the herbarium of the immortal Linnæus, the father of that system of nomenclature to which botany, no less than natural history in general, is so greatly indebted.

This collection, it is well known, after the death of the younger Linnæus, found its way to England, from whence it is not probable that it will ever be removed. The late Sir James Edward Smith, then a young medical student, and a botanist of much promise, was one morning informed by Sir Joseph Banks that the heirs of the younger Linnæus had just offered him the herbarium with the other collections and the library of the father, for the sum of 1000 guineas. Sir Joseph Banks not being disposed to make the purchase, recommended it to Mr. Smith; the latter, it appears, immediately decided to risk the expectation of a moderate independence, and to secure, if possible, these treasures for himself and his country; and before the day closed had actually written to Upsal, desiring a full catalogue of the collection, and offering to become the purchaser at the price fixed, in case it should answer his expectations.[1]

[1] The next day Mr. Smith wrote as follows to his father, informing him of the step he had taken and entreating his assistance: —

"Honored Sir: You may have heard that the young Linnæus is lately dead: his father's collections and library, and his own, are now to be sold; and the whole consists of an immense hortus siccus, with duplicates, insects, shells, corals, materia medica, fossils, a very fine library, all the unpublished manuscripts, in short, everything they were possessed of relating to natural history and physic; the whole has just been offered to Sir Joseph Banks for 1000 guineas, and he has declined buying it. The offer was made to him by my friend Dr. Engelhart, at the desire of a Dr. Acrel of Upsal, who has charge of the collection. Now, I am so ambitious as to wish to possess this treasure, with a view to settle as a physician in London, and read lectures on natural history. Sir Joseph Banks and all my friends to whom I have entrusted my intention, approve of it highly. I have written to Dr. Acrel, to whom Dr. Engelhart has recommended me, for particulars and the refusal, telling him if it was what I expected, I would give him a very good price for it. I hope, my dear sir, you and my good mother will look on this scheme in as favorable a light as my friends here do. There is no time to be lost, for the affair is now talked of in all companies, and a number of people wish to be purchasers. The Empress

His success, as soon appeared, was entirely owing to his promptitude, for other and very pressing applications were almost immediately made for the collection, but the upright Dr. Acrel, having given Mr. Smith the refusal, declined to entertain any other proposals while this negotiation was pending. The purchase was finally made for 900 guineas, excluding the separate herbarium of the younger Linnæus, collected before his father's death, and said to contain nothing that did not exist in the original herbarium; this was assigned to Baron Alstrœmer, in satisfaction of a small debt. The ship which conveyed these treasures to London had scarcely sailed, when the king of Sweden, who had been absent in France, returned home and dispatched, it is said, an armed vessel in pursuit. This story, though mentioned in the "Memoir and Correspondence of Sir J. E. Smith," and generally received, has, we believe, been recently controverted. However, the king and the men of science in Sweden were greatly offended, as indeed they had reason to be, at the conduct of the executors, in allowing these collections to leave the country; but the disgrace should perhaps fall more justly upon the Swedish government itself and the University of Upsal, which derived its reputation almost entirely from the name of Linnæus. It was, however, fortunate for science that they were transferred from such a remote situation to the commercial metropolis of the world, where they are certainly more generally accessible. The late Professor Schultes, in a very amusing journal of a botanical visit to England in the year 1824, laments indeed

of Russia is said to have thoughts of it. The manuscripts, letters, etc., must be invaluable, and there is, no doubt, a complete collection of all the inaugural dissertations which have been published at Upsal, a small part of which has been published under the title of "Amænitates Academicæ," a very celebrated and scarce work. All these dissertations were written by Linnæus, and must be of prodigious value. In short, the more I think of this affair the more sanguine I am, and earnestly hope for your concurrence. I wish I could have one half hour's conversation with you; but that is impossible." (Correspondence of Sir James Edward Smith, edited by Lady Smith, vol. i. p. 93.)

The appeal to his father was not in vain; and, did our limits allow, we should be glad to copy, from the work cited above, the entire correspondence upon this subject.

that they have fallen to the lot of the "toto disjunctos orbe Britannos"; yet a journey even from Landshut to London may perhaps be more readily performed than to Upsal.

After the death of Sir James Edward Smith the herbarium and the other collections, and library of Linnæus, as well as his own, were purchased by the Linnæan Society. The herbarium still occupies the cases which contained it at Upsal, and is scrupulously preserved in its original state, except that, for more effectual protection from the black penetrating dust of London, it is divided into parcels of convenient size, which are closely wrapped in covers of strong paper lined with muslin. The genera and covers are numbered to correspond with a complete manuscript catalogue, and the collection, which is by no means large in comparison with modern herbaria, may be consulted with great facility.

In the negotiation with Smith, Dr. Acrel stated the number of species as 8000, which probably is not too low an estimate. The specimens, which are mostly small, but in excellent preservation, are attached to half-sheets of very ordinary paper, of the foolscap size[1] (which is now considered too small), and those of each genus covered by a double sheet, in the ordinary manner. The names are usually written upon the sheet itself, with a mark or an abbreviation to indicate the source from which the specimen was derived. Thus those from the Upsal garden are marked *H. U.*, those given by Kalm, *K.*, those received from Gronovius, *Gron.*, etc. The labels are all in the handwriting of Linnæus himself, except a few later ones by the son, and occasional notes by Smith, which are readily distinguished, and indeed are usually designated by his initials. By far the greater part of the North American plants which are found in the Linnæan herbarium were received from Kalm, or raised from seeds collected by

[1] Upon this subject Dr. Acrel, giving an account of the Linnæan collections, thus writes to Smith: "Ut vero vir illustrissimus, dum vixit, nihil ad ostentationem habuit, omnia vero sua in usum accommodata; ita etiam in hoc herbario, quod per XL. annos sedulo collegit, frustra quæsiveris papyri insignia ornamenta, margines inauratas, et cet. quæ ostentationis gratia in omnibus fere herbariis nunc vulgaria sunt."

him. Under the patronage of the Swedish government, this enterprising pupil of Linnæus remained three years in this country, traveling throughout New York, New Jersey, Pennsylvania, and Lower Canada; hence his plants are almost exclusively those of the northern States.[1]

Governor Colden, to whom Kalm brought letters of introduction from Linnæus, was then well known as a botanist by his correspondence with Peter Collinson and Gronovius, and also by his account of the plants growing around Coldenham, New York, which was sent to the latter, who transmitted it to Linnæus for publication in the "Acta Upsalensia." At an early period he attempted a direct correspondence with Linnæus, but the ship by which his specimens and notes were sent was plundered by pirates;[2] and in a letter sent by Kalm, on the return of the latter to Sweden, he informs Linnæus that this traveler had been such an industrious collector as to leave him little hopes of being himself farther useful. It is not probable therefore that Linnæus received any plants from Colden, nor does his herbarium afford any such indication.[3]

[1] "Ex his Kalmium, naturæ eximium scrutatorem, itinere suo per Pennsylvaniam, Novum Eboracum, et Canadum, regiones Americæ ad septentrionem vergentes, trium annorum decursu dextre confecto, in patriam inde nuper reducem læti recipimus: ingentem enim ab istis terras reportavit thesaurum non conchyliorum solum, insectorum, et amphibiorum sed herbarum etiam diversi generis ac usus, quas, tam siccas quam vivas, allatis etiam seminibus eorum recentibus et incorruptis, adduxit." (Linn. Amæn. Acad., vol. iii. p. 4.)

[2] See Letter of Linnæus to Haller, September 24, 1746.

[3] The *Holosteum succulentum* of Linnæus (*Alsine foliis ellipticis carnosis* of Colden) is, however, marked in Linnæus's own copy of the "Species Plantarum" with the sign employed to designate the species he at that time possessed; but no corresponding specimen is to be found in his herbarium. This plant has long been a puzzle to American botanists; but it is clear from Colden's description that Dr. Torrey has correctly referred it, in his "Flora of the Northern and Middle States" (1824), to *Stellaria media*, the common Chickweed. Governor Colden's daughter seems fully to have deserved the praise which Collinson, Ellis, and others have bestowed upon her. The latter, in a letter to Linnæus (April, 1758), says: "Mr. Colden of New York has sent Dr. Fothergill a new plant, described by his daughter. It is called Fibraurea, gold-thread. It is a small creep-

From Gronovius, Linnæus had received a very small number of Clayton's plants, previous to the publication of the "Species Plantarum"; but most of the species of the "Flora Virginica" were adopted or referred to other plants on the authority of the descriptions alone.

Linnæus had another American correspondent in Dr. John Mitchell,[1] who lived several years in Virginia, where he collected extensively; but the ship in which he returned to England having been taken by pirates, his own collections, as well as those of Governor Colden, were mostly destroyed. Linnæus, however, had previously received a few specimens, as, for instance, those on which Proserpinaca, Polypremum, Galax, and some other genera were founded.

There were two other American botanists of this period, from whom Linnæus derived either directly or indirectly much information respecting the plants of this country, namely, John Bartram and Dr. Alexander Garden of Charleston, South Carolina.

ing plant, growing on bogs; the roots are used in a decoction by the country people for sore mouths and sore throats. The root and leaves are very bitter, etc. I shall send you the characters as near as I can translate them." Then follows Miss Colden's detailed generic character, prepared in a manner which would not be discreditable to a botanist of the present day. It is a pity that Linnæus did not adopt the genus, with Miss Colden's name, which is better than Salisbury's Coptis. "This young lady merits your esteem and does honor to your method. She has drawn and described four hundred plants in your system: she uses only English terms. Her father has a plant called after him Coldenia; suppose you should call this (alluding to a new genus of which he added the characters) Coldenella, or any other name which might distinguish her among your genera." (Ellis, Letter to Linnæus.)

[1] To him the pretty *Mitchella repens* was dedicated. Dr. Mitchell had sent to Collinson, perhaps as early as the year 1740, a paper in which thirty new genera of Virginian plants were proposed. This Collinson sent to Trew at Nuremberg, who published it in the "Ephemerides Acad. Naturae Curiosorum" for 1748; but in the mean time most of the genera had been already published, with other names, by Linnæus or Gronovius. Among Mitchell's new genera was one which he called Chamædaphne: this Linnæus referred to Lonicera, but the elder (Bernard) Jussieu, in a letter dated February 19, 1751, having shown him that it was very distinct both from Lonicera and Linnæa, and in fact belonged to a different natural order, he afterwards named it Mitchella.

The former collected seeds and living plants for Peter Collinson during more than twenty years, and even at that early day extended his laborious researches from the frontiers of Canada to southern Florida and the Mississippi. All his collections were sent to his patron Collinson,[1] until the death

[1] Mr. Collinson kept up a correspondence with all the lovers of plants in this country, among whom were Governor Colden, Bartram, Mitchell, Clayton, and Dr. Garden, by whose means he procured the introduction of great numbers of North American plants into the English gardens. "Your system," he writes Linnæus, "I can tell you, obtains much in America. Mr. Clayton and Dr. Colden at Albany, on Hudson's River, in New York, are complete professors, as is Dr. Mitchell at Urbana, on Rappahannock River in Virginia. It is he that has made many and great discoveries in the vegetable world." "I am glad you have the correspondence of Dr. Colden and Mr. Bartram. They are both very indefatigable, ingenious men. Your system is much admired in North America." Again, "I have but lately heard from Mr. Colden. He is well, but what is marvellous, his daughter is perhaps the first lady that has so perfectly studied your system. She deserves to be celebrated." "In the second volume of 'Edinburg Essays' is published a Latin botanic dissertation by Miss Colden ; perhaps the only lady that makes a profession of the Linnæan system, of which you may be proud." From all this, botany appears to have flourished in the North American colonies. But Dr. Garden about this time writes thus to his friend Ellis : "Ever since I have been in Carolina, I have never been able to set my eye upon one who had barely a regard for botany. Indeed, I have often wondered how there should be one place abounding with so many marks of the divine wisdom and power, and not one rational eye to contemplate them ; or that there should be a country abounding with almost every sort of plant, and almost every species of the animal kind, and yet that it should not have pleased God to raise up one botanist. Strange, indeed, that the creature should be so rare ! " But to return to Collinson, the most amusing portion of whose correspondence consists of his letters to Linnæus shortly after the publication of the "Species Plantarum," in which (with all kindness and sincerity) he reproves the great Swedish naturalist for his innovations, employing the same arguments which a strenuous Linnæan might be supposed to advance against a botanist of these latter days. "I have had the pleasure," Collinson writes, "of reading your 'Species Plantarum,' a very useful and laborious work. But, my dear friend, we that admire you are much concerned that you should perplex the delightful science of botany with changing names that have been well received, and adding new names quite unknown to us. Thus, botany, which was a pleasant study and attainable by most men, is now become, by alterations and new names, the study of a man's life, and

of that amiable and simple-hearted man, in 1768; and by him many seeds, living plants, and interesting observations were communicated to Linnæus, but few, if any, dried specimens. Dr. Garden, who was a native of Scotland, resided in Charleston, South Carolina, from about 1745 to the commencement of the American Revolution, devoting all the time he could redeem from an extensive medical practice to the zealous pursuit of botany and zoölogy. His chief correspondent was Ellis at London, but through Ellis he commenced a correspondence with Linnæus, and to both he sent manuscript descriptions of new plants and animals with many excellent critical observations. None of his specimens addressed to the latter reached their destination, the ships by which they were sent having been intercepted by French cruisers; and Linnæus complained that he was often unable to make out many of Dr. Garden's genera for want of the plants themselves. Ellis was sometimes more fortunate, but as he seems usually to have contented himself with the transmission of the descriptions alone, we find no authentic specimens from Garden in the Linnæan herbarium.

We have now probably mentioned all the North American correspondents of Linnæus; for Dr. Kuhn, who appears only to have brought him living specimens of the plant which bears his name, and Catesby, who shortly before his death sent a few living plants which his friend Lawson had collected in Carolina, can scarcely be reckoned among the number.[1]

none now but real professors can pretend to attain it. As I love you I tell you our sentiments." (Letter of April 20, 1754.) "You have begun by your 'Species Plantarum'; but if you will be forever making new names, and altering good and old ones, for such hard names that convey no idea of the plant, it will be impossible to attain to a perfect knowledge in the science of botany." (Letter of April 10, 1755: from Smith's Selection of the Correspondence of Linnæus, etc.)

[1] In a letter to Haller, dated Leyden, January 23, 1738, Linnæus writes: "You would scarcely believe how many of the vegetable productions of Virginia are the same as our European ones. There are Alps in the country of New York, for the snow remains all summer long on the mountains there. I am now giving instructions to a medical student here, who is a native of that country, and will return thither in the course of a year, that he may visit those mountains, and let me know whether

The Linnæan Society also possesses the proper herbarium of its founder and first president, Sir James E. Smith, which is a beautiful collection and in perfect preservation. The specimens are attached to fine and strong paper, after the method now common in England. In North American botany, the chief contributors are Menzies, for the plants of California and the Northwest coast; and Muhlenberg, Bigelow, Torrey, and Boott, for those of the United States. Here also we find the Cryptogamic collections of Acharius, containing the authentic specimens described in his works on the Lichens, and the magnificent East Indian herbarium of Wallich, presented some years since by the East India Company.

The collections preserved in the British Museum are scarcely inferior in importance to the Linnæan herbarium itself, in aiding the determination of the species of Linnæus and other early authors. Here we meet with the authentic herbarium of the "Hortus Cliffortianus," one of the earliest works of Linnæus, which comprises some plants which are not to be found in his own proper herbarium. Here also is the herbarium of Plunkenet, which consists of a great number of small specimens crowded, without apparent order, upon the pages of a dozen large folio volumes. With due attention, the originals of many figures in the "Almagestum" and "Amaltheum Botanicum," etc., may be recognized, and many Linnæan species thereby authenticated. The herbarium of Sloane, also, is not without interest to the North American botanist, since many plants described in the "Voyage to Jamaica," etc., and the "Catalogue of the Plants of Jamaica," were united by Linnæus, in almost every instance incorrectly, with species peculiar to the United States and Canada. But still more important is the herbarium of Clayton, from whose notes and specimens Gronovius edited the "Flora Virginica."[1] Many Linnæan species are founded on the plants

the same alpine plants are found there as in Europe." Who can this American student have been? Kuhn did not visit Linnæus until more than fifteen years after the date of this letter.

[1] "Flora Virginica, exhibens plantas quas J. Clayton in Virgini col-

here described for which this herbarium is alone authentic; for Linnæus, as we have already remarked, possessed very few of Clayton's plants. The collection is nearly complete, but the specimens were not well prepared, and are not therefore always in perfect preservation. A collection of Catesby's plants exists also in the British Museum, but probably the larger portion remains at Oxford. There is besides, among the separate collections, a small but very interesting parcel selected by the elder Bartram, from his collection made in Georgia and Florida almost a century ago, and presented to Queen Charlotte, with a letter of touching simplicity. At the time this fasciculus was prepared, nearly all the plants it comprised were undescribed, and many were of entirely new genera; several, indeed, have only been published very recently, and a few are not yet recorded as natives of North America. Among the latter we may mention *Petiveria alliacea* and *Ximinea Americana*, which last has again recently been collected in the same region. This small parcel contains the Elliottia, Muhl., Polypteris, Nutt., Baldwinia, Nutt., Macranthera, Torr., Glottidium, Mayaca, Chaptalia, Befaria, *Eriogonum tomentosum*, *Polygonum polygamum*, Vent., *Gardoquia Hookeri*, Benth., *Satureia* (*Pycnothymus*) *rigida*, Cliftonia, *Hypericum aureum*, *Galactia Elliottii*, *Krameria lanceolata*, Torr., *Waldsteinia* (*Comaropsis*) *lobata*, Torr. & Gr., the *Dolichos? multiflorus*, Torr. & Gr., the Chapmannia, Torr. & Gr., *Psoralea Lupinellus*, and others of almost equal interest or rarity, which it is much to be regretted were not long ago made known from Bartram's discoveries.

The herbarium of Sir Joseph Banks, now in the British Museum, is probably the oldest one prepared in the manner commonly adopted in England, of which, therefore, it may serve as a specimen. The plants are glued fast to half-sheets of very thick and firm white paper of excellent quality (similar to that employed for merchants' ledgers, etc.), all care-

legit." Ludg. Bat. 8vo, 1743. Ed. 2, 4to, 1762. The first edition is cited in the "Species Plantarum" of Linnæus; the second, again, quotes the specific phrases of Linnæus.

fully cut to the same size, which is usually $16\frac{1}{2}$ inches by $10\frac{3}{4}$, and the name of the species is written on the lower right-hand corner. All the species of a genus if they are few in number, or any convenient subdivision of a larger genus, are enclosed in a whole sheet of the same quality, and labelled at the lower left-hand corner. These parcels, properly arranged, are preserved in cases or closets, with folding-doors made to shut as closely as possible, being laid horizontally into compartments just wide enough to receive them, and of any convenient depth. In the Banksian herbarium, the shelves are also made to draw out like a case of drawers. This method is unrivalled for elegance, and the facility with which the specimens may be found and inspected, which to a working botanist with a large collection is a matter of the greatest consequence. The only objection is the expense, which becomes very considerable when paper worth at least ten dollars per ream is employed for the purpose, which is the case with the principal herbaria in England; but a cheaper paper, if it be only sufficiently thick and firm, will answer nearly as well. The Banksian herbarium contains authentic specimens of nearly all the plants of Aiton's "Hortus Kewensis," in which many North American species were early established. It is hardly proper indeed, that either the elder or younger Aiton should be quoted for these species, since the first edition was prepared by Solander, and the second revised by Dryander, as to vol. 1 and 2, and the remainder by Mr. Brown. Many American plants from the Physic garden at Chelsea, named by Miller, are here preserved, as also from the gardens of Collinson, Dr. Fothergill (who was Bartram's correspondent after Collinson's death), Dr. Pitcairne, etc. There are likewise many contributions of indigenous plants of the United States, from Bartram, Dr. Mitchell, Dr. Garden, Fraser, Marshall, and other early cultivators of botany in this country. The herbarium also comprises many plants from Labrador and Newfoundland, a portion of which were collected by Sir Joseph Banks himself; and in the plants of the northern and arctic regions it is enriched by the collections of Barry, Ross, and Dr. Richardson. Two sets of

plants collected by the venerable Menzies in Vancouver's voyage, are preserved at the British Museum, the one incorporated with the Banksian herbarium, the other forming a separate collection. Those of this country are of the Northwest Coast, the mouth of the Oregon River, and from California. Many of Pursh's species were described from specimens preserved in this herbarium, especially the Oregon plants of Menzies and those of Bartram and others from the more southern United States, which Pursh had never visited, although he often adds the mark *v. v.* (*vidi vivam*) to species which are only to be met with south of Virginia.

The herbarium of Walter still remains in the possession of the Fraser family, and in the same condition as when consulted by Pursh. It is a small collection, occupying a single large volume. The specimens, which are commonly mere fragments, often serve to identify the species of the "Flora Caroliniana," although they are not always labelled in accordance with that work.

The collections of Pursh, which served as the basis of his "Flora Americæ Septentrionalis," are in the possession of Mr. Lambert, and form a part of his immense herbarium. These, with a few specimens brought by Lewis and Clark from Oregon and the Rocky Mountains, a set of Nuttall's collections on the Missouri, and also of Bradbury's so far as they are extant, with a small number from Fraser, Lyon, etc., compose the most important portion of this herbarium, so far as North American botany is concerned. There is also a small Canadian collection made by Pursh subsequently to the publication of his Flora, a considerable number of Menzies's plants, and other minor contributions. To the general botanist, probably the fine herbarium of Pallas, and the splendid collection of Ruiz and Pavon (both acquired by Mr. Lambert at a great expense) are of the highest interest; and they are by no means unimportant in their relations to North American botany, since the former comprises several species from the Northwest Coast and numerous allied Siberian forms, while our California plants require in some instances to be compared with the Chilian and Peruvian plants of the latter.

Besides the herbaria already mentioned, there are two others in London of more recent formation, which possess the highest interest as well to the general as to the American botanist, namely that of Professor Lindley, and of Mr. Bentham. Both comprise very complete sets of the plants collected by Douglas in Oregon, California, and the Rocky Mountains, as well as those raised from seeds or bulbs, which he transmitted to England, of which a large portion have from time to time been published by these authors. Mr. Bentham's herbarium is probably the richest and most authentic collection in the world for *Labiatæ*, and is perhaps nearly unrivalled for *Leguminosæ*, *Scrophularineæ*, and the other tribes to which he has devoted especial attention; it is also particularly full and authentic in European plants. Professor Lindley's herbarium, which is very complete in every department, is wholly unrivalled in Orchidaceous plants. The genus-covers are made of strong and smooth hardware paper, the names being written on a slip of white paper pasted on the lower corner. This is an excellent plan, as covers of white paper, in the herbarium of an active botanist, are apt to be soiled by frequent use. The paper employed by Dr. Lindley is $8\frac{1}{2}$ inches in length and $11\frac{1}{2}$ inches wide, which, as he has himself remarked, is rather larger than is necessary, and much too expensive for general use.

The herbarium of Sir William J. Hooker, at Glasgow, is not only the largest and most valuable collection in the world, in the possession of a private individual, but it also comprises the richest collection of North American plants in Europe. Here we find nearly complete sets of the plants collected in the arctic voyages of discovery, the overland journeys of Franklin to the polar sea, the collections of Drummond and Douglas in the Rocky Mountains, Oregon, and California, as well as those of Professor Scouler, Mr. Tolmie, Dr. Gairdner, and numerous officers of the Hudson's Bay Company, from almost every part of the vast territory embraced in their operations from one side of the continent to the other. By an active and prolonged correspondence with nearly all the botanists and lovers of plants in the United States and Canada,

as well as by the collections of travelers, this herbarium is rendered unusually rich in the botany of this country; while Drummond's Texan collections, and many contributions from Mr. Nuttall and others, very fully represent the flora of our southern and western confines. That these valuable materials have not been buried, or suffered to accumulate to no purpose or advantage to science, the pages of the "Flora Boreali-Americana," the "Botanical Magazine," the "Botanical Miscellany," the "Journal of Botany," the "Icones Plantarum," and other works of this industrious botanist, abundantly testify; and no single herbarium will afford the student of North American botany such extensive aid as that of Sir William Hooker.

The herbarium of Dr. Arnott of Arlary, although more especially rich and authentic in East Indian plants, is also interesting to the North American botanist, as well for the plants of the "Botany of Captain Beechy's Voyage," etc., published by Hooker and himself, as the collections of Drummond and others, all of which have been carefully studied by this sagacious botanist.

The most important botanical collection in Paris, and, indeed, perhaps the largest in the world, is that of the Royal Museum at the Jardin des Plantes or Jardin du Roi. We cannot now devote even a passing notice to the garden and magnificent new conservatories of this noble institution, much less to the menagerie and celebrated museum of zoölogy and anatomy, of the cabinet of mineralogy, geology, and fossil remains, which, newly arranged in a building recently erected for its reception, has just been thrown open to the public. The botanical collections occupy a portion of this new building. A large room on the first floor, handsomely fitted up with glass cases, contains the cabinet of fruits, seeds, sections of stems, and curious examples of vegetable structure from every part of the known world. Among them we find an interesting suite of specimens of the wood, and another comprising the fruits, or nuts, of nearly all the trees of this country; both collected and prepared by the younger Michaux. The herbaria now occupy a large room or hall, immediately

over the former, perhaps eighty feet long and thirty wide above the galleries, and very conveniently lighted from the roof. Beneath the galleries are four or five small rooms on each side, lighted from the exterior, used as cabinets for study and for separate herbaria, and above them the same number of smaller rooms or closets, occupied by duplicate or unarranged collections. The cases which contain the herbaria occupy the walls of the large hall and of the side rooms. Their plan may serve as a specimen of that generally adopted in France. The shelves are divided into compartments in the usual manner: but instead of doors the cabinet is closed by a curtain of thick and coarse brown linen, kept extended by a heavy bar attached to the bottom, which is counterpoised by concealed weights, and the curtain is raised or dropped by a pulley. Paper of very ordinary quality is generally used, and the specimens are attached, either to half sheets or to double sheets, by slips of gummed paper, or by pins, or sometimes the specimen itself is glued to the paper. Genera or other divisions are separated by interposed sheets, having the name written on a projecting slip.

According to the excellent plan adopted in the arrangement of these collections, which is due to Desfontaines, three kinds of herbaria have been instituted, namely: 1. The general herbarium. 2. The herbaria of particular works or celebrated authors, which are kept distinct, the duplicates alone being distributed in the general collection. 3. Separate herbaria of different countries, which are composed of the duplicates taken from the general herbarium. To these, new accessions from different countries are added, which from time to time are assorted and examined, and those required for the general herbarium are removed to that collection. The ancient herbarium of Vaillant forms the basis of the general collection; the specimens, which are all labelled by his own hand, are in excellent preservation, and among them plants derived from Cornuti or Dr. Surrasin may occasionally be met with. This collection, augmented to many times its original extent, by the plants of Commerson, Dombey, Poiteau, Leschenault, etc., and by the duplicates from the special herbaria, probably contains at this

time thirty or forty thousand species. Of the separate herbaria, the most interesting to us is that made in this country by the elder Michaux, from whose specimens and notes the learned Richard prepared the "Flora Boreali-Americana."

Michaux himself, although an excellent and industrious collector and observer, was by no means qualified for authorship; and it is to L. C. Richard that the sagacious observations, and the elegant, terse, and highly characteristic specific phrases of this work are entirely due. There is also the very complete Newfoundland collection of La Pylaie, comprising about 300 species, and a set of Berlandier's Texan and Mexican plants, as well as numerous herbaria less directly connected with North American botany, which we have not room to enumerate. Here, however, we do not find the herbaria of several authors which we should have expected. That of Lamarck, for instance, is in the possession of Professor Rœper at Rostock, on the shores of the Baltic; that of Poiret belongs to Moquin-Tandon of Toulouse; that of Bosc, to Professor Moretti of Pavia; and the proper herbarium of the late Desfontaines, which however still remains at Paris, now forms a part of the very large and valuable collection of Mr. Webb. The herbarium of Mr. Webb, although of recent establishment, is only second to that of Baron Delessert; the two being far the largest private collections in France, and comprising not only many older herbaria, but also, as far as possible, full sets of the plants of recent collectors. The former contains many of Michaux's plants (derived from the herbarium of Desfontaines), a North American collection, sent by Nuttall to the late Mr. Mercier of Geneva, a full set of Drummond's collections in the United States and Texas, etc. The latter also comprises many plants of Michaux, derived from Ventenat's herbarium, complete sets of Drummond's collections, etc. But a more important, because original and perhaps complete, set of the plants of Michaux is found in the herbarium of the late Richard, now in possession of his son, Professor Achille Richard, which even contains a few species which do not exist in the herbarium at the Royal Museum. The herbarium of the celebrated Jussieu, a fine collection, which is scrupu-

lously preserved in its original state, by his worthy son and successor, Professor Adrien Jussieu, comprises many North American plants of the older collectors, of which several are authentic for species of Lamarck, Poiret, Cassini, etc.

The herbarium of De Candolle at Geneva, accumulated throughout the long and active career of this justly celebrated botanist, and enriched by a great number of correspondents, is surpassed by few others in size, and by none in importance. In order that it may remain as authentic as possible for his published works, especially the "Prodromus," no subsequent accessions to families already published are admitted into the general herbarium, but these are arranged in a separate collection. The proper herbarium, therefore, accurately exhibits the materials employed in the preparation of the "Prodromus," at least so far as these were in Professor De Candolle's own possession. As almost twenty years have elapsed since the commencement of this herculean undertaking, the authentic herbarium is of course much less rich in the earlier than in the later orders. The *Compositæ*, to which seven years of unremitted labor have been devoted, form themselves an herbarium of no inconsiderable size. It is unnecessary to enumerate the contributors to this collection (which indeed would form an extended list), since the author, at least in later volumes of the "Prodromus," carefully indicates, as fully as the work permits, the sources whence his materials have been derived. The paper employed is of an ordinary kind, somewhat smaller than the English size, perhaps fifteen inches by ten; and the specimens are attached to half sheets by loops or slips of paper fastened by pins so that they may readily be detached, if necessary, for particular examination. Several specimens from different sources or localities, or exhibiting the different varieties of a species, are retained when practicable; and each species has a separate cover, with a label affixed to the corner, containing the name and a reference to the volume and page of the "Prodromus" where it is described. The limits of genera, sections, tribes, etc., are marked by interposed sheets, with the name written on projecting slips. The parcels which occupy each compartment of the

well-filled shelves are protected by pieces of binder's board, and secured by a cord, which is the more necessary as the doors are not closed by doors or curtains.

The royal Bavarian herbarium at Munich is chiefly valuable for its Brazilian plants, with which it has been enriched by the laborious and learned Martius. The North American botanist will, however, be interested in the herbarium of Schreber, which is here preserved and comprises the authentic specimens described or figured in his work on the grasses, the American species mostly communicated by Muhlenberg. The *Graminæ* of this and the general herbarium have been revised by Nees von Esenbeck, and still later by Trinius. It was here that the latter, who for many years had devoted himself to the exclusive study of this tribe of plants, and had nearly finished the examination of the chief herbaria of the continent, preparatory to the publication of a new Agrostographia, was suddenly struck with a paralysis, which has probably brought his scientific labors to a close.

The imperial herbarium at Vienna, under the superintendence of the accomplished Endlicher, assisted by Dr. Fenzl, is rapidly becoming one of the most valuable and extensive collections in Europe. The various herbaria of which it is composed have recently been incorporated into one, which is prepared nearly after the English method. It, however, possesses few North American plants, except a collection made by Enslin (a collector sent to this country by Prince Lichtenstein, from whom Pursh obtained many specimens from the southern States), and some recent contributions by Hooker, etc. There is also an imperfect set of plants collected by Hænke (a portion of which are from Oregon and California), so far as they are yet published in the "Reliquæ Hænkeanæ" of Presl, in whose custody, as curator of the Bohemian museum at Prague, the original collection remains.

The herbarium of the late Professor Sprengle still remains in the possession of his son, Dr. Anthony Sprengle, at Halle, but is offered for sale. It comprises many North American plants communicated by Muhlenberg and Torrey. The herbarium of Schkuhr was bequeathed to the university of Wit-

tenberg, and at the union of this university with that of Halle was transferred to the latter, where it remains under the care of Professor von Schlechtendal. It contains a large portion of the Carices described and figured in Schkuhr's work, and is therefore interesting to the lovers of that large and difficult genus. The American specimens were mostly derived from Willdenow, who obtained the greater portion from Muhlenberg.

The royal Prussian herbarium is deposited at Schöneberg (a little village in the environs of Berlin), opposite the royal botanic garden, and in the garden of the Horticultural Society. It occupies a very convenient building erected for its reception, and is under the superintendence of Dr. Klotzsch, a very zealous and promising botanist. It comprises three separate herbaria, namely, the general herbarium, the herbarium of Willdenow, and the Brazilian herbarium of Sello. The principal contributions of the plants of this country to the general herbarium, garden specimens excepted, consist of the collections of the late Mr. Beyrich, who died in western Arkansas, while accompanying Colonel Dodge's dragoon expedition, and a collection of the plants of Missouri and Arkansas by Dr. Engelmann, now of St. Louis; to which a fine selection of North American plants, recently presented by Sir William Hooker, has been added. The botanical collections made by Chamisso, who accompanied Romanzoff in his voyage round the world, also enrich this herbarium; many are from the coast of Russian America and from California; and they have mostly been published conjointly by the late Von Chamisso and Professor Schlechtendal in the "Linnæa" edited by the latter.

The late Professor Willdenow enjoyed for many years the correspondence of Muhlenberg, from whom he received the greater part of his North American specimens, a considerable portion of which are authentic for the North American plants of his edition of the "Species Plantarum." In addition to these we find in his herbarium many of Michaux's plants communicated by Desfontaines, several from the German collector Kinn, and perhaps all the American species described by

Willdenow from the Berlin garden. It also comprises a portion of the herbarium of Pallas, the Siberian plants of Stephen, and a tolerable set of Humboldt's plants. This herbarium is in good preservation, and is kept in perfect order and extreme neatness. As left by Willdenow, the specimens were loose in the covers, into which additional specimens had sometimes been thrown and the labels often mixed, so that much caution is requisite to ascertain which are really authentic for the Willdenovian species. To prevent farther sources of error, and to secure the collection from injury, it was carefully revised by Professor Schlechtendal while under his management, and the specimens attached by slips of paper to single sheets, and all those that Willdenow had left under one cover, as the same species, are enclosed in a double sheet of neat blue paper. These covers are numbered continuously throughout the herbarium, and the individual sheets or specimens in each are also numbered, so that any plant may be referred to by quoting the number of the cover and that of the sheet to which it is attached. The arrangement of the herbarium is unchanged, and it precisely accords with this author's edition of the "Species Plantarum." Like the general herbarium, it is kept in neat portfolios, the back of which consists of three pieces of broad tape, which, passing through slits near each edge of the covers, are tied in front: by this arrangement their thickness may be varied at pleasure, which, though of no consequence in a stationary herbarium, is a great convenience in a growing collection. The portfolios are placed vertically on shelves protected by glass doors, and the contents of each are marked on a slip of paper fastened to the back. The herbaria occupy a suite of small rooms distinct from the working rooms, which are kept perfectly free from dust.

Another important herbarium at Berlin is that of Professor Kunth, which is scarcely inferior in extent to the royal collection at Schöneberg, but it is not rich or authentic in the plants of this country. It comprises the most extensive and authentic set of Humboldt's plants, and a considerable number of Michaux's which were received from the younger Richard. As the new "Enumeratio Plantarum" of this industrious

botanist proceeds, this herbarium will become still more important.

For a detailed account of the Russian botanical collections and collectors, we may refer to a historical sketch of the progress of botany in Russia, etc., by Mr. Bongard, the superintendent of the Imperial Academy's herbarium at St. Petersburg, published in the "Recueil des Actes" of this institution for 1834. An English translation of this memoir is published in the first volume of Hooker's "Companion to the Botanical Magazine."

NOTES OF A BOTANICAL EXCURSION TO THE MOUNTAINS OF NORTH CAROLINA.[1]

The peculiar interest you[2] have long taken in North American botany, and your most important labors in its elucidation, indicate the propriety of addressing to yourself the following remarks, relating, for the most part, to the hasty collections made by Mr. John Carey, Mr. James Constable, and myself, in a recent excursion to the higher mountains of North Carolina. Before entering upon our own itinerary, it may be well to notice very briefly the travels of those who have preceded us in these comparatively unfrequented regions. The history of the botany of the Alleghany Mountains would be at once interesting and on many accounts useful to the cultivators of our science in this country; but with my present inadequate means, I can only offer a slight contribution towards that object.

So far as I can ascertain, the younger (William) Bartram was the first botanist who visited the southern portion of the Alleghany Mountains. Under the auspices of Dr. Fothergill, to whom his collections were principally sent, and with whom his then surviving father had previously corresponded, Mr. Bartram left Philadelphia in 1773, and after traveling in Florida and the lower part of Georgia for three years, he made a transient visit to the Cherokee country, in the spring of 1776. In this journey he ascended the Seneca or Keowee River, one of the principal sources of the Savannah, and crossing the mountains which divide its waters from those of the Tennessee, continued his travels along the course of the latter to the borders of the present State of Tennessee. Finding that his researches could not safely be extended in

[1] American Journal of Science and Arts, xlii. 1. (1842.)

[2] Sir W. J. Hooker.

this direction, after exploring some of the higher mountains in the neighborhood, he retraced his steps to the Savannah River, proceeding thence through Georgia and Alabama to Mobile. His well-known and very interesting volume of travels[1] contains numerous observations upon the botany of these regions, with occasional popular descriptions, and in a few cases Latin characters of some remarkable plants; as, for example, the *Rhododendron punctatum* (which he calls *R. ferrugineum*), *Stuartia pentagyna* (under the name of *S. montana*), *Azalea calendulacea* (which he terms *A. flammea*), Trautvetteria, which he took for a new species of Hydrastris, *Magnolia auriculata*, etc. He also notices the remarkable intermixture of the vegetation of the north and south, which occurs in this portion of the mountains where Halesia, Styrax, Stuartia, and Gelseminum (although the latter "is killed by a very slight frost in the open air in Pennsylvania") are seen flourishing by the side of Birches, Maples, and Firs of Canada.

I should next mention the name of André Michaux, who at an early period, amid difficulties and privations of which few can now form an adequate conception, explored our country from Hudson's Bay to Florida, and westward to the Mississippi, more extensively than any subsequent botanist. A few of his plants have not yet been rediscovered, and a considerable number remain among the rarest and least known species of the United States; it may therefore be useful to give a particular account of his peregrinations, especially through the mountain region which he so diligently explored, and in which he made such important discoveries. For this purpose I am fortunately supplied with sufficient materials, having had the opportunity of consulting the original journals of Michaux, presented by his son to the American Philosophical Society. I am indebted for this privilege to the kindness of John Vaughn, Esq., the secretary of the society, who directed my attention to these manuscripts, and permitted me

[1] "Travels through North and South Carolina, Georgia, East and West Florida, the Cherokee Country," etc. By William Bartram. Philadelphia, 1791.

to extract freely whatever I deemed useful and interesting. The first fasciculus of the diary is wanting; but we learn from a chance record, as well as from published sources,[1] that he embarked at L'Orient on the 29th of September, 1785, and arrived in New York on the 13th of November. The private journal from which the following information is derived commences in April, 1787; prior to which date he had established two gardens, or nurseries, to receive his collections of living plants until they could be conveniently transported to France: one in New Jersey, near the city of New York; the other about ten miles from Charleston, South Carolina. Into the latter it appears he introduced some exotic trees, which he thought suitable to the climate; and the younger Michaux, who visited this garden several years afterwards, mentions two Ginkgos (*Salisburia adiantifolia*), which in seven years had attained an elevation of thirty feet; also some fine specimens of *Sterculia platanifolia*, and a large number of young plants of *Mimosa Julibrissin*, propagated from a tree which his father had brought from Europe. From this stock, probably, the latter has been disseminated throughout the southern States, and is beginning to be naturalized in many places.

I have no means of ascertaining what portions of the country Michaux had visited previously to April, 1787, when he set out from Charleston on his first journey to the Alleghany Mountains, by way of Savannah, ascending the river of that name to its sources in the Cherokee country, and following very nearly the route taken by Bartram eleven years before.[2] He reached the sources of the Keowee River on the

[1] See Michaux, "Flora Boreali-Americana"; Introduction. See also "A Sketch of the Progress of Botany in Western America," by Dr. Short, in the "Transylvania Journal of Medicine," No. 35; and in Hooker's "Journal of Botany" for November, 1840. I am informed that an interesting notice of Michaux is contained in the 8th volume of the "Dictionnaire Encyclopedique de Botanique" (under the head of *Voyageurs*); a work which unfortunately I am not able at this moment to consult.

[2] In this journey he was accompanied by his son, who shortly afterwards returned to Europe. Before they reached Augusta, their horses

14th of June, and was conducted by the Indians across the mountains to the head of the Tugaloo (the other principal branch of the Savannah), and thence to the waters of the Tennessee. After suffering much inconvenience from unfavorable weather and the want of food, he returned to the Indian village of Seneca by way of Cane Creek, descended along the Savannah to Augusta, and arrived at Charleston on the 1st of July. His notes in this as well as in subsequent journeys to the mountains often contain remarks upon the more interesting plants he discovered; and in some cases their localities are so carefully specified that they might still be sought with confidence. On the 16th of July he embarked for Philadelphia, which he reached on the 27th; and, after visiting Mr. Bartram, traveled to New York, arriving at the garden he had established in New Jersey about the 1st of August. Returning by water to Charleston the same month, he remained in that vicinity until February, 1788, when he embarked for St. Augustine, and was busily occupied, during this spring, in exploring east Florida. His journal mentions several sub-tropical plants, now well known to be indigenous to Florida, but which are not noticed in his Flora: such as the Mangrove *Guilandina Bonduc*, *Sophora occidentalis*, two or three Ferns, and especially the Orange.[1] Leaving Florida at the beginning of June, he returned by land to Savannah and Charleston, where he was confined by sickness the remainder of the summer. Late in the autumn, however, he made a second excursion to the sources of the Savannah, chiefly to obtain the roots and seeds of the remarkable plants he had previously discovered. He pursued the same route as before, except that he ascended the Tugaloo, instead of the

were stolen, a misfortune which, it appears from Michaux's remarks, was of no uncommon occurrence in those days ; and they were obliged to pursue their journey to that place on foot. On the way he discovered "a shrubby Rumex," which he terms *Lapathum occidentale ;* doubtless the *Polygonella parvifolia* of his Flora, and also the *Polygonum polygamum* of Ventenat.

1 "Les bois etoient remplis d'oranges aigres," etc. Michaux, MSS. See also Bartram's "Travels"; and Torrey & Gray, "Flora of North America," i. p. 222.

Seneca or Keowee River, crossing over to the latter; and, climbing the higher mountains about its sources in the inclement month of December, when they were mostly covered with snow, he at length found some trees of *Magnolia cordata*, to obtain which was the principal object of this arduous journey. Retracing his steps, he reached Charleston at the end of December, with a large collection of living trees, roots, and seeds. The remainder of the winter Michaux passed in the Bahama Islands, returning to Charleston in the month of May. Early in June he set out upon a journey to a different portion of the mountains of North Carolina, by way of Camden, Charlotte (the county seat of Mecklenburg), and Morganton, reaching the higher mountains at "Turkey Cove, thirty miles from Burke Court House" (probably the head of Turkey Creek, a tributary of the Catawba), on the 15th of June. From this place he made an excursion to the Black Mountain, in what is now Yancey County, and afterwards to the Yellow Mountain, which Michaux at that time considered to be the highest mountain in the United States. If the Roan be included in the latter appellation, as I believe it often has been, this opinion is not far from the truth; since the Black Mountain alone exceeds it, according to Professor Mitchell's recent measurements. Descending this elevated range on the Tennessee side, and traveling for the most part through an unbroken wilderness, near the end of June he reached the Block House on the Holston, famous in the annals of border warfare. Several persons had been killed by the Indians during the preceding week, and general alarm prevailing, Michaux abandoned his intention of penetrating into Kentucky, and resolved to botanize for a time in the mountains of Virginia. He accordingly entered that State, and arrived on the 1st of July at "Washington Court House, première ville dans la Virginie que l'on trouve sur la cote occidentale des montagnes, en sortant de la Carolinie Septentrionale." To this he adds the following note: "Première ville, si l'on peut nommer ville une Bourgade composée de douze maisons (log-houses). Dans cette ville on ne mange que de pain de Mays. Il n'y a viande fraiche, ni cidre, mais

seulement du mauvais Rum." Abingdon, the county seat of Washington County, is now a flourishing town; but Michaux's remarks are still applicable to more than one *première ville* in this region. From this place he continued his course along the valley of Virginia throughout its whole extent, crossing New River, the Roanoke, and passing by Natural Bridge, Lexington, Staunton, and Winchester; thence by way of Frederick in Maryland, and Lancaster, Pennsylvania, he arrived at Philadelphia on the 21st of July, and at New York on the 30th. In August and September he returned to Charleston by way of Baltimore, Alexandria, Richmond, and Wilmington, North Carolina. In November he revisited the mountains explored early in the preceding summer, passing through Charlotte, Lincolnton, and Morganton, to his former headquarters at Turkey Cove; from whence he visited the north branch of Catawba (North Cove, between Linville Mountain and the Blue Ridge?), the Black Mountain, Toe River, etc.; and returned to Charleston in December, with two thousand five hundred young trees, shrubs, and other plants. From January until April, 1791, this indefatigable botanist remained in the vicinity of Charleston; but his memoranda for the remainder of that year are unfortunately wanting. The earliest succeeding date I have been able to find is March 22, 1792, when he sold the "Jardin du Roi" at Charleston, and going shortly afterwards by water to Philadelphia, he botanized in New Jersey and around New York until the close of May. In the beginning of June he visited Milford, Connecticut, to procure information from a Mr. Peter Pound, who had traveled far in the northwest; and at New Haven took passage in a sloop for Albany, where he arrived on the 14th of June (having botanized on the way at West Point, Poughkeepsie, etc.); on the 18th he was at Saratoga; on the 20th he embarked at Skenesborough (Whitehall), botanized more or less on the shores of Lake Champlain, reaching Montreal on the 30th of June, and Quebec on the 16th of July.[1] The remainder of this season was

[1] Among the plants collected in this journey, he particularly mentions having found *Aconitum uncinatum* near Quebec; but in the Flora no

devoted to an examination of the region between Quebec and Hudson's Bay, the botany of which, as is well known, he was the first to investigate. His journal comprises a full and very interesting account of the physical geography and vegetation of that inclement district.

Leaving Quebec in October, and returning by the same route, we find our persevering traveler at Philadelphia early in December. It appears that he now meditated a most formidable journey, and made the following proposition to the American Philosophical Society: "Proposé à plusieurs membres de la Société Philosophique les avantages pour les Etats-Unis d'avoir des informations geographiques des pays a l'ouest de Mississippi, et demandé qu'ils aient à endosser mes traites pour la somme de £3600, si je suis disposé a voyager aux sources du Missouri, et même rechercher les rivières qui coulent vers l'ocean Pacifique. Ma proposition ayant été accepté, j'ai donné a Mr. Jefferson, Secretaire d'Etat, les conditions auxquels je suis disposé à entreprendre ce voyage. . . . J'offre de communiquer toutes les connoisances et informations geographiques à la Société Philosophique; et je reserve à mon profit toutes les connoisances en histoire naturelle que j'acquirerai dans ce voyage." Remaining in Philadelphia and its vicinity until the following summer, he set out for Kentucky in July, 1793, with the object of exploring the western States (which no botanist had yet visited), and also of conferring with General Clarke (at Mr. Jefferson's request) on the subject of his contemplated journey to the Rocky Mountains. He crossed the Alleghanies in Pennsylvania, descended the Ohio to Louisville, Kentucky, traversed that State and western Virginia to Abingdon, and again traveled through the valley of Virginia to Winchester, Harper's Ferry, etc., arriving at Philadelphia on the 12th of December of the same year. Conferences respecting his projected expedition were now renewed, in which Mr. Genet, the envoy from the French republic, took prominent part; but here the mat-

other locality is given than the high mountains of North Carolina. Major Le Conte found it several years ago in the southwestern part of New York, and Mr. Lapham has recently detected it in Wisconsin.

ter seems to have dropped, since no further reference is made to the subject in the journal; and Michaux left Philadelphia in February, 1794, on another tour to the southern States. In July of that year he again visited the mountains of North Carolina, traveling from Charleston to Turkey Cove by his old route. On this occasion he ascended the Linville Mountain, and the other mountains in the neighborhood; but having "differé a cause du manque des provisions," he left his old quarters (at Ainsworth's), crossed the Blue Ridge, and established himself at Crab Orchard on Doe River. From this place he revisited the Black Mountain, and, accompanied by his new guide, Davenport, explored the Yellow Mountain, the Roan, and finally the Grandfather, the summit of which he attained on the 30th of August.[1] Returning to the house of his guide, he visited Table Mountain on the 5th of September, and proceeded (by way of Morganton, Lincolnton, Salisbury, and Fayetteville, North Carolina) to Charleston, where he passed the winter.

On the nineteenth day of April, 1795, our indefatigable traveler again set out, reached the Santee River at Nelson's Ferry, ascended the Wateree, or Catawba, to Flat Rock Creek, visited Flat Rock,[2] crossed Hanging-Rock Creek, and ascended

[1] His earlier journals are full of expressions of loyalty to the king under whose patronage his travels were undertaken; but now transformed into a republican: "Monté au sommet de la plus haute montagne de toute l'Amerique Septentrionale, chanté avec mon compagnon-guide l'hymne de Marseillois, et crié, 'Vive la Liberté et la Republique Française.'" If this enthusiasm were called forth by mere elevation, he should have chanted his pæans on the Black Mountain and the Roan, both of which are higher than the Grandfather.

[2] I believe this is the only instance in which the name of Flat Rock occurs in Michaux's journal; it is in South Carolina, not far from Camden. Here, without doubt, he discovered *Sedum pusillum* (Diamorpha, Nutt.), the habitat of which is said to be "in Carolina Septentrionali, loco dicto Flat Rock." Mr. Nuttall, who subsequently collected the plant at the same locality, inadvertently continued this mistake, by assigning the habitat, "Flat Rock near Camden, *North* Carolina," as well in his "Genera of North American Plants," as in a letter to Dr. Short on this subject. (Vide Short on Western Botany, in the "Transylvania Journal of Medicine," and in Hooker's "Journal of Botany" for November, 1840, p. 103.)

the Little Catawba to Lincolnton. In the early part of May he revisited Linville Mountain, the Yellow Mountain, the Roan, and some others, and then descended Doe River and the Holston to Knoxville, Tennessee. Thence, crossing the Cumberland Mountains, and a wilderness one hundred and twenty miles in extent, he arrived at Nashville on the 16th of June, at Danville, Kentucky, on the 27th, and at Louisville on the 20th of July. In August he ascended the Wabash to Vincennes, crossed the country to the Illinois River, and devoted the months of September, October, and November to diligent herborizations along the course of that river, the Mississippi, the lower part of the Ohio, and throughout the country included by these rivers. In December he descended the Mississippi in a small boat to the mouth of the Ohio, and ascended the latter and the Cumberland to Clarksville, which he reached on the 10th of January, 1796, after a perilous voyage in the most inclement weather. Leaving that place on the 16th, he arrived at Nashville on the 19th of January; and after making a journey to Louisville and back again, he started for Carolina at the close of February, crossed the Cumberland Mountains early in March, reached Knoxville on the 8th, Greenville on the 18th, Jonesborough on the 19th, and on the 22d crossed the Iron Mountains into North Carolina, descended Cane Creek (which rises in the Roan), and spent several days in exploring the mountains in the vicinity, with his former guide, Davenport. In April he returned to Charleston by his usual route; and on the 13th of August embarked for Amsterdam in the ship Ophir. This vessel was wrecked on the coast of Holland, on the 10th of October, and Michaux lost a part of the collections he had with him; on the 23d of December, 1796, he arrived at Paris

Hence some confusion has arisen respecting the locality of this interesting plant, since there is both a Flat Rock and a village named Camden in North Carolina, although the two are widely separated. After all, Pursh's habitat, "on flat rocks in North Carolina, and elsewhere," proves sufficiently correct, since Mr. Nuttall himself, and also Mr. Curtis and others, have subsequently obtained it in such situations near Salisbury in that State, and Dr. Leavenworth found it abundantly throughout the upper district of Georgia.

with the portion he had saved. This notice of the travels of Michaux on this continent will suffice to show with what untiring zeal and assiduity his laborious researches were prosecuted; it should, however, be remarked, that greater facilities were afforded him, in some important respects, than any subsequent botanist has enjoyed; the expenses of his journey having been entirely defrayed by the French government, under whose auspices and direction they were undertaken.

The name of Fraser, so familiar in the annals of North American botany, ought, perhaps, to have preceded that of Michaux in our brief sketch; since the elder Mr. Fraser, who had visited Newfoundland previous to the year 1784, commenced his researches in the southern States as early as 1785; and Michaux, on his first expedition to the mountains in 1787, speaks as having traveled in his company for several days. We believe, however, that he did not explore the Alleghany Mountains until 1789. Under the patronage of the Russian government, he returned to this country in 1799, accompanied by his eldest son, and revisited the mountains, ascending the beautiful Roan, where, "on a spot which commands a view of five States, namely, Kentucky, Virginia, Tennessee, North Carolina, and South Carolina, the eye ranging to a distance of seventy or eighty miles when the air is clear, it was Mr. Fraser's good fortune to discover and collect living specimens of the new and splendid *Rhododendron Catawbiense*, from which so many beautiful hybrid varieties have since been obtained by skilful cultivators."[1] The father and son revisited the southern States in 1807; and the latter, after the decease of the father in 1811, returned to this country, and continued his indefatigable researches until 1817.

Many of the rarest plants of these mountains were made

[1] "Biographical Sketch of John Fraser, the Botanical Collector," in Hooker's "Companion to the Botanical Magazine," vol. ii. p. 300: an article from which I have derived nearly all the information I possess respecting the researches of the Frasers in this country, and to which the reader is referred for more particular information. A full list of the North American plants introduced into England by the father and son, is appended to that account.

known especially to English gardens and collections, by Mr. John Lyon, whose indefatigable researches are highly spoken of by Pursh, Nuttall, and Elliott. It is very probable that he had visited the mountains previous to his assuming the charge of Mr. Hamilton's collections near Philadelphia, which he resigned to Pursh in 1802. At a later period, however, he assiduously explored this region, from Georgia as far north at least as the Grandfather Mountain; and died at Asheville in Buncombe County, North Carolina, some time between 1814 and 1818. I am informed by my friend, the Rev. Mr. Curtis, that his journals and a portion of his herbarium were preserved at Asheville for many years, and that it is probable that they may yet be found.

Michaux the younger, author of the "Sylva Americana," who accompanied his father in some of his earlier journeys, returned to this country in 1801, and crossed the Alleghany Mountains twice; first in Pennsylvania on his way to the western States, and the next year in North Carolina on his return to the seaboard. In crossing from Jonesboro', Tennessee, to Morganton, by way of Toe River (not Doe River as is stated in his Travels), he accidentally stopped at the house of Davenport, his father's guide in these mountains. The observations of the younger Michaux on this part of the Alleghany Mountains, in a chapter of his Travels devoted to that subject, are mainly accurate.

"In the beginning of 1805," Pursh, as he states in the preface to his Flora, "set out for the mountains and western territories of the southern States, beginning at Maryland and extending to the Carolinas (in which tract the interesting high mountains of Virginia and Carolina took my particular attention), and returning late in the autumn through the lower countries along the seacoast to Philadelphia." This plan, however, was not fully carried out, since he does not appear to have crossed the Alleghanies into the great western valley, nor to have botanized along these mountains farther south than where the New River crosses the valley of Virginia. At any rate, it is certain that the original tickets of his specimens in the herbarium of the late Professor Barton, under

whose patronage he traveled, as well as those in Mr. Lambert's herbarium, furnish no evidence that he extended his researches into the mountainous portion of North Carolina; but it appears probable (from some labels marked Halifax or Mecklenburg, Virginia) that he followed the course of the Roanoke into the former State. His most interesting collections were made at Harper's Ferry, Natural Bridge, the Peaked Mountains (which separate the two principal branches of the Shenandoah), the Peaks of Otter, in the Blue Ridge; also, Cove Mountain, Salt Pond Mountain, and Parnell's Knob (with the situation of which I am unacquainted), the region around the Warm Sulphur Springs, Capon Springs, the Sweet Springs, and the mountains of Monroe and Greenbrier counties.

Early in the present century, Mr. Kin, a German nurseryman and collector, resident at Philadelphia, traveled somewhat extensively among the Alleghany Mountains, chiefly for the purpose of obtaining living plants and seeds. He also collected many interesting specimens, which may be found in the herbaria of Muhlenberg and Willdenow, where his tickets may be recognized by the orthography, and the amusing mixture of bad English and German (with occasionally some very singular Latin) in which his observations are written.

In the winter of 1816, Mr. Nuttall crossed the mountains of North Carolina from the west, ascending the French Broad River (along the banks of which he obtained his *Philadelphus hirsutus*, etc.) to Asheville, passing the Blue Ridge, and exploring the Table Mountain, where he discovered *Hudsonia montana*, etc., and collected many other rare and interesting plants.[1]

As early as 1817, the mountains at the sources of the Saluda River were visited by the late Dr. MacBride, the friend

[1] The spur of the Blue Ridge from which the picturesque Table Mountain rises like a tower, is called by Mr. Nuttall the Catawba Ridge. I am informed, however, by my friend Mr. Curtis, who is intimately acquainted with this interesting region, that it is not known by that name, but is called the Table Mountain Ridge. Its base is not washed by the Catawba River, but by its tributary the Linville.

and correspondent of Elliott; who, in the preface to the second volume of his "Sketch," renders an affecting and deserved tribute to his memory, and acknowledges the important services which he had rendered to that work during its progress.

The name of Rafinesque should also be mentioned in this connection; since that botanist crossed the Alleghanies four or five times between 1818 and 1833 (in Pennsylvania, Maryland, and the north of Virginia), and also explored the Cumberland Mountains.

A few years since, the Peaks of Otter, in Virginia, were visited by Mr. S. B. Buckley; and still more recently the same botanist has explored the mountains in the upper part of Alabama and Georgia, and the adjacent borders of North Carolina. Among the interesting contributions which the authors of the "Flora of North America" have received from this source, I may here mention the *Coreopsis latifolia* of Michaux, which had not been found by any subsequent botanist until it was observed by Mr. Buckley in the autumn of 1840.

No living botanist, however, is so well acquainted with the vegetation of the southern Alleghany Mountains, or has explored those of North Carolina so extensively, as the Rev. Mr. M. A. Curtis; who, when resident for a short time in their vicinity, visited, as opportunity occurred, the Table Mountain, Grandfather, the Yellow Mountain, the Roan, the Black Mountain, etc., and subsequently (although prevented by infirm health from making large collections) extended his researches through the counties of Haywood, Macon, and Cherokee, which form the narrow southwestern extremity of North Carolina. To him we are indebted for local information which greatly facilitated our recent journey, and, indeed, for a complete itinerarium of the region south of Ashe County. But, as the latter county had not been visited by Mr. Curtis, nor so far as we are aware by any other botanist, and being from its situation the most accessible to the traveler from the north, we determined to devote to its examination the principal part of the time allotted to our own excursion.

Intending to reach this remote region by the way of the Valley of Virginia, we left New York on the evening of the 22d of June, and traveling by railroad, reached Winchester, a distance of three hundred miles, before sunset of the following day. At Harper's Ferry, where the Potomac, joined by the Shenandoah, forces its way through the Blue Ridge, in the midst of some of the most picturesque scenery in the United States, we merely stopped to dine, and were therefore disappointed in our hope of collecting *Sedum telephoides*, *S. pulchellum*, *Paronychia dichotoma*, and *Draba ramosissima*, all of which grow here upon the rocks. We observed the first in passing, but it was not yet in flower. On the rocky banks of the Potomac below Harper's Ferry, we saw for the first time the common Locust-tree (*Robinia Pseudacacia*) decidedly indigenous. It probably extends to the southern confines of Pennsylvania; and from this point south it is everywhere abundant, but we did not meet with it east of the Blue Ridge. From Winchester, the shiretown of Frederick County, we proceeded by stage-coach directly up the Valley of Virginia, as that portion of the State is called which lies between the unbroken Blue Ridge and the most easterly ranges of the Alleghanies. From the Potomac to the sources of the Shenandoah it is, strictly speaking, a valley, from twenty to thirty miles in width, with a strong, chiefly limestone soil of great fertility. It is scarcely interrupted, indeed, up to where the Roanoke rises; but a branch of the Alleghanies intervenes between the latter and New River, as the upper part of the Great Kenawha is termed, from which point it loses its character in some degree, and is exclusively traversed by the western waters. The same valley extends to the north and east through Maryland and Pennsylvania, and even into the State of New York, preserving throughout the same geological character and fertile soil. Our first day's ride was to Harrisonburg, in Rockingham County, a distance of sixty-nine miles from Winchester. From the moment we entered the valley, we observed such immense quantities of *Echium vulgare*, that we were no longer surprised at the doubt expressed by Pursh whether it was really an introduced plant. This "vile foreign

weed," as Dr. Darlington, agriculturally speaking, terms this showy plant, is occasionally seen along the roadside in the northern States; but here, for the distance of more than a hundred miles, it has taken complete possession even of many cultivated fields, especially where the limestone approaches the surface, presenting a broad expanse of brilliant blue. It is surprising that the farmers should allow a biennial like this so completely to overrun the land. Another plant much more extensively introduced here than in the north (where it scarcely deserves the name of a naturalized species) is *Bupleurum rotundifolium*, which in the course of the day we met with abundantly. The *Marubium vulgare* is everywhere naturalized; and *Euphorbia Lathyris* must also be added to the list of naturalized plants. The little *Verbena angustifolia* is also a common weed. We collected but a single indigenous plant of any interest, and one which we by no means expected to find, namely, *Carex stenolepsis* of Torrey,[1] which here, as in western States, to which we supposed it confined, takes the place of the northern *C. retrorsa*. We searched for its constant companion, *C. Shortii*, and the next day we found the two growing together. During the day's ride we observed that the bearded wheat was almost exclusively cultivated, and were informed that it had been found less subject to the ravages of the "Fly" than the ordinary varieties; which may be owing to recent introduction of the seed of the bearded variety from districts unmolested by this insect.

The following day we traveled only sixteen miles on our route, but from Mount Sidney made an interesting excursion on foot to Weyer's Cave, one of the largest, and certainly the most remarkable grotto in the United States. It has been so often described as to render any account on our part super-

[1] It is the *C. Frankii* of Kunth (1837), and of Kunze's Supplement to Schkuhr's "Caricography," where it is well figured. It was also distributed among Dr. Frank's plants under the name of *C. atherodes*, and with the locality of Baltimore in Pennsylvania! I had always supposed it to be derived from some part of the western States; but since it abounds in the Valley of Virginia, it may have been collected near Baltimore, Maryland.

fluous. Near the cave we saw some trees of *Tillia heterophylla*, Vent. (*T. alba*, Michx. f.), and collected a few specimens with unopened flower-buds. It appears to be the most abundant species along the mountains.

Our ride next day offered nothing of interest. Near Staunton, we saw some patches of *Delphinium consolida*, where it was pretty thoroughly naturalized in the time of Pursh. We did not observe *Spiræa lobata*, which Michaux first met with in this vicinity, and which Pursh, as well as later botanists, found in various parts of the valley. Passing the town of Lexington in the evening, we arrived at the Natural Bridge towards morning, where we remained until Monday, and had an opportunity of botanizing for a short time before we left. On the rocks we found plenty of *Asplenium Ruta-muraria*, *Sedun ternatum*, and *Draba ramosissima* with ripe fruit. In the bottom of the ravine, directly under the stupendous natural arch (the point which affords the most impressive view of this vast chasm), we collected specimens of *Heuchera villosa*, Michx., in fine flower on the 28th of June; although, in the higher mountains of North Carolina, where it also abounds, the flowers did not appear until near the end of July. This species is excellently described by Michaux, to whose account it is only necessary to add that the petals are very narrow, appearing like sterile filaments. Although a smaller plant than *H. Americana*, the leaves are larger, and vary considerably in the depths of the lobes. It is both the *H. villosa* and *H. caulescens* of Pursh, who probably derived the latter name from the strong elongated rhizoma, often projecting and appearing like a suffrutescent stem, by which the plant is attached to the rocks; since he does not describe the scape as leafy, nor is this at all the case in the original specimens. The *H. caulescens* of Torrey and Gray's Flora with the synonym, must also be united with *H. villosa*, which in that work is chiefly described from specimens collected by Dr. Short in Kentucky, where everything seems to grow with extraordinary luxuriance. With these, the plant we collected entirely accords except that the leaves are mostly smaller, and more deeply lobed;

but this character is not constant.[1] Soon after leaving Natural Bridge, we observed indigenous trees of the Honey Locust (*Gleditschia triacanthos*), also *Æsculus Pavia?* and, in crossing the valley of the James River, we noticed the Papaw (*Uvaria triloba*) and Negundo. The roadside was almost everywhere occupied with *Verbesina Siegesbeckia* not yet in flower; and in many places with *Melissa* (*Calamintha*) *Nepeta*, which Mr. Bentham has not noticed as an American plant, although Pursh has it as a native of the country. It was, however, doubtless introduced from Europe, but is completely naturalized in the valley of Virginia, in Tennessee, and in North Carolina east of the Blue Ridge.

On Tuesday, the 29th of June, we crossed the New River, arrived at Wytheville, or Wythe Court House, towards evening; and at Marion, or Smythe Court House, on the Middle Fork of the Holston, early the next morning. The vegetation of this elevated region is almost entirely similar to that of the northern States. The only herbaceous plants we noticed, as we passed rapidly along, which we had not seen growing before, were *Galax aphylla*, and *Silene Virginica:* the showy, deep-red flowers of the latter, no less than the different habitus, caused us to wonder how it could ever have been confounded with the northern *S. Pennsylvanica*. The only forest tree with which we were not previously familiar was the large Buckeye, *Æsculus flava*, which abounds in this region, and attains the height of sixty to ninety feet, and the diameter of two or three feet or more at the base.

At Marion we determined to leave the valley road, and to cross the mountains into Ashe County, North Carolina; the morning was occupied in seeking a conveyance for this pur-

[1] Much to our disappointment we did not meet with *Heuchera hispida*, although I have since learned from an inspection of Barton's herbarium, that we passed within a moderate distance from the place where Pursh discovered it. The habitat given on the original ticket, "High Mountains between Fincastle and the Sweet Springs, and some other similar places," we here cite, with the hope that it may guide some botanist to its rediscovery. The habitat in Pursh's Flora, "High Mountains of Virginia and Carolina," is probably a mere guess, so far as relates to the latter State.

pose. With considerable difficulty we at length procured a carry-all (a light, covered wagon with springs, drawn by a single horse), capable of conveying our luggage and a single person besides the driver, a simple shoemaker who had never before undertaken so formidable a journey, and who accordingly proved entirely wanting in the skill and tact necessary for conducting so frail a vehicle over such difficult mountain tracks, for roads they can scarcely be called. We had first to ascend the steep ridge interposed between the middle and south Forks of the Holston, called Brushy Mountain, during the ascent of which we commenced botanizing in earnest. The first interesting plant we met with was *Saxifraga erosa* of Pursh, but only with ripe fruit, and even with the seeds for the most part fallen from the capsules. The same locality also furnished us with specimens of the pretty *Thalictrum filipes*, Torr. & Gray (to which the name of *T. clavatum*, DC. must be restored), a plant which abounds along all the cold and clear brooks throughout the mountains of North Carolina; where it could not well have escaped the notice of Michaux, in whose herbarium De Candolle found the specimen (with no indication of its habitat) on which his *T. clavatum* was established. The authors of the "Flora of North America," having only an imperfect fruiting specimen of their *T. filipes*, and not sufficiently remarking the discrepancies between the *T. clavatum*, Hook. "Fl. Bor.-Am." and the figure and description of De Candolle's plant, in regard to the length of the styles, assumed the former to be the true *T. clavatum*, and described their own plant as a new species. But our specimens accord so perfectly with the figure of Delessert (except in the greater but variable length of the stipes to the fruit, and in the veining of the carpels, which, doubtless by an oversight of the artist, is omitted in the figure) as to leave no doubt of their identity. The subarctic plant may be appropriately called *T. Richardsonii*, in honor of its discoverer. The flowers of this species are uniformly perfect, as indeed they are figured by Delessert, although De Candolle has otherwise described them. It is a slender, delicate plant, from eight to twelve, or rarely exceeding eigh-

teen inches in height, with pure white flowers. During this ascent we collected *Galium latifolium*, Michx., just coming into flower; and we subsequently found this species so widely diffused throughout the mountains of North Carolina, that we were much surprised at its remaining so little known since the time of Michaux. On a moist, rocky bank by the road-side, we gathered some specimens of a Scutellaria, which did not again occur to us. It proves to be a species mentioned by Mr. Bentham under *S. serrata*, and subsequently described by Dr. Riddell with the name of *S. saxatilis*, which apparently is not of uncommon occurrence westward of the Alleghany Mountains. It is a slender plant, from six to twenty inches high; and the stems often produce slender subterranean runners from their base. We here also collected *Asarum Virginicum*, Linn. in similar situations. In the higher mountains the northern *A. Canadense* takes the place of the former species, while *A. arifolium*, Michx. seems to be confined to the lower country. The banks of the shady and cool rivulets, which we crossed every few minutes during our ascent, were in many places covered by the prostrate or creeping *Hedyotis serpyllifolia*, Torr. & Gray (*Houstonia serpyllifolia*, Michx.), which continues to flower sparingly throughout the summer. This pretty plant has quite the habit of *Arenaria Balearica;* and the root is certainly perennial. We found it very abundant in similar situations throughout this mountain region. Towards the summit of this ridge we first met with the *Magnolia Fraseri* (*M. auriculata*, Bart.), which resembles the Umbrella-tree (*Magnolia Umbrella*) in the disposition of its leaves at the extremity of the branches. This, as well as *M. acuminata* (the only other species of Magnolia that we observed), is occasionally termed Cucumber-tree: but the people of the country almost uniformly called the former Wahoo, a name which in the lower part of the southern States is applied to *Ulmus alata*, or often to all the Elms indifferently. The bitter and somewhat aromatic infusion of the green cones of both these Magnolias in whiskey or apple-brandy is very extensively employed as a preventive against intermittent

fevers; a use which, as the younger Michaux remarks, would doubtless be much less frequent, if, with the same medical properties, the aqueous infusion were substituted.

Nearly at the top of this mountain we overtook our awkward driver, awaiting our arrival in perfect helplessness, having contrived to break his carriage upon a heap of stones, and to overthrow his horse into the boughs of a prostrate tree. So much time was occupied in extricating the poor animal and in temporary repairs to the wagon, that we had barely time to descend the mountain on the opposite side, and to seek lodgings for the night in the secluded valley of the South Fork of the Holston. In moist, shady places along the descent of this mountain, and in similar situations throughout the mountains of North Carolina, we found plenty of the northern *Listera convallarioides*, in fine state, entirely similar to the plant from Vermont, Canada, Newfoundland, and the Northwest Coast, and agreeing completely with the figure of Swartz (in Weber & Mohr, "Beiträge zur Naturkunde," I., 1805, p. 2, t. I.), and the recent one of Hooker's "Flora Boreali Americana." It is difficult to conceive why Willdenow should cite the *Ophrys cordata* of Michaux under the *Epipactis convallarioides* of Swartz, while there is so little accordance in their characters; but this has not prevented Pursh from combining the specific phrase of the two authors into one, while he assigns a locality for the plant (New Jersey), where the *Listera convallarioides* certainly does not grow. The Rev. Mr. Curtis, I believe, first detected the plant in these mountains.

The next day (July 1) we crossed the Iron Mountains (the great chain which divides the States of North Carolina and Tennessee, and which here forms the northwestern boundary of Grayson County, Virginia) by Fox-Creek Gap, and traversing the numerous tributaries of the North Fork of the New River, which abundantly water this sequestered region, we slept a few miles beyond the boundary of North Carolina, after a journey of nearly thirty miles. It must not be imagined that we found hotels or taverns for our accommodation; as, except at Ashe Court House, we saw no house of public

entertainment from the time we left the valley of Virginia until we finally crossed the Blue Ridge and quitted the mountain region. Yet we suffered little inconvenience on this account, as we were cordially received at the farm-houses along the road, and entertained according to the means and ability of the owners; who seldom hesitated either to make a moderate charge, or to accept a proper compensation for their hospitality, which we therefore did not hesitate to solicit from time to time. On the Iron Mountains we met with nearly all the species we had collected during the previous day, and with a single additional plant of much interest, namely, the *Boykinia aconitifolia*, Nutt. We found it in the greatest abundance and luxuriance on the southern side of the mountain, near the summit, along the rocky margins of a small brook, which for a short distance were completely covered with the plant. It here attains the height of two feet or more; the stems, rising from a thick rhizoma (and clothed below, as well as the petioles, with deciduous rusty hairs), are terminated by a panicle of small cymes, which at first are crowded, but at length are loose, with the flowers mostly unilateral. The rather large, pure white petals are deciduous after flowering, not marcescent as in Saxifraga and Heuchera. We did not again meet with this plant; but Mr. Curtis collected it several years ago near the head of Linville River, and Mr. Buckley obtained it in the mountains of Alabama. It also extends further north than our own locality; for, although not described in his Flora, Pursh collected it on the Salt-Pond Mountain in Virginia.[1] I have little doubt that the *Saxifraga Richardsonii* would be more correctly transferred to Boykinia, as well as the *S. ranunculifolia*; and, since the *S. elata* of Nuttall, in Torrey and Gray's

[1] The specimen in Professor Barton's herbarium (in fruit) is ticketed by Pursh: "*Heuchera villosa*, Michx.? Salt-Pond Mountain, under the naked knob, near a spring. This spring is the highest I have seen." I know not the exact situation of this mountain from which Pursh obtained many interesting plants. The *Boykinia aconitifolia*, I may remark, would be a very desirable plant in cultivation, and might be expected to endure the winter of New York and Philadelphia; it would certainly flourish in England.

"Flora," is referred to *Boykinia occidentalis*, in the supplement to that work, no pentandrous Saxifrage remains, except the ambiguous *S. Sullivantii*, Torr. & Gr. But the authors of the Flora having received fruiting specimens of this interesting plant, do not hesitate to remove it from the genus to which it was provisionally appended, and to dedicate it to their esteemed correspondent, the promising botanist who discovered it.[1]

While descending the mountain on the opposite side, we met with *Clethra acuminata*, a very distinct and almost arborescent species, which is well characterized by Michaux. The flowers were not yet expanded; but towards the end of July we obtained from other localities specimens in full flower, while the racemes and capsules of the preceding year were still persistent. The conspicuous bracts, it may be remarked, are as caducous in the wild as they are said to be in the cultivated plant; usually falling before the flower-buds have attained their full size. We also saw *Campanula divaricata*, Michx., not yet in flower; and obtained fruiting specimens of the *Convallaria umbellulata*, Michx. (Clintonia, Raf., not of Dougl.). While the character in Michaux is drawn from

[1] *Sullivantia.* Torrey & Gray, "Fl. N. Amer." suppl. ined. — Calyx inferne imo ovario adnatus, limbo quinquefido. Petala 5 spathulata, unguiculata, integra, summo calycis tubo inserta, marcescentia. Stamina 5, laciniis calycinis breviora: antheræ biloculares. Styli, 2, breves; stigmatibus simplicibus. Capsula calyce inclusa, bilocularis, birostris, polysperma, rostris intus longitudinaliter dehiscentibus. Semina adscendentia, scobiformia; testa membracea, relaxata, utrinque ultra nucleum ovalem alatim producta. Embryo cylindricus albumine vix brevior. — Herba humilis, in rupibus calcareis Ohionis vigens; radice fibrosa perenni; foliis plerisque radicalibus, rotundato-reniformibus, inciso-dentatis sublobatisve, longe petiolatis; scapo gracili, decumbente; floribus parvis (corolla conspicua, alba), cymuloso-paniculatis, post anthesin in apicem pedicellorum arcte deflexis.

S. Ohionis. — *Saxifraga?* *Sullivantii*, Torrey & Gray, "Fl. N. Amer." i. 575. — Genus a Saxifraga præcipue diversum staminibus petalis isomeris, et seminibus scobiformibus: a Boykinia calyce fere libero, atque seminibus; ab Heuchera capsula biloculari, etc.; a Leptarrhena staminibus 5, antheris bilocularibus, et seminibus alato-marginatis, nec utrinque subulatis.

this species, the "planta Canadensis" there mentioned is the nearly allied *Dracæna borealis* of the "Hortus Kewensis." The two species are mixed in Michaux's herbarium; and, although the latter is almost exclusively a northern plant, we found the two species growing together on the Grandfather, Roan, and other high mountains of North Carolina. Towards the base of the mountain we saw for the first time the Pyrularia of Michaux (Oil-nut, Buffalo-tree, etc.; *Hamiltonia oleifera*, Muhl.): a low shrub which is not of unfrequent occurrence in rich, shady soil. Its geographical range extends from the Cherokee country on the confines of Georgia (where the elder Michaux discovered it on his earliest visit to the mountains, and where Mr. Curtis has recently observed it), to the western ranges of the Alleghanies of Pennsylvania in lat. 40°, where it was found by the younger Michaux.[1] It flowers early in the season, and the oleaginous fruit in the specimens we collected had attained the size of a musket-ball.

In wet places, on the very borders of North Carolina, but still within Virginia, we first met with *Trautvetteria palmata* and *Diphylleia cymosa*; the former in full flower, the latter in fruit. Trautvetteria, which I doubt not is more nearly allied to Thalictrum than to Cimicifuga or Actæa, was collected by Pursh in Virginia, both on the Salt-Pond Mountain and on the Peaks of Otter. The Diphylleia is confined to springy places, and the margins of the shaded mountain brooks, and the rich and deep alluvial soil which is so general throughout these mountains, never occurring perhaps at a lower elevation than three thousand feet above the level of the sea. It is a more striking plant than we had supposed; the cauline leaves (generally two, but sometimes three in number) being often two feet in diameter, and the radicle, which is obicular and centrally peltate as in Podophyllum, frequently still larger; so that it is not easy (at this season) to obtain manageable specimens. The branches of the cymes are usually reddish or purple, and the gibbous deep blue and glaucous berries are almost dry when ripe. The latter often

[1] "Travels to the Westward of the Alleghany Mountains," etc. English edition, p. 57, etc.

contain as many as four perfect seeds; and it is proper to remark that the embryo is not "very minute," as described in the "Flora of North America"; but, in the ripe seeds recently examined, is one third the length of the albumen, as stated by Decaisne, or even longer. The cotyledons are elliptical, flattish, and nearly the length of the thick, slightly club-shaped radicle. The whole embryo is also somewhat flattened; so that when the seed is longitudinally divided in one direction, the embryo, examined in place, appears to be very slender, and to agree with De Candolle's description. The albumen is horny when dry, and has a bitter taste. Along the roadside we shortly afterwards collected the equivocal *Vaccinium erythrocarpum* of Michaux, or *Oxycoccus erectus* of Pursh; a low, erect, dichotomously branched shrub, with the habit, foliage, and fruit of Vaccinium, but the flowers of Oxycoccus. It here occurred at a lower elevation than usual, scarcely more than three thousand feet above the level of the sea, and in a dwarfish state (about a foot high): subsequently we only met with it on the summit of the Grandfather and other mountains which exceed the altitude of five thousand feet, where it is commonly three or four feet high. We were too early for the fruit, a small, red or purplish berry, which does not ripen until August or September. It has an exquisite flavor, according to Pursh, who found the plant on the mountains of Virginia; but our friend Mr. Curtis informs us that it is rather insipid, and entirely destitute of the fine acidity of the cranberry.

On the 2d of July we continued our journey (eleven miles) to Jefferson or Ashe Court House, a hamlet of twenty or thirty houses, and the only village in the county. Intending to make this place our headquarters while we remained in the region, we had the good fortune to find excellent accommodations at the house of Colonel Bower, who evinced every disposition to further our inquiries, and afforded us very important assistance. We may remark indeed, that during our residence amongst the mountains we were uniformly received with courtesy by the inhabitants; who for the most part lacked the general intelligence of our obliging host at Jeffer-

son, and could scarcely be made to comprehend the object of our visit, or why we should come from a distance of seven hundred miles to toil over the mountains in quest of their common and disregarded herbs. Curiosities as we were to these good folks, their endless queries had no air of impertinence, and they entertained us to the best of their ability, never attempting to make unreasonable charges. A very fastidious palate might occasionally be at a loss; but good corn-bread and milk are everywhere abundant; the latter being used from preference quite sour, or even curdled. Sweet milk appears to be very generally disliked, being thought less wholesome, and more likely to produce the "milk sickness," which is prevalent in some very circumscribed districts; so that our dislike of sour and fondness for sweet milk was regarded by this simple people as one of our very many oddities. Nearly every farmer has a small dairy-house built over a cold brook or spring, by which the milk and butter are kept cool and sweet in the warmest weather.

We botanized for several days upon the mountains in the immediate neighborhood of Jefferson, especially the Negro Mountain, which rises abruptly on one side of the village, the Phœnix Mountain, a sharp ridge on the other side, and the Bluff, a few miles distant in a westerly direction. The altitude of the former is probably between four and five thousand feet above the sea; the latter is apparently somewhat higher. They are all composed of Mica-slate; and we should remark that we entered upon a primitive region immediately upon leaving the Valley of Virginia. The mountain sides, though steep or precipitous, are covered with a rich and deep vegetable mould, and are heavily timbered, chiefly with Chestnut, White Oak, the Tulip-tree, the Cucumber-tree, and sometimes the Sugar Maple. Their vegetation presents so little diversity, that it is for the most part unnecessary to distinguish particular localities. Besides many of the plants already mentioned, and a very considerable number of northern species which we have not room to enumerate, we collected or observed on the mountain sides, *Clematis Viorna* in great abundance; *Tradescantia Virginica; Iris cristata* in fruit;

Hedyotis (*Amphiotis*) *purpurea*, which scarcely deserves the name, since the flowers are commonly almost white; *Phlox paniculata? Aristolochia Sipho*, without flowers or fruit; *Ribes Cynosbati*, *rotundifolium*, Michx. (*R. triflorum*, Willd.) and *prostratum*, L'Her.; *Allium cernuum* and *tricoccum*;[1] *Galax aphylla; Ligusticum actæifolium*, the strong-scented roots of which are eagerly sought and eaten by boys and hogs;[2] The Ginseng, here called "sang" (the roots of which are largely collected, and sold to the country merchants when fresh for about twelve cents per pound, or when dry for triple that price); *Menziesia globularis*, mostly in fruit; and the showy *Azalea calendulacea*, which was also out of flower, except in deep shade.[3] In the latter situations

[1] The latter is known throughout this region by the name of "Ramps"; doubtless a corruption of "Ramsons," the popular appellation of *A. ursinum* in England.

[2] It is here termed "Angelico"; while in Virginia it is called "Nondo." Bartram (Travels, p. 45, and p. 367), who found it in Georgia, notices it under the name of *Angelica lucida*, or "White-root" of the Creek and Cherokee traders. "Its aromatic carminative root is in taste much like that of ginseng, though more of the taste and scent of aniseseed: it is in high estimation with the Indians as well as white inhabitants, and sells at a great price to the southern Indians of Florida, who dwell near the sea-coast, where this never grows spontaneously." (Bartram, *l. c.*)

[3] Bartram well describes this species, under the name of *Azalea flammea*, or fiery Azalea. "The epithet fiery I annex to this most celebrated species of Azalea, as being expressive of the appearance of its flowers; which are in general of the color of the finest red-lead, orange and bright gold, as well as yellow and cream-color. These various splendid colors are not only in separate plants, but frequently all the varieties and shades are seen in separate branches on the same plant; and the clusters of the blossoms cover the shrubs in such incredible profusion on the hillsides, that suddenly opening to view from dark shades, we are alarmed with apprehension of the woods being set on fire. This is certainly the most gay and brilliant flowering shrub yet known; they grow in little copses or clumps, in open forests as well as dark groves, with other shrubs, and about the bases of hills, especially where brooks and rivulets wind about them; the bushes seldom rise above six or seven feet in height, and generally but three, four, or five, but branch and spread their tops greatly; the young leaves are but very small whilst the shrubs are in bloom, from which circumstance the plant exhibits a greater show of splendor." (Bartram's Travels, p. 323.)

we found an arborescent tetramerous species of Prinos (in fruit only), with large and membranaceous ovate leaves. The same species has been collected on the Pokono Mountains in Pennsylvania by Mr. Wolle, and on the Catskills by Mr. S. T. Carey. We should deem it the *P. lævigatus* of Pursh (not of Torrey, Fl. Northern States), on account of the solitary and subsessile fertile flowers, as well as the habitat, were not the flowers of that species said to be hexamerous.

In damp, very shady places high up the Negro Mountain we saw an Aconitum not yet in flower; and on moist rocks near the summit, obtained a few fruiting specimens of a Saxifraga which was entirely new to us. In a single very secluded spot on the north side of the mountain, near the summit, the rocks were covered with a beautiful small Fern, which proves to be the *Asplenium Adiantum-nigrum* of Michaux, the *A. montanum*, Willd., an extremely rare plant. It is certainly distinct from the *A. Adiantum-nigrum;* being not only a much smaller and more delicate species (two to four inches high), but the fronds are narrower, the pinnæ ovate and much shorter, 3–5 parted, with the pinnulæ toothed or incised at the apex.

The *Veratrum parviflorum*, Michx., is of frequent occurrence throughout this region, but was not yet fully in flower, so that our specimens were not collected until near the end of July. The plant is excellently described in the Flora of Michaux, where it is, probably with justice, referred to Veratrum rather than to Melanthium; since the divisions of the perianth (yellowish-green from the first) are wholly destitute of glands, and only differ from Veratrum in being stellate, and tapering at the base. I may here remark that the name Melanthium must undoubtedly be retained for *M. Virginicum* and *M. hybridum*. Some years since, in rearranging the North American species of this family, I followed Rœmer and Schultes in adopting the genus Leimanthium of Willdenow, without considering that Melanthium was established by Clayton and Gronovius on *M. Virginicum*, and thus taken up by Linnæus, with the addition of a Siberian plant, which

belongs to Zigadenus.[1] The *Melanthium Capense* (Androcymbium, Willd.) was added some time afterwards.

The rocky summits of the mountains afforded us *Sedum telephoides; Heuchera villosa; Paronychia argyrocoma*, which forms dense silvery tufts on the highest and most exposed peaks; *Veronica officinalis*, *serpyllifolia*, and *agrestis* (all certainly native); *Lycopodium rupestre*, in a very beautiful state, and on the Phœnix Mountain we found a solitary specimen of *L. Selago; Arabis lyrata*, with perfectly accumbent cotyledons; *Potentilla tridentata*, which we only saw on the Bluff Mountain; *Woodsia Ilvensis; Saxifraga leucanthemifolia*, which not unfrequently attains the height of two feet, with a large and slender effuse panicle; *Diervilla trifida*, entirely resembling the northern plant; *Pyrus melanocarpa; Sorbus Americana*, *β. microcarpa; Rhododendron Catawbiense*, just out of flower, while *R. maximum*, extremely abundant along the streams and mountain sides, was only beginning to expand its blossoms.[2] In such situations also we found a marked dwarfish variety of *Hedyotis purpurea*, growing somewhat in tufts, and scarcely exceeding four or five inches in height. The flowers, which are deep pink, while in the ordinary form in this region they are nearly white, present the dimorphism which obtains in several sections of the genus; the stamens in some specimens being inserted in the throat of the corolla and exsert, while in others they are inserted near the base of the tube and included; in the former the style is uniformly short and included, and in the latter long and somewhat exserted. These two forms were often seen growing side by side, and appeared to be equally fertile. The *Amianthium muscætoxicum*, which is common in the low country of the southern States, we here found only

[1] The *Helonias glaberrima*, "Botanical Magazine," t. 1680, on which *Zigadenus commutatus* of Schultes is founded, is *Z. glaucus;* the specimens came from Fraser's nursery, but doubtless were not derived from the southern States. *Helonias bracteata*, "Botanical Magazine," t. 1703, is *Z. glaberrimus*, Michx., not fully developed.

[2] These shrubs bear the name of "Laurel"; while the *Kalmia latifolia* is universally called "Ivy," or "Ivy-bush."

in the rich open woods of the Bluff Mountain, and in similar places further south. The flowers are pure white or cream-color, in a dense and very showy raceme, at length changing to green. The cattle which roam in the woods for a great part of the year are sometimes poisoned by feeding, as is supposed, on the foliage of this plant during the autumn: hence its name of "Fall-poison." The wild Pea-vine, which is so highly prized as an autumnal feed for cattle, is the Amphicarpæa.[1] The Lily of the Valley (*Convallaria majalis*), which we occasionally met with in fruit, appears to be identical with the European plant. It extends from the mountains of Virginia to Georgia, where it was long ago noticed by the younger Bartram. We also collected a handsome Phlox, of frequent occurrence in rich woods, which differs from *P. Carolina* (with which it has perhaps been confounded) in its perfectly smooth stem, and broader, less pointed calyx-teeth. The leaves are sometimes an inch in width, and four or five in length; the uppermost often ovate-lanceolate, and more or less cordate at the base.

A species of Carex, nearly allied to *C. gracillima*, occurs in the greatest abundance on all the higher mountains of North Carolina, forming tufts on the earth or on rocks, and flowering throughout the summer. On this account it is called *C. æstivalis* by Mr. Curtis, who discovered it several years since, and pointed out its characters.[2] We also met

[1] In the large woods the surface of the soil is covered with a species of wild peas, which rise three feet above the earth, and of which the cattle are very greedy. They prefer this pasture to every other, and when removed from it they fall away, or make their escape to return to it." (Michaux, F. A., Travels, p. 316.)

[2] *C. æstivalis* (M. A. Curtis, ined.): spicis 3–5 gracilibus laxifloris suberectis, infirma pedunculata, cæteris subsessilibus, suprema androgyna inferne mascula, bracteis inferioribus foliaceis vix vaginantibus superioribus setaceis, perigyniis ovoideis trigonis basi apiceque acutiusculis obsolete nervosis glabris ore subintegro squamam ovatam obtusam (nunc mucronatam) duplo superantibus, stigmatibus tribus, vaginis foliorum inferiorum pubescentibus.

Hab. in montibus altioribus Carolinæ Septentrionalis ubique. Julio-Augusto floret. — *C. gracillimæ* nimis affinis; at diversa, culmis foliisque gracilioribus, vaginis infirmis pubescentibus; bracteis vix vaginantibus;

with *C. canescens*, Linn. ex Boott (*C. Buxbaumii*, Wahl.) and *C. conoidea*, Schk., on the moist grassy brow of a precipice of the Bluff; and towards the base of the Negro Mountain we observed *C. virescens* and *C. digitalis*, Willd.

In a cool, sequestered brook, we found the true *Cardamine rotundifolia*, Michx., growing like the Water-cress (for which it might be substituted, as its leaves have exactly the same taste), but producing numerous stolons two or three or

spicis angustioribus et laxifloris erectis, superioribus brevissime pedunculatis; acheniis oblongo-ovoideis magis stipitatis.

The figure of *C. gracillima*, in Professor Kunze's Supplement to Schkuhr's Carices, is excellent, except that the immature perigynia are represented with more distinct beaks than I have ever seen. To this genus, already perhaps the most extensive in the vegetable kingdom, after Senecio, Mr. Sullivant has recently added another species, an account of which may be appended to this note. As Dr. Boott had already dedicated it to the zealous discoverer, without being aware that he had distributed it under another name, I trust I may be allowed to publish the notes of this sedulous caricographer unchanged:

"*C. Sullivantii* (Boott): spica mascula solitaria cylindrica, fœmineis 3–5 cylindricis erectis gracilibus pedunculatis laxifloris, superioribus contiguis, infirma remota longe pedunculata basi attenuata, stigmatibus tribus, perigyniis ellipticis brevi-rostratis emarginatis pellucido-punctatis apice marginibusque piloso-hispidis squamam ovatam ciliatam hispido-mucronatam subæquantibus.

"Culmus bipedalis, gracilis, triqueter, pilis albis sparsis longis scabriusculus, pars spicas gerens 2–9–uncialis. Folia 2 lin. lata, culmo breviora, marginibus nervisque scabris. Bractea infirma vaginans, foliacea, culmum adæquans, reliquæ sensim breviores, superiores evaginatæ demum setaceæ. Spica mascula uncialis, vix lineam lata, sessilis vel brevi-pedunculata: squamæ, muticæ, obtusæ, apice ciliobatæ, nemo scabro, pallidæ castaneæ spicæ fœmieæ 3–5, laxifloræ, 1–1½ uncias longæ, 1–1½ lineas latæ; superiores contiguæ; infirma remota (uno exemplo basi composita); squamæ pellucidæ ciliolatæ, nervo viridi scabro, hispido-mucronatæ. Pedunculi scabri, superiores sensim breviores. Perigynium (vix maturum) 1⅚ lin. longum, ⅞ lin. latum, viride, enervium? apice hispidulum, ciliatum, brevistipitatum, squamam subæquans vel eo paululum longius. Achenium immaturum." (Boott in litt.)

Hab. in sylvaticis prope Columbum, Ohionis, ubi detexit W. S. Sullivant, cum C. pubescente, C. gracillima, etc. vigens. Affinis *C. arctatæ* (C. sylvaticæ, auct. Amer.) ex cl. Boott. — In exemplis nuperrime receptis, perigynia satis matura sunt ovato-elliptica, lata, compresso-plana, enervia (marginibus exceptis), apice vix rostrata.

more feet in length. These runners arise not only from the base of the stem, but from the axils of the upper leaves, and very frequently from the apex of the weak ascending raceme itself, which is thus prolonged into a leafy stolon, hanging down into the water or mud, where it takes root. Its habit and appearance are so unlike even the summer state of our northern *C. rhomboidea*, that we could not hesitate to consider it a distinct species. The subjoined diagnostic character will doubtless suffice for its discrimination.[1]

On the 7th of July we started for the high mountains farther south, having hired a cumbrous and unsightly, but convenient tilted wagon, with a pair of horses and a driver (who rode one of the horses, according to the usual custom of this region), for the conveyance of our luggage, and which afforded us, at intervals, the luxury of reposing on straw at the bottom, while we were dragged along at the rate of two or three miles an hour.

Our first day's journey of about twenty-four miles was somewhat tedious, as we found no new plants of any interest. We saw, however, a variety of *Lonicera parviflora?* with larger leaves and flowers than ordinary, the latter dull purplish; probably the *Caprifolium bracteosum*, var. *floribus violaceo-purpureis* of Michaux. The following morning we reached the Watauga River (a tributary of the Holston); and leaving our driver to follow up the banks of the stream to the termination of the road at the foot of the Grandfather, we ascended an adjacent mountain, called Hanging-rock, and reached our quarters for the night by a different route. The fine and close view of the rugged Grandfather amply rewarded

[1] *Cardamine rotundifolia* (Michx.) : glaberrimer decumbens, stolonibus repentibus, radice fibrosa, foliis omnibus conformibus (radicalibus sæpe trisectis, segmentis lateralibus parvis), petiolatis rotundatis plerumque subcordatis integriusculis vel repando sinuatis, siliquis parvis stylo subulatis, stigmate minuto, seminibus ovalibus. *C. rotundifolia*, Michx., Fl. 2, p. 30 ; Hook., Bot. Misc., 3, t. 109, (statu vernali : in exemplis Carolinianis folia caulinia magis petiolata ;) Darlingt., Fl. Cest., ed. 2, p. 384. *C. rotundifolia*, Torr. & Gray, Fl. N. Amer., i. p. 88.

Hab. in rivulis fontibusque opaculis montium Carolinæ, Virginiæ, Kentucky, et in Pennsylvania.

the toil of ascending this beetling cliff, where we also obtained the *Geum* (*Sieversia*) *radiatum*, probably the most showy species of the genus. The brilliant golden flowers have a disposition to double, even in the wild state, in which we often found as many as eight or nine petals. This tendency would doubtless be fully developed by cultivation. Around the base of these mountains we saw *Blephilia nepetoides*, and another Labiate plant not yet in flower, which we took for *Pycnanthemum montanum*, Michx.

The next day (July 9th) we ascended the Grandfather, the highest as well as the most rugged and savage mountain we had yet attempted; although by no means the most elevated in North Carolina, as has generally been supposed.[1] It is a sharp and craggy ridge, lying within Ashe and Burke counties, very near the northeast corner of Yancey, and cutting across the chain to which it belongs (the Blue Ridge) nearly at right angles. It is entirely covered with trees except where the rocks are absolutely perpendicular; and towards the summit, the Balsam Fir of these mountains, *Abies balsamifera*, partly, of Michaux's Flora (but not of the younger Michaux's Sylva), the *A. Fraseri*, Pursh, prevails, accompanied by the *Abies nigra* or Black Spruce. The earth, rocks, and prostrate decaying trunks, in the shade of these trees, are carpeted with mosses and lichens; and the whole present the most perfect resemblance to the dark and sombre forests of the northern parts of New York and Vermont, except that the trees are here much smaller. The resemblance extends to the whole vegetation; and a list of the shrubs and herbaceous plants of this mountain would be found to include a large portion of the common plants of the extreme northern States and Canada.[2] Indeed the vegetation is essentially Canadian,

[1] According to Professor Mitchel's barometrical measurements, the Grandfather attains the altitude of five thousand five hundred and fifty-six feet above the level of the sea; the Roan, six thousand and thirty-eight feet; and the highest peak of the Black Mountain, six thousand four hundred and seventy-six feet, which exceeds Mount Washington in New Hampshire (hitherto accounted the highest mountain in the United States) by more than two hundred feet.

[2] Among the northern species which we had not previously observed

with a considerable number of peculiar species intermixed. Under the guidance of Mr. Levi Moody we followed the Watauga, here a mere creek, for four or five miles along the base of the Grandfather, until we reached a ridge which promised a comparatively easy ascent. In the rich soil of this ridge, at an elevation of about four hundred feet above the Watauga, we found one of the plants which of all others we were desirous of obtaining, namely, *Carex Fraseriana*. Mr. Curtis had made diligent but ineffectual search for this most singular and rarest of Carices, "along the Catawba near Morganton," and "near Table Mountain," where Fraser is said to have discovered it; and we believe that no subsequent botanist has ever met with it, except Mr. Kin, whose specimen in Muhlenberg's herbarium is merely ticketed "Deigher walli in der Wilternus." Muhlenberg assigns the habitat, "Tiger Valley, Pennsylvania;" but Kin probably obtained his plant in Tygart's Valley, Virginia, a secluded vale among the western ranges of the Alleghanies (in Randolph County), not far from Greenbrier Mountains, and other localities visited by this collector, as his tickets prove. Kin cultivated the plant for some time at Philadelphia, where it was seen by several botanists, and among them by Pursh, who took it for the *Mapinia sylvatica* of Aublet; a mistake which he did not discover whilst writing his Flora in Europe, although he had the cultivated *Carex Fraseriana* before him. We were too late for good specimens, but succeeded in obtaining a considerable number with the fruit still adherent. The plant grows in tufts, after the manner of *C. plantaginea;* the evergreen leaves are a foot or more in length, and often an inch and a half in width, with singularly undulate margins; the slender scapes are naked except towards the root, where they are sheathed by the convulate bases of the leaves. To the description of the spike, fruit, etc., we have nothing of any consequence to add.

in this region, we may mention *Carex flexuosa, C. plantaginea, C. scabrata, C. intumescens, Oxalis Acetosella, Streptopus roseus, Viburnum lantanoides,* and *Platanthera orbiculata* in the finest condition, and in greater profusion than we ever before met with this, the most striking of North American *Orchidaceæ.*

Long before we reached the summit we again met with the new Saxifraga,[1] which we had previously gathered on the mountains near Jefferson; but we now found it in great abundance, both in flower and with mature fruit. It grew in the greatest profusion on the dripping face of a rocky precipice near our encampment for the night, on the northwestern side of the mountain, five or six hundred feet beneath the highest summit. The vegetation here is so backward that the *Saxifraga leucanthemifolia* growing on the brow of this precipice was not yet in blossom, and the *Saxifraga erosa*, Pursh, in the wet soil at its base was scarcely out of flower, while at the foot of the mountain it had long since shed its seeds. We were therefore enabled to satisfy ourselves that *S. erosa* belongs to the section Hydatica, and that the *S. Wolleana*, Torr. & Gray, from a mountain near Bethlehem in Pennsylvania, is only a variety of this species. Pursh gathered his plant in Virginia, "out of a run near the road from the Sweet Springs to the Union Springs, five miles from the former." But if this species be the *Robertsonia micranthifolia* of Haworth's Succulent Plants, as is most probable, and consequently the *Aulaxis micranthifolia* of this author's

[1] *Saxifraga Careyana* (spec. nov.): foliis radicalibus longe petiolatis glabris (tenuibus) ovato-rotundis grosse crenato-dentatis basi truncatis vel subcordatis, scapo gracili nudo apice paniculato-cymoso, floribus effusis, pedicillis filiformibus, petalis lanceolato-oblongis sessilibus sepala recurva plus duplo superantibus, carpellis discretis turgidis demum divaricatis calyce liberis. Variat 1, scapo petiolisque glabriusculis : 2, scapo pedicillis nec non pagina foliorum pilis viscosis pubescentibus : 3, scapo foliis aut bracteis foliaceis 1–2 instructo : 4, foliis ovalibus oblongisve, nunc argute dentatis, in petiolum plus minus attenuatis.

Crescit in rupibus humidis opacis altissimorum montium comitatus Ashe, præsertim ad montem Grandfather dictum, alt. 3500–5000 pedes: Junio floret. Herba spithamæa, rarius pedalis. Flores parvi. Petala consimilia, sessilia, subtriplinervia, alba, immaculata. Filamenta subulato-filiformis. Carpella ovoidea, stylis brevibus apiculata, (stigmatibus subincrassatis,) basi vix aut ne vix coalita ad maturitatem per totam suturam ventralem dehiscentia, ut in pleris Saxifragis plus minus apocarpeis. Semina ovalia, striis elevatis denticulatis (per lentem augentem) longitudinaliter notata. Species distinctissima, habitu ad sect. Hydaticam, sed characteribus ad Micrantheum accedens.

subsequent "Enumeration of Saxifragaceous Plants," it must have been introduced into the English gardens by Fraser as early as 1810.[1] We know not how such a common plant could have escaped the notice of Michaux. Under the name of Lettuce the leaves are eaten by the inhabitants as a salad. At this same place we also saw an Umbelliferous plant not yet in flower, which we believe to be *Conioselinum Canadense*, Torr. & Gr. (*Selinum Canadense*, Michx.), a very rare plant in the extreme northern States and Canada, to which we had supposed it exclusively confined. We found plenty of *Cimicifuga Americana*, Michx., but were obliged to content ourselves with specimens not yet in flower, and with vestiges of the last year's fruit. It should be collected in September.

We were also too early in the season for *Chelone Lyoni*, Pursh, which we found in abundance between the precipice mentioned above and the summit of the mountain, with the flower-buds just beginning to appear. Mr. Curtis remarks that Mr. Nuttall could not have met with this exclusively mountain flower near Wilmington; and also, that the *C. Lyoni* of Pursh and the *C. latifolia* of Muhlenberg and Elliott, are doubtless founded upon one and the same species. Both, indeed, are said to have been collected by Lyon, and the leaves vary from ovate-lanceolate or oval with an acute base, to ovate with a rounded but scarcely cordate base. Pursh's character is drawn from a cultivated specimen. Here we again met with the *Aconitum* previously observed in similar situations on the Negro Mountain, and which, being then only in bud, we took for the *A. uncinatum*, a species collected in this region by Michaux, and recently by Mr. Curtis and other botanists. We were greatly surprised, therefore, to find that

[1] The only important discrepancy respects Haworth's character, "Corolla irregularis, petalis 2 inferioribus elongatis divaricantibus gracilioribus," and "Flores albi, rubro minute punctati"; while the petals in our plant are very nearly equal and similar, and pure white, except the yellow spot at the base. *Aulaxis nuda*, Haworth, *l. c.* (of unknown origin), appears to be the more ordinary and nearly glabrous form of this species. Mr. Don's description of *S. erosa*, probably drawn from the cultivated plant, also differs from our plant in several minor points.

our plant, here just coming into blossom, had cream-colored flowers, very different from those of *A. uncinatum*, and more nearly resembling those of *A. Lycoctonum*.[1] On our return to Jefferson, we obtained good specimens at our original locality, where it is very abundant. The weak stems at first ascending, become prostrate when the plant is in flower, and frequently attain the length of seven or eight feet. As the stem does not climb, and its flowers are so different from those of *A. uncinatum*, it can hardly be the plant mentioned by Pursh under that species, which he saw at the foot of the Peaks of Otter and about the Sweet Springs in Virginia. It may be remarked that the ovaries of *A. uncinatum* are often nearly glabrous, and the claws of the petals entirely so. The seeds are strongly plicate-rugose, with a wing-like margin on one side.

Near the summit of the mountain we saw immense quantities of a low but very large-leaved Solidago, not yet in flower, which I take to be the *S. glomerata* of Michaux, who could not have failed to observe such a conspicuous and abundant plant, especially as it must have been in full blossom at the

[1] *Aconitum reclinatum* (spec. nov. § Lycoctonum): caule elongato decumbente foliisque palmatifidis glabris, lobis divaricatis cuneatis apicem versus incisis, racemis paniculisve divergentibus laxifloris (floribus albidis), bracteolis minimis, galea horizontali conico-cylindracea ore obliquo, labio cucullorum obcordato ab ungue distante, calcare adunco, filamentis edentulis, carpellis glabris 2–4–spermis, seminibus (immaturis) squamoso-rugosis.

Hab. in opacissimis sylvis ad montes Negro Mountain et Grandfather dictos, alt. 4000–5000 pedes. Julio-Augusto floret. Caulis flaccidus, adscendens vel declinatus, denique procumbens, 3–8 pedalis, ramis gracilibus, seu paniculis laxifloris, divaricatis. Folia flaccida; inferiora longe petiolata (circumscriptione suborbiculari), profunde 5-7 fida; segmentis interdum 2-3 lobatis apice incisodentatis dentibus mucronatis; summa subsessilia, 3–5 partita; venis et pagina quandoque superiori tenuissime pubescentibus. Pedicelli sparsi (pedunculique puberuli) flore longiores, bracteolis 2–3 minimis stipati. Flores minores quam in *A. Lycoctono*, albi vix flavidis tincti (in siccis leviter purpurascentes); sepalis intus pilis aureis barbatis. Galea primum adscendens, mox horizontalis, rostello brevi rectiusculo. Unguis petalorum medium cuculli adfixus; saccus angustus, ore valde obliquo in labium obcordatum expanso. Ovaria tria, 4–6–ovulata.

time he ascended this mountain. It does not, however, altogether accord with Michaux's description, nor does that author notice the size of the heads, which in our plant are among the largest of the genus. Specimens in flower were procured by Mr. Curtis, who visited this mountain at a more favorable season. With the latter we found a Geum, which Mr. Curtis had formerly observed on the Roan Mountain (where we afterwards met with it in great abundance), and referred, I think correctly, to *G. geniculatum*, Michx., although that species is said to have been collected in Canada. The lower portion of the style is less hairy in our specimens than in Michaux's plant, a difference which, if constant, is not perhaps of specific importance. In the subjoined character I have supplied an inadvertent omission in the "Flora of North America," where the sessile head of carpels, which so readily distinguishes this species from *G. rivale*, is not mentioned.[1] Here we again found *Vaccinium erythrocarpum*, as already mentioned; and obtained beautiful flowering specimens of *Menziesia globularis*, a straggling shrub which in this place attains the height of five or six feet.

[1] *Geum geniculatum* (Michx.): capitulo carpellorum sessili, articulo styli superiore plumoso inferiorem pubescentem excedente, achenio hirsuto petalis cuneato-obovatis (nunc emarginatis aut leviter obcordatis) exunguiculatis calycem æquantibus; floribus mox erectis.

β *Macreanum*, articulo styli inferiore sursum glabrescente. *G. Macreanum*, M. A. Curtis, in litt.

Crescit in Canada ex Michaux: an recte? Var. β in umbrosis ad montes Grandfather et Roan, Carolinæ Septentrionalis, alt. 5500–6000 pedes, ubi imprimis detexit cl. Curtis. Julio floret. Caulis 2–3-pedalis, gracilis, foliosus, inferne pilis rigidiusculis retrorsis, superne pilis mollibus patentibus crebrioribus villosus. Folia membranacea; radicalia nunc palmatim 3-secta, nunc interrupte pinnatisecta, haud rariusque indivisa vel sublobata in eodem stirpe; caulinia trisecta trilobatave, lobis acutis; superiora sessilia. Flores minores et numerosiores quam in *G. rivali*; petala albida, venis purpurascentibus. Styli pars inferior portione plumosa primum multo, postremum modice brevior, in exemplo Michx. manifeste, at juxta apicem parce piloso-pubescens; in var. β superne glabrata.

Should the Carolina plant hereafter prove to be a distinct species, it will of course retain the name proposed by Mr. Curtis, in honor of his friend and former associate in botanical labors, Dr. James F. McRee of Wilmington, North Carolina.

The only unwooded portion of the ridge which we ascended, an exposed rock a few yards in extent, presents a truly Alpine aspect, being clothed with lichens and mosses, and with a dense mat of the mountain Leiophyllum, a stunted and much branched shrub (five to ten inches high), with small coriaceous leaves greatly resembling *Azalea procumbens*.[1] The much denser growth, and the broader, more petiolate, and perhaps uniformly opposite leaves, as well as the very different habitat, would seem to distinguish the mountain plant from the *L. buxifolium* of the Pine Barrens of New Jersey, etc.; but although I think the learned De Candolle has correctly separated the former, under the name of *L. serpyllifolium* (*Ledum serpyllifolium*, L'Her. ined.), it is not easy to find sufficient and entirely constant distinctive characters; since the sparse scabrous puburluence of the capsule may also be observed upon the ovary of the low-country plant, in which the leaves are likewise not unfrequently opposite; and no reliance can be placed on the length of the pedicels. The synonymy requires some correction; the *Ledum buxifolium* of Michaux ("in summis montibus excelsis Carolinæ"), and of Nuttall, so far as respects the plant which "is extremely abundant on the highest summits of the Catawba Ridge," (that is, on Table Mountain,) as well as the *Leiophyllum buxifolium* of Elliott (from the mountains of Greenville district, South Carolina), must be referred to *L. serpyllifolium*, DC. We were too late to obtain the plant in blossom, excepting one or two straggling specimens; but we were so fortunate as to obtain a few flowering specimens of *Rhododendron Catawbiense*.

I should have remarked, that so much time was occupied in the ascent of this mountain as nearly to prevent us from herborizing around the summit for that day; since we had to descend some distance to the nearest spring of water, and prepare our encampment for the night. The branches of the

[1] We are confident that the latter does not grow on the Grandfather Mountain, as is stated by Pursh, on the authority of a specimen collected by Lyon; and have little doubt that he mistook for it this species of Leiophyllum. Vide Pursh, "Flora Amer. Sept." i. pp. 154, 301.

Balsam afforded excellent materials for the construction of our lodge; the smaller twigs with large mats of moss stripped from the rocks furnished our bed, and the dead trees supplied us with fuel for cooking our supper and for the large fire we were obliged to keep up during the night. We re-ascended the summit the next morning, and devoted several hours to its examination; but the threatening state of the weather prevented us from visiting the adjacent ridges, or the southern and eastern faces of the mountain, and we were constrained to descend towards evening to the humble dwelling of our guide, which we reached before the impending storm commenced.

Our next excursion was to the Roan Mountain, a portion of the elevated range which forms the boundary between North Carolina and Tennessee, distant nearly thirty miles southwest from our quarters at the foot of Grandfather by the most direct path, but at least sixty by the nearest carriage road. We traveled for the most part on foot, loading the horses with our portfolios, paper, and some necessary luggage, crossed the Hanging-rock Mountain to Elk Creek, and thence over a steep ridge to Cranberry Forge, on the sources of Doe River, where we passed the night. On our way we cut down a Service-tree (as the *Amelanchier Canadensis* is here called), and feasted upon the ripe fruit, which throughout this region is highly, and indeed justly prized, being sweet with a very agreeable flavor; while in the northern States, so far as our experience goes, this fruit, even if it may be said to be edible, is not worth eating. As "Sarvices" are here greedily sought after, and are generally procured by cutting down the trees, the latter are becoming scarce in the vicinity of the "plantations," as the mountain settlements are universally called. Along the streams we met with the mountain species of Andromeda (Leucothoë), doubtless Pursh's *A. axillaris;* but whether the original *A. axillaris* of the "Hortus Kewensis" pertains to this or to the species of the low country, I cannot at this moment ascertain. A portion of Pursh's character seems also to belong to the low country rather than to the mountain species, and the two are by no

means clearly distinguished in subsequent works. The leaves in our specimens are oblong-lanceolate, finely acuminate, the margins closely beset throughout with spinulose-setaceous teeth; and the rather loose spicate racemes (the corolla having fallen) are nearly half the length of the leaves.

Hitherto we had searched in vain for the *Astilbe decandra;* but we first met with this very interesting plant in the rich and moist mountain woods between Elk Creek and Cranberry Forge, and subsequently in similar situations, particularly along the steep banks of streams, quite to the base of the Roan. Mr. Curtis found it abundantly near the sources of the Linville River, and at the North Cove, where it could not have escaped the notice of Michaux; and it is doubtless the *Spiræa Aruncus* var. *hermaphrodita* of that author. It indeed greatly resembles *Spiræa Aruncus*, and at a distance of a few yards is not easily distinguished from that plant, but on a closer approach the resemblance is much less striking. Michaux appears to have been the original discoverer of this plant, and from him the specimens cultivated in the Malmaison Garden, and described by Ventenat under the name of *Tiarella biternata*, were probably derived. It was afterwards collected by Lyon,[1] and described by Pursh from a specimen cultivated in Mr Lambert's garden at Boynton. We noticed a peculiarity in this plant, which explains the discrepancy between Ventenat and Pursh (the former having figured it with linear-spatulate petals, while the latter found it apetalous), and perhaps throws some additional light upon the genus. The flowers are diœcio-polygamous, the two forms differing from each other in aspect much as the staminate and pistillate plants of *Spiræa Aruncus*. In one form, the filaments are exserted to twice or thrice the length of the calyx, and the spatulate-linear petals, inconspicuous only on account of their narrowness, are nearly as long as the stamens; the ovaries

[1] Muhlenberg's specimen was also received from Lyon. The only habitat cited in this author's catalogue is Tennessee, and we ourselves collected it within the limits, as well as on the borders of that State. The late Dr. Mcbride found it in South Carolina, near the sources of the Saluda.

are well formed and filled with ovules, which, however, so far as I have observed, are never fertilized; and the stigmas are smaller than in the fertile plant, and not papillose. In the other or fertile form, both the stamens and the petals are in an abortive or rudimentary state, and being shorter than the sepals, and concealed by them in dried specimens, are readily overlooked; the stigmas are large, truncate, and papillose, and a portion of the ovules become fertile. The Japanese species (*Hoteia Japonica*, Morr. & Decaisne, the *Spiræa Aruncus* of Thunberg) appears to have uniform and perfect flowers;[1] but the species from Nepal (*Astilbe rivularis*, Don, the *Spiræa barbata* of Wallich, but not of Lindley) is probably polygamo-diœcious, like our own species; at least, the flowers are apetalous in a fragment given me by Professor Royle, and the stamens mostly equal in number to the sepals. I have no doubt that these three species belong to a single and very natural genus, for which the name of Astilbe must be retained; for I see neither justice nor reason in superseding the prior name, as suggested by Endlicher,[2] on account of the incompleteness of the character, which correctly describes one state, at least, of the plant intended, by the subsequent Hoteia, the character of which is equally incomplete, when applied to the whole genus. The number of genera which are either divided between North America, Japan, and the mountain region of central Asia, or have nearly allied species in these countries or in the two former, is very considerable: in other cases a North American genus is replaced by a nearly allied one in Japan, etc., as Decumaria by Schizophragma, Schizandra by Sphærostemma, Hamamelis by Corylopsis, etc. I have elsewhere alluded to this subject and shall probably consider it more particularly on some future occasion.

Our next day's journey was from Cranberry Forge to Crab

[1] "Flores in meo Japonico specimine omnes inveni hermaphroditos, nec ullos polygamos." (Thunberg, "Flora Japonica," p. 212, sub *Spiræa Arunco*.)

[2] "Si, quod nunc asserunt auctores, Hoteia et Astilbe, Don, revera plantæ congeneres, posterius incomplete ad auctore suo descriptum supprimendum, et prius egregie stabilitum servandum erit." (Endlicher, "Genera," Suppl. p. 1416.)

Orchard on Doe River, in Tennessee, and up Little Doe River to Squire Hampton's, where we took a guide and ascended the Roan. While ascending the Little Doe River, about three miles from the junction with the large stream of that name, at one of the numerous places where the road crosses this rivulet, we again met with *Carex Fraseriana*. The plant did not appear to be so abundant in this Tennessee locality as at the Grandfather, but it is doubtless plentiful on the mountain side just above. We ascended the north side of the Roan, through the heavy timbered woods and rank herbage with which it is covered; but found nothing new to us excepting *Streptopus lanuginosus*, in fruit, and among the grove of *Rhododendron maximum* towards the summit, we also collected *Diphyscium foliosum*, a moss which we had not before seen in a living state. In more open moist places near the summit, we found the *Hedyotis* (*Houstonia*) *serpyllifolia*, still beautifully in flower, and the *Geum geniculatum*, which we have already noticed. It was just sunset when we reached the bald and grassy summit of this noble mountain, and after enjoying for a moment the magnificent view it affords, had barely time to prepare our encampment between two dense clumps of *Rhododendron Catawbiense*, to collect fuel, and make ready our supper. The night was so fine that our slight shelter of Balsam boughs proved amply sufficient; the thermometer, at this elevation of about six thousand feet above the level of the sea, being 64° Fahr. at midnight, and 60° at sunrise. The temperature of a spring just under the brow of the mountain below our encampment we found to be 47° Fahr. The Roan is well characterized by Professor Mitchell as the easiest of access and the most beautiful of all the high mountains of that region. "With the exception of a body of (granitic) rocks, looking like the ruins of an old castle, near its southwestern extremity, the top of the Roan may be described as a vast meadow (about nine miles in length, with some interruptions, and with a maximum elevation of six thousand and thirty-eight feet), without a tree to obstruct the prospect; where a person may gallop his horse for a mile or two, with Carolina at his feet on one side,

and Tennessee on the other, with a great ocean of mountain raised into tremendous billows immediately about him. It is the pasture ground for the young horses of the whole country about it during the summer. We found the strawberry here in the greatest abundance and of the finest quality, in regard to both size and flavor, on the 30th of July." [1]

At sunrise we had fine weather and a most extensive view of the surrounding country. In one direction we could count from eight to twelve successive ranges of mountains, and nearly all the higher peaks of this whole region were distinctly visible. Soon, however, we were enveloped in a dense fog which continued for several hours, during which time we traversed the southwestern summit and made a list of the plants we saw. The herbaceous plants of this bald and rounded summit are chiefly *Aira flexuosa*, *Juncus tenuis*, *Carex intumescens*, *festucacea*, *æstivalis* of Mr. Curtis, and a narrow-leaved variety of *C. Pennsylvanica*, the latter constituting the greater part of the grassy herbage, *Luzula campestris*, *Lilium Philadelphicum* and *Canadense*, which here only attain the height of from four to eight inches, *Sisyrinchium anceps*, *Smilacina bifolia*, *Habenaria* (*Platanthera*) *peramœna*, *Veratrum viride*, *Helonias* (*Chamælirium*) *dioica*, *Osmunda Claytoniana*, Linn. (*O. interrupta*, Michx.), *Athyrium asplenioides*, *Pedicularis Canadensis*, mostly with purplish-brown flowers, now just in blossom, *Trautvetteria palmata*, *Ranunculus repens*, *Thalictrum dioicum* just in flower, *Geum radiatum* in the greatest profusion (it was here that Michaux obtained this species), *Potentilla tridentata* and *Canadensis*, *Fragaria Virginiana*, the fruit just ripe and of the finest flavor, *Rubus villosus* now in flower, *Castilleja coccinea*, *Geranium maculatum*, *Clematis Viorna* about eight inches high, *Sanicula Marilandica*, *Zizia aurea*, *Heracleum lanatum*, *Hypericum corymbosum*, with larger flowers than usual, a more upright and larger-leaved variety of *Hedyotis serpyllifolia*, *Œnorthera glauca β*, *Senecio*

[1] Professor Mitchell of the Chapel Hill University, in the "Raleigh Register" of November 3, 1835, and in the "American Journal of Science and Arts" for January, 1839.

Balsamitæ, *Rudbeckia triloba*, and a dwarf variety of *R. laciniata*, *Liatris spicata*, *Cacalia atriplicifolia*, *Cynthia Virginica*, *Aster acuminatus*, *Solidago bicolor*, *S. spithamea*, (Curtis in Torr. & Gray, Fl. ined.,) a very distinct dwarf species, *S. Curtisii*, Torr & Gr. *l. c.*, not yet in flower, and *S. glomerata* in the same state as at Grandfather Mountain; also *Saxifraga leucanthemifolia*, *Sedum telephioides*, *Heuchera villosa*, *Polypodium vulgare*, the dwarf variety of *Hedyotis purpurea* previously noticed, *Scirpus cæspitosus*, and *Agrostis rupestris!* which are confined to the rocky precipice already mentioned. The only tree is *Abies Fraseri*, a few dwarf specimens of which extend into the open ground of the summit; and the following are all the shrubs which we observed, namely, *Diervilla trifida*, *Menziesia globularis*, *Vaccinium erythocarpum*, *Rhododendron Catawbiense*, forming very dense clumps, *Leiophyllum serpyllifolium*, *Sorbus Americana*, two to four feet high, *Cratægus punctata* only a foot in height, *Pyrus arbutifolia* var. *melanocarpa*, *Ribes rotundifolium;* and a low and much branched species of Alder, which Mr. Curtis proposes to call *Alnus Mitchelliana*, in honor of Professor Mitchell; but we fear it may prove to be a variety of what we deem the *A. crispa*, Ait. from the mountains of New York, New Hampshire, etc., and Newfoundland, although it has more rounded leaves with the lower surface nearly glabrous, except the primary veins; while in the former (to which the names of *A. crispa* and *A. undulata* are not very appropriate), the leaves are often, but not always, somewhat velvety-pubescent beneath. To our list must be added any apparently undescribed species of Vaccinium, first noticed by Mr. Constable.[1] We made a hasty

[1] *Vaccinium Constablæi* (spec. nov.): pumilum, foliis deciduis ovalibus pallidis subtus glaucis reticulato-venosisque glanduloso-mucronatis integerrimis vel obsoletissime serrulatis ciliatis, racemis brevissimis sessilibus, bracteis squamaceis parvis caducis, corollis brevissime cylindricis, antheris inclusis muticis, ovariis 10-locularibus, loculis pluri-ovulatis.

In summo jugo "Roan Mountain" dicto (Tennessee et Carolina Septentrionali), ad alt. 6000 pedes. Julio floret. — Frutex 1-3-pedalis, erectus, ramis griseo-viridibus teretis. Folia sesqui-biuncialia, lato-ovalia vel elliptica, utrinque sæpius acuta, glabra, nisi costa supra puberula

visit to the other principal summit, where we found nothing that we had not already collected, excepting *Arenaria glabra*, Michx., and descended partly by way of the contiguous Yellow Mountain.

Retracing our steps, we returned the next day to the foot of Grandfather, and reached our quarters at Jefferson the second day after. We had frequently been told of an antidote to the bite of the Rattlesnake and Copperhead (not unfrequent throughout this region), which is thought to possess wonderful efficacy, called Thurman's Snake-root after an "Indian Doctor," who first employed it; the plant was brought to us by a man who was ready to attest its virtues from his personal knowledge, and proved to be the *Silene stellata*! Its use was suggested by the markings of the root beneath the bark, in which these people find a fancied resemblance to the skin of the Rattlesnake. Nearly all the reputed antidotes are equally inert; such herbs as *Impatiens pallida*, etc., being sometimes employed; so that we are led to conclude that the bite of these reptiles is seldom fatal, or even very dangerous, in these cooler portions of the country.

About the foot of the Roan and Grandfather we obtained

et margines ciliati, subsessilia, infra saturate glauca. Racemi 5-10-flori, sæpe corymbosi, ad apicem ramulorum anni præcedentis solitarii vel aggregati. Baccæ immaturæ cæruleæ, glaucæ, limbo calycis majusculo coronatæ, decem- (nunc abortu quinque ?) loculares; loculis pleio (3-6 ?) spermis.

Professor Dunal (in DC. Prodr. 7, p. 566) notices as an extraordinary exception to the character of Vaccinium, a species with an 8-10-celled fruit and a single (?) seed in each cell. The first-named character is not unfrequent in the genus: several of the more common species which I have cursorily examined, exhibit a more or less completely 8-10-celled ovary, but with many ovules in each cell. There is a small group, however [*Decachæna*, Torr. & Gray ined.], presenting a different structure, which is best exemplified in *V. resinosum*, Ait. The 10 carpels of this species, inclosed in the baccate calyx, are very slightly coherent with each other, and become crustaceous or bony nuts, each containing a single ascending seed. The same is the case in what I take to be *V. dumosum* and *V. hirtellum*; and probably in some other species which have the leaves sprinkled with resinous dots. *V. frondosum*, Willd. (which is the *V. decamerocarpon* of Dunal), is similar in structure, except that the carpels appear to be more coherent and less indurated.

a few specimens of *Pycnanthemum montanum*, Michx. (Monardella, Benth.) just coming into blossom. Our plant accords with Michaux's description, except that there are frequently two or even three axillary heads besides the terminal one. The flowers have altogether the structure of Pycnanthemum, and the upper lip of the corolla is entire; so that it cannot belong to Monardella, although placed as the leading species of that genus. As to the species from which Mr. Bentham derived the generic name (*Pycnanthemum Monardella*, Michx.), I am by no means certain that it belongs either to Pycnanthemum or Monardella. The specimen in the Michauxian herbarium is not out of flower, as has been thought, but the inflorescence is undeveloped, and perhaps in an abnormal state. In examining a small portion taken from the head, I found nothing but striate-nerved bracts, obtuse and villous at the apex, and abruptly awned; the exterior involucrate and often lobed; the innermost linear, and tipped with a single awn. The aspect of the plant, also, is so like *Monarda fistulosa*, that I am strongly inclined to think it a somewhat monstrous state of that, or some nearly allied species; in which case, the genus Monardella should be restricted to the Californian species. Pursh's *P. Monardella*, I may observe, was collected beneath the Natural Bridge in Virginia, where we also obtained the plant, and subsequently met with it throughout the mountains. It is certainly a form of *Monarda fistulosa*, according to Bentham's characters, but the taste is much less pungent, the throat of the calyx less strongly bearded than is usual in that species, and the corolla nearly white. We thought it probably a distinct species; but these differences may be owing to the deep shade in which it commonly occurs. The *P. Monardella* of Elliott, according to his herbarium, is identical with that of Pursh. We collected in Ashe County several other species of Pycnanthemum, and in the endeavor to discriminate them, we encountered so many difficulties that I am induced to give a revision of the whole genus.

Some additional plants were obtained around Jefferson which were not previously in blossom, such as *Campanula*

divaricata ; Cacalia reniformis ; Silphium perfoliatum ; the larger form of *Coreopsis auriculata*, with nearly all the leaves undivided; the glabrous and narrow-leaved variety of *C. senifolia* (*C. stellata*, Nutt.) which alone occurs in this region; *Melanthium Virginicum*, which is a very handsome plant, with the flowers cream-colored when they first expand; and *Stenanthium angustifolium*, Gray, which is doubtless the *Helonias graminea* of the "Botanical Magazine." We also made an excursion to the White Top in Virginia, twenty miles northwest from Jefferson; a mountain of the same character as the Roan, but on a smaller scale, and with the pasturage of its summit more closely fed. We were not rewarded, however, with any new plants, and the cloudy weather obscured the prospect, which is said to be very extensive. On our return, we found *Cedronella cordata*, Benth., nearly out of flower, with runners often two or three feet in length. Mr. Bentham has omitted to mention the agreeable balsamic odor of the genus, which in our plant is much less powerful than in *C. triphylla*. We saw plenty of *Cimicifuga Americana*, but the flowers were still unexpanded. Our endeavors to obtain the fruit of *Cimicifuga cordifolia* (common in this region) were likewise unsuccessful; without which it is not always easy to distinguish this species from *C. racemosa*. The leaflets of the former are frequently very large, the terminal ones resembling the leaves of the Vine in size and shape, as remarked by De Candolle; in one instance we found them ten inches in diameter; but they are generally much smaller and more divided, apparently passing into the former species. The number of the ovaries does not afford marked characters, since the lowest flowers of *C. racemosa* sometimes present two, while the upper ones of *C. cordifolia* are almost always monogynous.

We were too early in the season for several interesting plants, especially *Compositæ*, and did not extend our researches far enough south to obtain many others; such as *Hudsonia montana*, which appears to be confined to Table Mountain, *Rhododendron punctatum*, *Stuartia pentagyna*, *Philadelphus hirsutus*, *Silene ovata* (which Mr. Curtis found

in Buncombe and Haywood counties), *Berberis Canadensis* (which, however, Pursh collected on the mountains of Greenbrier in Virginia), *Parnassia asarifolia* (which according to Mr. Curtis first appears in Yancey County, but Pursh procured it from "mountain runs on the Salt Pond Mountain, Virginia, and on the top of the Alleghanies near Christianburg"), and, above all, the new Thermopsis; (*T. Caroliniana*, M. A. Curtis, MSS.) recently discovered by our friend Mr. Curtis, in Haywood and Cherokee counties. We were likewise unsuccessful in our search for a remarkable undescribed plant, with a habit of Pyrola and the foliage of Galax, which was obtained by Michaux in the high mountains of Carolina. The only specimen extant is among the "Plantæ incognitæ" of the Michauxian herbarium, in fruit only; and we were anxious to obtain flowering specimens, that we might complete its history; as I have long wished to dedicate the plant to Professor Short of Kentucky, whose attainments and eminent services in North American botany are well known and appreciated both at home and abroad.[1]

We left this interesting region near the end of July, returning to New York by way of Raleigh, Richmond, etc.; and found a marked change in the vegetation immediately on crossing the Blue Ridge. I cannot extend these remarks to the plants observed in our homeward journey, except to mention that the Schrankia of this part of the country, which extends to the eastern slope of the Blue Ridge, is the *S. angustata*, Torr. & Gray; at least we observed no other species.

[1] *Shortia*, Torrey & Gray. Calyx quinquesepalus; sepala imbricata, squamacea, striata, persistentia, exteriora ovata, interiora oblonga. Corolla — Stamina — Capsula calyce brevior, subglobosa, stylo filiformi (subpersistente) superata, trilocularis, loculicide trivalvis, valvis medio septiferis placenta centrali magna persistente. Semina multa, parva; testa nucleo conformis. Embryo teres, rectiusculus, albumine brevior. Herba cæspitosa? subaculis, perenni, glabra; foliis longe petiolatis, rotundatis, subcordatis, apice nunc retusis, crenato-serratis, crenaturis mucronatis; scapis unifloris, nudis, apicem versus squamoso-bracteatis.

S. galacifolia, Torrey & Gray. (V. spec. sicc. in herb. Michx., cum schedula, "Hautes montagnes de Carolinie. An Pyrola spec.? an genus novum?")

This is doubtless the *S. uncinata* of De Candolle; but not, I think, of Willdenow. I may here remark, that the reticulate-leaved species (*S. uncinata*, Torr. & Gr.) is the Leptoglottis of De Candolle ("Mem Legum"), as I have ascertained from a fragment of the original specimen in the rich herbarium of Mr. Webb, which that gentleman obligingly sent me; but I find no neutral flowers or sterile filaments in the numerous specimens of this plant, from different localities, which I have from time to time examined.

THE LONGEVITY OF TREES.

THE "Histoire des Arbres Forestières de l'Amérique Septentrionale"[1] of the younger Michaux is chiefly known in this country through the English translation made by Mr. Hillhouse, under the superintendence of the author, who added some new plates, and information not contained in the French edition. It was published in Paris in the year 1819. We have no intention of formally reviewing, at this late day, a work of such long-established reputation as the Sylva of Michaux. It has been the standard treatise upon the subject ever since its publication; and it well deserves the rank it holds. We wish rather to offer our grateful acknowledgments to the memory of the late Mr. William Maclure for his liberal endeavors to render this important and quite expensive work more generally accessible in the country, the noble forests of which it is designed to illustrate. In furtherance of this object, Mr. Maclure, if we are rightly informed, purchased in Paris the copies which remained unsold the year after its publication, and sold them in the United States at a very reduced price. With liberal forethought, he bought also the original copperplates of this book, and of several other expensive works of science and art; intending to have them reprinted in this country in a cheaper form, so as to insure them a wider circulation.[2] During the last twelve years of his life, or from the time of the total failure of the "great social experiment" made at New Harmony, Indiana, by the celebrated Robert Owen and himself, down to his decease in 1840, Mr. Maclure resided in the city of Mexico. The

[1] "North American Review," July, 1844.

[2] Among these are the complete copperplates of Veillot's "Histoire Naturelle des Oiseaux de l'Amérique Septentrionale"; and Audebert's "Histoire Naturelle des Oiseaux," etc.

great interest which he still felt in the advancement and diffusion of scientific knowledge in this country, was manifested by his continued and munificent benefactions to the Academy of Natural Sciences at Philadelphia.[1] It was his desire also that some competent person, at his expense, should collect additional information respecting the forest trees of North America, and prepare a new and augmented edition of Michaux's Sylva. Although this generous plan was not carried into execution in Mr. Maclure's lifetime, we understand that he left testamentary instructions for its accomplishment, or at least for the reprint of Michaux's original work.

Mr. Maclure's munificent intentions are — fulfilled, shall we say? by the edition now before us, printed at New Harmony, Indiana, upon wretched, flimsy, whity-brown paper, of texture scarcely firm enough to receive the impression of the worn-out type, which seems to have done long service in the columns of some country newspaper. The engravings, to do the work full justice, are very good, at least in the colored copies; being impressions from the original plates imported by Mr. Maclure, and colored after the French edition. The subscription price was high enough to pay for the best typography and paper; and so popular a work would surely have found a ready sale at any reasonable price, as the Paris edition had been long out of print. We are bound to suppose that the testamentary instructions of the late Mr. Maclure have been literally fulfilled. How far his generous intentions have been answered, or to what extent defeated, by the wretched character of the reprint, are questions which we shall not attempt to answer, except by stating that, far from having obtained the wide circulation which Mr. Maclure desired, these volumes have entirely disappeared from the shelves of our booksellers, where, so far as we can learn,

[1] Besides his invaluable library of nearly 4000 volumes, containing the choicest works in natural history, antiquities, the fine arts, voyages and travels, etc., and many smaller contributions to this flourishing institution, of which he was president for more than twenty years, Mr. Maclure presented to it, in the latter part of his life, the sum of twenty thousand dollars. See Dr. Morton's "Memoir of William Maclure," Philadelphia, 1841.

there was no demand for them; and our diligent attempt to find a single copy in the public and private libraries of three of our largest cities has proved entirely unsuccessful.

The foundation of the North American Sylva was laid by the laborious researches of the elder Michaux; who, under the auspices of the French government, devoted ten years, from 1785 to 1796, to a thorough exploration of the country, from the sunny, sub-tropical groves of Florida to the cold and inhospitable shores of Hudson's Bay; repeatedly visiting nearly all the higher peaks and deepest recesses of the Alleghany Mountains, and extending his toilsome journeys westward to the prairies of Illinois and the banks of the Mississippi. He had formed indeed, and was only prevented by untoward circumstances from executing, a plan — more hardy than we can well conceive at this late day — for ascending the Missouri to its sources, and crossing the mountains into the then untrodden but now litigated country on the Oregon. The curious reader will find an extract from his private diary in the "American Journal of Science and Arts" for January, 1842;[1] showing that he had laid his plans and proposals upon this subject before Mr. Jefferson, who was then Secretary of State. The papers submitted by him may have suggested the scheme of the national expedition of discovery, soon afterwards ordered by Jefferson, and nobly carried into effect by Lewis and Clarke.

Soon after his return to France, and the year before he fell a victim to scientific zeal upon the coast of Madagascar, the elder Michaux published his History of North American Oaks;[2] which may be deemed the nucleus of the more comprehensive work subsequently published by his son. The younger Michaux accompanied his father in the earlier portion of his travels, through South Carolina, Georgia, and Florida; but he afterwards returned to Europe. Revisiting this country in the autumn of 1801, and passing the winter in South Carolina, he traveled, during the following season,

[1] Vol. xlii. p. 7.

[2] "Histoire des Chênes de l'Amérique Septentrionale." Par A. Michaux. Paris, 1801. 1 vol. fol.

from New York, across the mountains in Pennsylvania, to the Ohio, and carefully explored the States of Kentucky and Tennessee; thence, recrossing the mountains in North Carolina to Charleston, he again embarked for France.[1] Again returning in 1807, he journeyed along the whole extent of our Atlantic coast, and visited the principal ports to examine the timber employed in shipbuilding and in workshops of every description; besides making separate excursions into the interior: "the first, along the rivers Kennebec and Sandy, passing through Hallowell, Norridgewock, and Farmington; the second, from Boston to Lake Champlain; the third, from New York to the lakes Ontario and Erie; the fourth, from Philadelphia to the borders of the rivers Monongahela, Alleghany, and Ohio; and the fifth, from Charleston to the sources of the Savannah and Oconee." Having thus faithfully collected the requisite information, his great work upon our forest trees — the fruit of so much labor — was published at Paris in 1810–13.

But this work is not the only result of the well directed industry and zeal of the elder and the younger Michaux. To these two persons, chiefly, are the French plantations indebted for their surpassingly rich collections of American trees and shrubs; which long since gave rise to the remark, as true at this day as it was twenty years ago, that an American must visit France to see the productions of his native forests. When shall it be said that the statement is no longer true? When shall we be able to point to a complete, or even a respectable, American collection of our indigenous trees and shrubs?

A few words will suffice for the second work on our list.[2]

[1] The observations made in this tour are recorded in his "Voyages à l'Ouest de Monts Alléghanys," 8vo, Paris, 1804; and in a "Mémoire sur la Naturalisation des Arbres Forestières de l'Amérique Septentrionale," 8vo, Paris, 1805.

[2] "The North American Sylva"; or a description of the forest trees of the United States, Canada, and Nova Scotia, not described in the work of F. Andrew Michaux, and containing all the forest trees discovered in the Rocky Mountains, the Territory of Oregon down to the shores of the Pacific and into the confines of California, as well as in various parts of the United States; illustrated by 122 fine plates.

Mr. Nuttall, it appears, first arrived in this country the very year that the younger Michaux finally left it. And from that time to the present, no botanist has visited so large a portion of the United States, or made such an amount of observations in the field and forest. Probably few naturalists have ever excelled him in aptitude for such observations, in quickness of eye, tact in discrimination, and tenacity of memory. In some of these respects, perhaps, he may have been equalled by Rafinesque, — and there are obvious points of resemblance between the later writings of the two, which might tempt us to continue the parallel; — but in scientific knowledge and judgment he was always greatly superior to that eccentric individual. Mr. Nuttall has also enjoyed the best opportunities for exploring the wide regions beyond the Mississippi. In 1811, along with Mr. Bradbury, he ascended the Missouri to the Mandan villages 1600 miles above its mouth; and shortly after his return published his extended and most happily executed botanical work, the "Genera of North American Plants." In 1819, at the imminent risk of his life, he ascended the Arkansas to the Great Salt River. And, in 1834, he finally succeeded in crossing the Rocky Mountains by the now well-trodden road along the sources of the Platte, and exploring the territory of Oregon and of Upper California. Mr. Nuttall was therefore peculiarly qualified for the preparation of a supplementary North American Sylva, designed especially to comprise the forest trees of these wide regions, which are now, for obvious reasons, attracting particular attention.

The work, according to the announcement on the title-page, will consist of three volumes; and we understand that the editor committed the whole manuscript to the publisher's hands more than two years ago, when he returned to his native country, to take possession of an ample family inheritance. But from some cause, the publication has been greatly delayed; only one volume having yet appeared, and that in two portions, of which the first bears the date of 1842, while the second has but just reached us. We postpone all critical remarks until the entire publication is completed; merely ob-

serving, lest the interests of the publisher should suffer from nominal connection between this work and the New Harmony reprint of the original Sylva, that the paper and typography are good, and the plates, which are colored lithographs, are respectable. Meanwhile the interest of the subject, and the well-known scientific character of the author, will commend the work to general attention and patronage.

The plan and object of the late Mr. Loudon's greatest work, "The Arboretum et Fruticetum Britannicum," is fully set forth in the copious title-page. All its promises are more than redeemed in the execution of the work, which is truly a fine monument of industry and careful research. We have particular reasons, which will appear in the sequel, for commending this work to the notice of any readers interested in these subjects, who do not already possess it. By purchasing a copy of it, or of the valuable abridgment, the "Encyclopædia of Trees and Shrubs," a work of moderate price, they will render important aid to the embarrassed family of the author. To give some idea of the astonishing industry of the late Mr. Loudon in the preparation of scientific books, we extract the following account from the "Gardeners' Magazine," an excellent periodical, the publication of which, after it had continued for a period of eighteen years, terminated at the death of its indefatigable editor.

"Mr. Loudon was brought up as a landscape-gardener, and began to practise in 1808, when he came to England with numerous letters of introduction to some of the first landed proprietors in the kingdom. He afterwards took a large farm in Oxfordshire, where he resided in 1809. In the years 1813, 1814, 1815, he made the tour of northern Europe, traversing Sweden, Russia, Poland, and Austria; in 1819, he traveled through Italy, and in 1828, through France and Germany. Mr. Loudon's career as an author began in 1803, when he was only twenty years old; and it continued, with very little interruption, during the space of forty years, being only concluded by his death. The first works he published were the following: 'Observations on laying out Public Squares,' in 1803, and on 'Plantations,' in 1804; a 'Treatise on Hot-

houses,' in 1805, and on 'Country Residences,' in 1806, both 4to; 'Hints on the Formation of Gardens,' in 1812; and three works on 'Hothouses,' in 1817 and 1818. In 1822 appeared the first edition of the 'Encyclopædia of Gardening'; a work remarkable for the immense mass of useful matter which it contained, and for the then unusual circumstance of a great quantity of woodcuts being mingled with the text: this book obtained an extraordinary sale, and fully established his fame as an author. Soon after was published an anonymous work, written either partly or entirely by Mr. Loudon, called the 'Greenhouse Companion'; and shortly afterwards, 'Observations on laying out Farms,' in folio, with his name. In 1824, a second edition of the 'Encyclopædia of Gardening' was published, with very great alterations and improvements; and the following year appeared the first edition of the 'Encyclopædia of Agriculture.' In 1826, the 'Gardeners' Magazine' was commenced, being the first periodical ever devoted exclusively to horticultural subjects. The 'Magazine of Natural History,' also the first of its kind, was begun in 1828. Mr. Loudon was now occupied in the preparation of the 'Encyclopædia of Plants,' which was published early in 1829, and was speedily followed by the 'Hortus Britannicus.' In 1830, a second and nearly rewritten edition of the 'Encyclopædia of Agriculture' was published, and this was followed by an entirely rewritten edition of the 'Encyclopædia of Gardening,' in 1831; and the 'Encyclopædia of Cottage, Farm, and Villa Architecture,' the first he published on his own account, in 1832. This last work was one of the most successful because it was one of the most useful he ever wrote, and it is likely long to continue a standard book on the subject of which it treats. Mr. Loudon now began to prepare his great and ruinous work, the 'Arboretum Britannicum,' the anxieties attendant on which were, undoubtedly, the primary cause of that decay of constitution which terminated in his death. This work was not, however, completed till 1838, and in the mean time he began the 'Architectural Magazine,' the first periodical devoted exclusively to architecture. The labor he underwent at this time is almost in-

credible. He had four periodicals, namely, the 'Gardeners',' 'Natural History,' and 'Architectural Magazines,' and the 'Arboretum Britannicum,' which was published in monthly numbers, going on at the same time; and, to produce these at the proper times, he literally worked night and day. Immediately on the conclusion of the 'Arboretum Britannicum,' he began the 'Suburban Gardener,' which was also published in 1838, as was the 'Hortus Lignosus Londinensis'; and in 1839 appeared his edition of Repton's 'Landscape Gardening.' In 1840 he accepted the editorship of the 'Gardeners' Gazette,' which he retained till November, 1841; and in 1842 he published his 'Encyclopædia of Trees and Shrubs.' In the same year he completed his 'Suburban Horticulturist'; and finally, in 1843, he published his work on 'Cemeteries,' the last separate work he ever wrote. In this list, many minor productions of Mr. Loudon's pen have necessarily been omitted; but it may be mentioned that he contributed to the 'Encyclopædia Britannica,' and Brande's 'Dictionary of Science'; and that he published numerous supplements, from time to time, to his various works."

The adverse circumstances under which all this labor was performed make the result appear still more remarkable. Early in life, an attack of inflammatory rheumatism permanently stiffened one of his knees and contracted his left arm. In 1820, his right arm was broken near the shoulder, and the bones were never properly united. But he still continued to write with his right hand until 1825, when the arm was broken a second time, and it became necessary to amputate it; after which, having about this time lost the use of the thumb and two fingers of the left hand, he was obliged to employ amanuenses. It was under these trying circumstances that his great "Arboretum" was prepared. The work was undertaken against the advice of his publishers and friends, who foresaw that it would wholly absorb his former earnings. But it was his favorite and crowning work, and he unflinchingly carried it onward to completion; though the result verified the predictions of his advisers. It cost ten thousand pounds sterling; and the whole proceeds of the sale,

as well as the copyrights of his other works, were pledged to the publishers for payment. It is gratifying to learn, that, since the writer's decease, an appeal has been made to the public in England, and with great success, to purchase copies of this work and of the other publications of Mr. Loudon, so that the copyright may be redeemed, and their future proceeds applied to the benefit of his surviving family. The subjects of landscape-gardening and arboriculture are attracting increased attention in the United States, and these valuable treatises are not yet so generally known as they deserve to be; we have thought it proper, therefore, to make this statement.

While systematically treating of the botanical character, propagation, management, and economical uses of trees, Mr. Loudon has interwoven a vast amount of curious matter respecting their history, geography, and literature. Being himself a distinguished landscape-gardener, he has successfully treated of their character and adaptation as component parts of general scenery, of which they form a most important element; for no other constituents — no lifeless objects — produce impressions at once so strong and so widely varied as trees. He has also collected interesting statistics respecting the longevity of trees; a subject upon which we intend to task the patience of our readers to some extent.

The most interesting ideas connected with trees are those suggested by their stability and duration. They far outlast all other living things, and form the familiar and appropriate symbols of long-protracted existence. "As the days of a tree shall be the days of my people" is one of the most beautiful and striking figures under which a blessing can be conveyed. We are naturally led to inquire, whether there is any absolute limit to their existence. If not destroyed by accident, — that is, by extrinsic causes, of whatever sort, — do trees eventually perish, like ourselves, from old age? It is commonly thought, no doubt, that trees are fully exposed to the inevitable fate of all other living things. The opposite opinion seems to involve a paradox, and to be contradicted by every one's observation. But popular opinion is an unsafe

guide ; — the more so in this case, as our ordinary conceptions on the subject spring from a false analogy, which we have unconsciously established, between plants and animals. This common analogy might, perhaps, hold good, if the tree were actually formed like the animal, all the parts of which are created at once in their rudimentary state, and soon attain their fullest development, so that the functions are carried on, throughout life, in the same set of organs. If this were the case with the tree, it would likewise die, sooner or later, of old age, — would perish from causes strictly analogous to those which fix a natural limit to the life of animals. The unavoidable induration and incrustation of its cells and vessels, apart from other causes, would put an early and sure limit to the life of the tree, just as it does in fact terminate the existence of the leaf, the proper emblem of mortality, — which, although it generally lives only a single season, may yet truly be said to die of old age. But, as the leaves are necessarily renewed every year, so also are the other essential organs of the plant. The tree is gradually developed by the successive addition of new parts. It annually renews not only its buds and leaves, but its wood and its roots ; everything, indeed, that is concerned in its life and growth. Thus, like the fabled Æson, being restored from the decrepitude of age to the bloom of early youth, — the most recent branchlets being placed, by means of the latest layer of wood, in favorable communication with the newly formed roots, and these extending at a corresponding rate into fresh soil, —

"Quae quantum vertice ad auras
Ætherias, tantum radice in Tartara tendit,"

why has not the tree all the conditions of existence in the thousandth, that it possessed in the hundredth, or the tenth, year of its age? The old and central part of the trunk may, indeed, decay ; but this is of little moment, so long as new layers are regularly formed at the circumference. The tree survives ; and it is difficult to show that it is liable to death from old age in any proper sense of the term. Nor do we arrive at a different conclusion when we contemplate the tree under a less familiar but more philosophical aspect, —

considering it, not as a simple individual, like man or the higher animals, but as an aggregate of many individuals, which though ordinarily connected with the parent stock, are capable of growing by themselves, and indeed often do separate spontaneously, and in a variety of ways acquire independent existence. If, then, the tree be, as it undeniably is, a complex being, an aggregate of as many individuals, united in a common trunk, as there are, or have been, buds developed on its surface; and if the component individuals be annually renewed, why should not the aggregate, the *tree*, last indefinitely? To establish a proper analogy, we must not compare the tree with man, but with the coral formations, in which numberless individuals, engrafted and blended on a common base, though capable of living when detached from the mass, conspire to build up those arborescent structures so puzzling to the older naturalists that they were not inappropriately named "zoöphytes," or animal-plants. The immense coral-groves, which have thus grown up in tropical seas, have, no doubt, endured for ages; the inner and older parts consisting of the untenanted cells of individuals that have long since perished, while fresh structures are continually produced on the surface. The individuals, indeed, perish; but the aggregate may endure as long as time itself. So with a tree, considered under this point of view. Though the wood in the centre of the trunk and large branches — the produce of buds and leaves that have long ago disappeared — may die and decay; yet while new individuals are formed upon the surface with each successive crop of fresh buds, and placed in as favorable communication with the soil and the air as their predecessors, the aggregate tree would appear to have no necessary, no inherent, limit to its existence.[1]

[1] A beautiful confirmation of this view may be drawn from the celebrated Banyan, or India Fig-tree, and a few other tropical trees, which freely strike root, high in the open air, from their spreading branches. These aërial roots, after reaching the earth, become in time new trunks; and the whole tree appears like a huge tent, supported by many columns. Milton's description of the Banyan (in "Paradise Lost") is incorrect, so far as it supposes the bending branches themselves to reach the ground, and there to strike root, just as the gardener propagates shrubs

No one denies, however, that different species may have an habitual period of death; we only insist that this is not a necessary period. In the course of things, a multitude of different accidents conspire to fix a mean limit to the life of man, which, though far below the natural period of death by old age, yet occurs with such regularity, under given conditions, that it is made a matter of calculation. So a particular kind of tree may be liable to certain accidents, which habitually insure its destruction within a definite period. A tree of rapid growth generally has a soft and fragile wood, and is therefore especially subject to decay, or to be broken or overthrown by tempests; and the chances of its destruction are fearfully multiplied with the increasing spread and weight of the branches. Each species, too, being somewhat uniformly exposed to a particular class of accidents, according to its constitution and mode of growth, may consequently exhibit something like an average duration. But death can no more be said to ensue from old age, in such a case, than in that of the ordinary mortality of mankind. The whole tree does not necessarily suffer, like the animal, from the death or amputation of its limbs; those that remain may be thereby placed, perhaps, in a more favorable condition than before. A tree may certainly be conceived to survive all ordinary accidents, or to be protected against them, and thus to live indefinitely; while animals, even if shielded from all external injury, must at last succumb to internal causes of destruction, — unavoidable, because inseparable from their organization.

by layering; whereas the roots themselves descend from a great height. When a sufficient number of these collateral trunks are formed to support the whole weight, the central, original stem may decay and disappear, as it often does, without affecting the existence of the tree; which thus increases into a grove, "high over-arched, with echoing walks between," that obviously may endure for an indefinite period. Many such trees are known, of immense magnitude, and doubtless of most extraordinary age. But the vegetable physiologist well knows that these essentially differ from ordinary trees, only in that a portion of the new wood, detached as it were from the branches, forms separate trunks instead of adhering throughout to the main trunk and contributing to its increase in circumference. These collateral trunks merely represent the outer and newer layers of ordinary trees, while the main stem represents the old and often decaying centre.

The Talipot Palm, which blossoms but once, and then perishes, — or the Century plant, which continues in our conservatories even for a hundred years without flowering, but dies when it has ripened its fruit, — may be adduced as cases of death by old age. But in its native climes, where our so-called Century-plant blossoms in the fifth or sixth year of its age, it as uniformly dies immediately afterwards. The result, in all such cases, is rather analogous to death from parturition, than to death by old age.

This doctrine of the indefinite longevity of trees — that they die from injury or disease, or, in one word, from accidents, but never really from old age — was first propounded by the distinguished De Candolle in one of his earliest writings,[1] near the commencement of the present century. It is entirely a modern doctrine (unless, indeed, we may suppose that Pliny comprehended the full meaning of his words, "Vites sine fine crescunt," which is improbable), and it is by no means surprising that it should have been received with incredulity, or vehemently controverted, by those who had not taken the pains to understand it. For the *a priori* considerations, from which the young Genevan botanist deduced his novel theory, were then, in truth, more or less hypothetical, and involved some hardy assumptions. They are now, however, amply confirmed, or at least so generally admitted by all vegetable physiologists, as to give the theory a high degree of antecedent probability. But De Candolle proceeded to indicate a mode in which its correctness might almost be tested by actual observation, and, having accumulated a great number of interesting data, he published, in 1831, the memoir[2] which, having been still further augmented, now constitutes one of the most interesting chapters of his masterly "Physiologie Végétale."

If this view be well founded, it is to be expected that different individuals of the same species should perish at very irregular periods; and that some should be found to escape all

[1] "Flore Francaise," 1, p. 223.

[2] "*Notice sur la Longévité des Arbres.*" Par Aug. Pyr. De Candolle, in the Bibliothèque Universelle, May, 1831.

the ordinary accidents that trees are heir to, and thus attain a longevity far transcending the habitual duration of the species. Is this view sustained by observation?

Before adducing the evidence which bears upon this question, it is necessary to inquire how the actual age of a tree may be ascertained. In most cases, — in all those trees which increase in diameter by annual concentric layers, — that is to say in nearly all trees except Palms and their allies, which for the present we may leave out of the question, the age may be directly ascertained by counting the annual rings on a cross section of the trunk. The record is sometimes illegible or nearly so, but it is perfectly authentic; and when fairly deciphered, we may rely on its correctness.[1] But the venerable trunks, whose ages we are most interested in determining, are rarely sound to the centre; and if they were, even the paramount interests of science would seldom excuse the arboricide. This decisive test, therefore, can seldom be practically employed, except in the case of comparatively young trees. The most remarkable recorded instance of its application is that of one of the old oaks at Bordza, in Samogitia (Russian Poland); which, having been greatly injured by a conflagration, was felled in the year 1812, and seven hundred and ten concentric layers were distinctly counted on the transverse

[1] The discovery, or at least the first explicit announcement of the now familiar fact, that ordinary trees grow by annual layers, so that the record of their age is inscribed upon the section of the trunk, is generally attributed to Malpighi. But, probably, we should understand this celebrated anatomist as merely giving a formal statement of what was already popularly known; for so obvious a fact could scarcely have escaped notice. Professor Adrien de Jussieu, the present representative of that illustrious family, has, moreover, lately reproduced a passage in the "Voyage de Montaigne en Italie," written in the year 1581, nearly fifty years before Malpighi was born, which proves this to have been the case. "*L'ouvrier, homme ingénieux et fameux à faire de beaux instruments de Mathématique, m'enseinga que tous les arbres portent autant de cercles qu'ils ont duré d'années, et me le fit voir dans tous ceux qu'il avoit dans sa boutique, travaillant en bois. Et la partie qui regard le septentrion est plus étroite, et a les cercles plus serrés et plus denses que l'autre.*"

And now it appears that Leonardo da Vinci knew, and mentioned it — as well as phyllotaxis. (MSS. note in Dr. Gray's handwriting.)

section, from the circumference towards the centre, where the space in which the layers could not be clearly made out was estimated to have comprised three hundred more. If the injured portion was not overestimated, the tree must have been a thousand years old. We have now before us a section of a fine trunk of the American Cypress (*Taxodium distichum*), upon the radius of which, twenty-seven inches in length, six hundred and seventy annual layers may be distinctly counted. The wood of this tree is so durable, that probably the age of trunks of more than twice that size might be ascertained by direct inspection.

When such a section cannot be obtained, we are obliged to resort to other and less direct evidence, affording only approximate, or more or less probable, conclusions. Sometimes lateral incisions, not endangering the life of the tree, furnish the means of inspecting and measuring a considerable number of the outer layers, and of computing the age of the trunk from its diameter and actual rate of growth. But as young trees grow much more rapidly than old ones, we should greatly exaggerate the age of a large trunk, if we deduced its rate of growth from the outer layers alone. We must therefore ascertain, by repeated observations, the average thickness of the layers of young trees of the same species; and by the judicious combination of both these data, a highly probable estimate may often be formed.

When unable to inspect any portion of the annual layers of remarkable old trees, we may occasionally obtain other indications upon which some reliance may be placed; such as the amount of increase in circumference between stated intervals; but as, on the one hand, we can never depend upon the entire accuracy of two measurements made at widely distant periods, while, on the other, the growth of a small number of years, however carefully ascertained, would be an unsafe criterion, this method can seldom be employed with much confidence. A more common mode is to employ the average rate of growth of the oldest trees of which complete sections have been examined, for the approximate determination of the age of remarkably large trunks of the same species, where the size

alone is known. For often repeated observation proves that the increase is greatest, in other words, the layers are thickest, in young trees; but that afterwards — after the first century, for instance — the tree increases in diameter at a much slower but somewhat uniform, or else still decreasing rate, which does not greatly vary in different trees of the same species. Such estimates would, therefore, always tend to underrate, rather than to exaggerate, the age of a large tree. But it is unsafe to apply this method to other than really venerable trunks; for the growth of a tree is liable to great variations during the first century or two; either from year to year, or between different individuals of the same species. The injury of a single leading root or branch, or the influence of a stratum of sterile soil, may affect the whole growth of a young tree for a series of years; while, in an older individual, the wide distribution of the roots and multiplication of the branches render the effect of local injuries nearly inappreciable, and the influence of any one or more unfavorable seasons is lost in the average of a great number. Thus the fine Elm in Cambridge, which during the last winter fell a victim to one of the most fatal and frequent accidents which in this country interfere with the longevity of trees, — having been cut down to make room for a petty building, just as it had reached its hundredth anniversary, — was fourteen feet in circumference at the height of three or four feet from the ground.[1] The girth of its more renowned and fortunate neighbor, the "Washington Elm," is but little over thirteen feet; and it might accordingly be inferred that it is some years the junior of the "Palmer Elm." But we learn from a very authentic source, that the celebrated Whitfield, when excluded from the pulpits of the town and college, preached under the shade of this tree in the summer of 1744, — just a century ago. It is, doubtless, at least one hundred

[1] This "Palmer Elm," as it was called, grew with more than ordinary rapidity for the first seventy years; when, to casual observation, it must have appeared nearly as large as when it was felled. For, during the last twenty-two years, it had increased only five and one-half inches in diameter, that is, at the rate of a quarter of an inch per annum.

and twenty or one hundred and thirty years old. We wish to place its size upon record for the use of future generations; and we therefore take this opportunity to state, that the trunk of the "Washington Elm," at Cambridge, now measures thirteen feet and two and a half inches in circumference, at the height of three feet from the ground; this being the point at which the girth is smallest, being unaffected either by the expansion of the roots below, or of the branches above, and therefore the proper place to measure it for this purpose. That this size is conformable to the age assigned is apparent from a comparison with other trees; such, for instance, as the "Aspinwall Elm, in Brookline, standing near the ancient house belonging to the family of that name, and which was known to be one hundred and eighty-one years old in 1837, when it measured sixteen feet eight inches at five feet from the ground, and twenty-six feet five inches close by the surface."[1] The noted Elm upon Boston Common should be about the same age. Its present girth, at five feet from the ground, is sixteen feet and one inch; at the height of three feet it measures seventeen feet eleven inches; near the earth, twenty-three feet and six inches. We have seen a map of Boston, published in the year 1720, upon which this Elm is delineated as a large tree. Its age, therefore, is certainly as great as that assigned to it in the subjoined account, which recently appeared in the newspapers of the day; — we know not upon what authority.[2]

[1] We quote the manuscript of an esteemed friend, who has devoted much attention to the history and growth of trees, and whose long expected volume, on the trees of New England, we hope will soon be given to the public.

[2] As such data may hereafter possess some interest, we may simply state, that the large "English Elm," one of the finest trees on Boston Common, is now eleven feet two inches in girth, at five feet from the ground; and twelve feet three inches, at the height of three feet. The American Elm, near the Botanic Garden at Cambridge, in front of the house of Judge Phillips, has a girth of thirteen feet, at six feet from the earth, and of fifteen feet, three feet lower down. Its neighbor, opposite the gardener's residence, is fourteen feet three inches in circumference, at six feet from the ground. (MSS. note in Dr. Gray's handwriting. 1874, Phillip's 14-9¾, Sander's 15-7½.)

"The 'Boston Traveller' states that this noble tree was set out about the year 1670, by Captain Daniel Henchman, and is therefore one hundred and seventy-five years old. Captain Henchman was a schoolmaster in Boston from 1666 to 1671. He joined the Ancient and Honorable Artillery Company in 1675. He was a distinguished captain, in King Philip's war, of a company of infantry. Forty-five years ago, the Great Elm had a large hollow in it, and was rapidly decaying, but was treated in a mode recommended by Forsyth, by clearing the cavity of rotten wood, and filling it with a composition composed principally of lime, rubbish from old buildings, and clay, and thus restored. It is now apparently as flourishing as ever, and without any appearance of the hollow, which was once large enough for a boy to hide himself in. The tree is a native Elm, which is the most hardy kind. Many of the old Elm-trees are what are called English Elms, with less extended roots and branches than the American."

But more commonly, perhaps, our estimates rest, either wholly or in part, upon historical evidence or tradition; and the most numerous and best authenticated cases of this kind may be expected to occur in Europe, where many trees, especially Chestnuts, Lindens, Oaks, and Yews, may be satisfactorily traced by records through several centuries.

Having thus briefly indicated the kinds or sources of evidence which are brought to bear with more or less directness and force upon this interesting question, we proceed to offer a condensed account of some of the more remarkable or curious cases of longevity in trees; which may show to what extent, and with what results, this various testimony has been actually applied. The evidence is cumulative. Individual cases would be little worth, if unsupported by others. But mutually strengthening each other, the obvious conclusion becomes almost irresistible, even when the testimony in particular cases is very imperfect.

We leave entirely out of view the numerous allusions to old trees that may be gathered from classical writers. Nor are the more circumstantial accounts by Pausanias, Josephus, or the Elder Pliny, available for our present purpose. The two

latter, indeed, speak of trees as old as the creation ;[1] but they have unfortunately neglected to mention the evidence upon which their opinions were founded. Restricting ourselves, therefore, to trees which still survive, or which have existed within recent times, we commence our enumeration with one which is rather remarkable for its historical associations than for any extraordinary longevity ; namely, the celebrated Sycamore Maple (*Acer Pseudo-Platanus*), which stands near the entrance of the village of Trons, in the Grisons, the cradle of liberty among the Rhetian Alps. Under the once spreading branches of this now hollow and cloven trunk, the Gray League — so called, either from the gray beards, or the home-spun clothing, of the peasants who there met the nobles favorable to their cause — was solemnly ratified in March, 1424. Upon the supposition that it was only a century old when the meeting, to which its celebrity is owing, took place, — and a younger tree would hardly have been selected for the purpose, — it has now attained the age of five hundred and twenty years. It can scarcely be younger, it may be much older than this. In some of the earlier accounts, this tree is said to be a Linden. Indeed, it is so called in the inscription upon the walls of the adjacent little chapel. They were better patriots than botanists in those days ; for the investigations of Colonel Bontemps leave no doubt as to the identity of the tree.[2]

The Linden itself, however, is associated with some interesting points of Swiss history ; it also affords some instances of remarkable longevity, which the lightness and softness of its wood would by no means lead us to expect. The Linden in the town of Freiburg, which was planted in 1476, to commemorate the bloody battle of Morât, though now beginning to decay, has already proved a more durable memorial than the famous ossuary on the battlefield,

[1] Josephus relates, that he saw near Hebron a Terebinthus which had existed ever since the creation (Lib. V., c. 31) ; and Pliny speaks of Oaks in the Hercynian forest, which he deems coeval with the world. (Hist. Nat., Lib. xvi., c. 2.)

[2] "Bibliothèque Univ. de Genéve," Août, 1831.

> "Where Burgundy bequeathed his tombless host,
> A bony heap, through ages to remain
> Themselves their monument;" —

and may even outlast the obelisk recently erected upon its site. The age of this tree and the girth of its trunk being well known, — having attained the circumference of fourteen English feet in 364 years, — it has been employed as a standard of comparison, in computing the age of larger and more venerable trunks of the same species.

Such a tree is still standing at the village of Villars-en-Moing, near the town of Morât, in full health and vigor, although portions of the bark are known to have been stripped off about the time of the battle in 1476, when it was already a noted tree. At four feet above the ground, the trunk has a circumference of thirty-eight English feet, and consequently a diameter of about twelve feet. Supposing it to have grown, on the whole, even a little more rapidly than the Freiburg Linden, which may be deemed a safe estimate, when we recollect that old trees grow much more slowly than younger ones, — supposing it to have increased in diameter at the average rate of one sixth of an inch in a year, it must have been 864 years old at the time the measurement was made, in the year 1831. It is not probable that this estimate materially exaggerates the age of the tree, even supposing the Linden at Freiburg to have grown at less than the average rate for the species. It is nearly corroborated, indeed, by the more celebrated Linden of Neustadt on the Kocher, in Würtemberg, whose age rests wholly upon historic evidence. The readers of Evelyn will surely remember his interesting account of this tree; and in recent times, some further particulars in its history have been rescued from oblivion by M. Jules Trembley, who visited it in 1831, at the instance of the illustrious De Candolle. It must have been already remarkable early in the thirteenth century; for, as is proved by documents still extant in the registers of the town, the village of Helmbundt, having been destroyed in the year 1226, was rebuilt three years afterwards, at some distance from its former site, in the vicinity of this tree, and took the name of "Neustadt an der grossen

Linden." An old poem, which bears the date of 1408, informs us, that "before the gate rises a Linden, whose branches are sustained by sixty-seven columns." The number of these columns, or pillars of stone, raised to support the heavy and widely spreading branches, one of which extends horizontally for more than a hundred feet, had increased to eighty-two when the tree was visited by Evelyn, and to one hundred and six when it was examined by Trembley. To these supports, doubtless, its preservation is chiefly owing; as the tender wood of the Linden could never sustain the enormous weight of the limbs, or resist the force of the winds. These pillars are nearly covered with inscriptions; of which the most ancient that was extant in Evelyn's time bore the date of 1551; but the oldest now legible bears the arms of Christopher, Duke of Würtemberg, with the date of 1558. At five or six feet from the ground, the trunk is thirty-five and a half English feet in circumference. If, therefore, it has grown at the actual rate of the Freiburg Linden, it must nearly have reached its thousandth anniversary. Or if, as in the case of the tree near Morât, we allow a sixth of an inch per annum for the average increase in diameter, its computed age would be a little over 800 years; surely, a moderate estimate for a tree which was called the Great Linden more than six centuries ago.

No tree of temperate climates so frequently attains an extraordinary size as the Plane, or Sycamore (Platanus); trunks of forty or fifty feet in circumference being by no means uncommon in this country. The Oriental Plane offers many equally striking instances in the south of Europe, particularly in the Levant. The celebrated tree on the island of Cos, so conspicuously seen from the channel on the Asiatic side, has recently been beautifully figured in Allen's "Pictorial Tour in the Mediterranean."

But old trunks, both of Oriental and our own very similar species, are always hollow, — mere shells; hence, in the absence of historical data, their age is only to be computed by their rate of growth; which is so rapid for the first century or two, and, at the same time, the wood is so liable to decay, that the Plane-tree is not likely to afford any instances of extreme

longevity. A different conclusion might, indeed, be drawn from the account of an enormous Plane in the valley of Bouyoudereh, near Constantinople, described by Olivier, Dr. Walsh, and others; the trunk of which is one hundred and fifty feet in girth, with a central hollow of eighty feet in circumference. But the recent observations of an excellent scientific observer, Mr. Webb, leave no doubt that this monster-trunk is formed by the junction of several original trees, planted in close proximity.[1] Along the shores of the Bosphorus there are many groups of younger Planes, which, for their shade, have been designedly planted in a narrow circle, and their trunks will in time become similarly incorporated. Pliny's Lycian Plane, with a cavity of eighty-one feet in circumference, in which the consul Licinius Mutianus used to lodge with a suite of eighteen persons, may have had such an origin.

We next notice the Chestnuts, for the purpose of disposing of an analogous case of pseudo-longevity; that of the famous "Castagno di cento cavalli"; so named from the somewhat apocryphal tradition, that Jeanne of Aragon, and a hundred cavaliers of her suite, took refuge under its branches during a heavy shower, and were completely sheltered from the rain. According to Brydone, who visited it in the year 1770, the trunk, or rather trunks, — for it then had the appearance of five distinct trees, — measured two hundred and four feet in circumference; but later and more trustworthy observers reduce these dimensions to one hundred and eighty or one hundred and ninety feet. A hut has been erected in the hollow space, with an oven, in which the inhabitants dry the chestnuts and other fruits which they wish to preserve for winter, using at times, for fuel, pieces cut with a hatchet from the interior of the tree. The separation of a large hollow

[1] Moquin-Tandon, "Teratologie Végétale," p. 290. — By the way, although De Candolle was not at the time apprised of the real nature of this Plane, yet he was far too cautious a reasoner to estimate its age at 2000 years, as Mr. Nuttall has inadvertently stated (Sylva, i. p. 50). Whoever will read the whole paragraph in the "Physiologie Végétale," ii. p 994, will perceive that it will by no means bear that construction.

trunk into independent portions, appearing like the remains of as many distinct trees, is not in itself improbable. The ancient Yew in Fortingal churchyard, Scotland, presents a striking instance of the kind. Indeed, Brydone's guide assured him "that, by the universal tradition and even testimony of the country, all these were once united in one stem; that their grandfathers remembered this, when it was looked upon as the glory of the forest, and visited from all quarters; though, for many years past, it had been reduced to the ruin we beheld. We began to examine it with more attention, and found that there is an appearance that these five trees were once really united in one. The opening in the middle is at present prodigious, and it does indeed require faith to believe that so vast a space was once occupied by solid timber. But there is no appearance of bark on the inside of any of the stumps, nor on the sides that are opposite to one another. . . . I have since been told by the Canon Ricupero, an ingenious ecclesiastic of this place, that he was at the expense of carrying up peasants with tools to dig round the 'Castagno di cento cavalli'; and he assures me, upon his honor, that he found all these stems united below ground into one root." [1]

It appears, however, that Brydone has not fairly represented the worthy Canon Ricupero's opinion; for he thought it probable that these present trunks were offshoots from the persistent base of a more ancient stem; a conclusion which is fully sustained by the observations of several competent naturalists, such as Duby,[2] Brunner,[3] and Philippi.[4] Every one knows how readily the Chestnut will throw up shoots from the root; and Philippi says it is a general custom in Sicily to cut them down after they have attained a considerable size, when the new stems that are thrown out from the base shortly become trees again. Other considerations would pre-

[1] "Tour through Sicily and Malta."

[2] De Candolle, "Phys. Vég.," ii. p. 992.

[3] "Excursion through the East of Liguria, Sicily, and Malta."

[4] "Ueber die Vegetation am Ætna"; in "Linnæa," vii. p. 727; and in "Comp. to Bot. Mag.," i. p. 90.

vent our assigning the highest antiquity to a tree not originally indigenous to Sicily, but doubtless introduced from the East.

There are, however, some colossal Chestnuts upon Mount Etna, with undoubtedly single trunks; three of which, recently measured, are found to have a circumference respectively of fifty-seven, sixty-four, and seventy feet. Some general idea of their age may perhaps be formed by a comparison with other individuals, whose history is better known, such as that at Sancerre, described by Bosc, which, although only thirty-three feet in girth at six feet from the ground, has been called the "Great Chestnut of Sancerre" for six hundred years; or the celebrated "Tortworth Chestnut," which Strutt, who in his "Sylva Britannica" has given a fine illustration of its massive bole, considers as probably the largest as well as the oldest tree standing in England, and which in the reign of Stephen, who ascended the throne in 1135, was already remarkable for its size, and well known as a signal boundary to the manor of Tamworth, now Tortworth, in Gloucestershire. But even this tree, although it has probably long since celebrated its thousandth anniversary, does not equal the smallest of the three Sicilian Chestnuts, being only fifty-two feet in circumference at five feet from the ground.

In the ascending scale of longevity, we pass from the Chestnut to the Oak, the emblem of embodied strength, one of the longest-lived, as it is the slowest-growing, of deciduous-leaved forest trees. The light and soft wood of the Linden, and even of the Chestnut, seems incompatible with great longevity. Such trees of eight hundred or a thousand years old are extraordinary phenomena, owing their prolonged existence to a rare conjunction of favorable circumstances, — the more important, as they are unexpected witnesses to the truth of our leading proposition. But this is no very uncommon age for that

"Lord of the woods, the long-surviving Oak."

The briefest biographical notice of Oaks remarkable for their age or size, or for historic memorials attesting their antiquity, would alone fill our pages. We can only refer the

curious reader to the pages of Evelyn, of Gilpin, and of Strutt; to the learned, but over-labored, "Amœnitates Quercineæ" of the late Professor Burnet, in Burgess's "Eidodendron," and especially to the more accessible and standard Arboretum of Loudon, whose condensed statistical account of celebrated British Oaks, occupying thirty closely printed pages of that elaborate work, is a monument of diligence, and contains a vast amount of interesting information. Indeed, Mr. Loudon's whole account of the Oak is incomparable, and should alone suffice to immortalize his name. Among the oldest specimens now extant in England are to be enumerated, the "Parliament Oak," in Clipstone Park, supposed to be the oldest park in England, which derives its name from a Parliament having been held under it by Edward the First, in 1290; the Oak in Yardly Chase, which Cowper has immortalized; the "Winfarthing Oak," now a bleached ruin, which is said to have been called an old oak at the time of the Conquest; the Oak in Melbury Park, Dorsetshire, which Mitchell calls "as curly, surly, knotty an old monster as can be conceived"; the "Greendale Oak," in the Duke of Portland's park at Welbeck, well known from Evelyn's account, and from the series of figures which his editor, Hunter, has given of its mutilated trunk, pierced by a lofty arch through which carriages have been driven; the "Cowthorpe Oak," in Yorkshire, also figured by Hunter, the trunk of which measures seventy-eight feet in circumference near the ground, and the age is estimated as nearly coeval with the Christian era; and the "Great Oak of Salcey Forest," in Northamptonshire, "a most picturesque sylvan ruin," which is perhaps of equal antiquity.

We have already mentioned the tree at Bordza, felled some thirty years ago, which was proved, by inspection of its annual layers, to have been about a thousand years old. Its trunk was forty English feet in circumferenee, or twelve and a half feet in diameter. This was a goodly tree for an Oak; but it shrinks almost to insignificance when compared with one in the south of France; an account of which has quite recently been published. From a late number of the "Gardeners'

Chronicle," edited by Professor Lindley, we copy the following account, which purports to have been extracted from the Annals of the Agricultural Society of Rochelle.

"At about six miles west-southwest of Saintes (in the Lower Charente) near the road to Cozes, stands an old Oak-tree, in the large court of a modern mansion, which still promises to live many centuries, if the axe of some Vandal does not cut it down. The following are the proportions of this king of the forests of France, and probably of all Europe. The diameter of the trunk at the ground is from nine to ten yards [consequently its circumference is from eighty-five to ninety-four feet]; at the height of a man, from six and a half to seven and a half yards [from sixty to sixty-seven feet in circumference]; the diameter of the whole head, from forty to forty-three yards; the height of the trunk, eight yards; the general height of the tree, twenty-two yards. A room has been cut out of the dead wood of the interior of the trunk, measuring from nine to twelve feet in diameter, and nine feet high; and they have cut a circular seat out of the solid wood. They put a round table in the middle, when it is wanted, around which twelve guests can sit. A door and a window admit daylight into this new sort of dining-room, which is adorned by a living carpet of Ferns, Fungi, Lichens, etc. Upon a plate of wood taken from the trunk about the height of the door, two hundred annual rings have been counted, whence it results, in taking a horizontal radius from the exterior circumference to the centre of the oak, that there must have been from 1800 to 2000 of these rings; which makes its age nearly two thousand years."

We should have been told, however, from what portion of the radius this block was taken. If near the circumference, where the rings are the narrowest, the age of the tree has been over-estimated; perhaps not materially so, as it must have been growing at a slow and nearly equable rate for many centuries; if towards the centre, the computed age is within the truth. To this tree, therefore, as being probably the patriarch of the species in Europe, may well be applied the lines addressed by Cowper to the Yardley Oak: —

"O, couldst thou speak,
As in Dodona once thy kindred trees,
Oracular, I would not curious ask
The future, best unknown; but, at thy mouth,
Inquisitive, the less ambiguous past!
By thee I might correct, erroneous oft,
The clock of history; facts and events
Timing more punctual; unrecorded facts
Recovering; and misstated, setting right."

Rich although this country is, above all other parts of the world, in different species of the Oak, it would not be difficult to explain why we cannot boast of such venerable trees,

"Whose boughs are mossed with age,
And high top bald with dry antiquity."

It is not merely, or chiefly, that, in clearing away the forest which so recently covered the soil, "men were famous according as they had lifted up axes upon the thick trees." The close, stifling growth of our primeval forests, like the democratic institutions which they seem to foreshadow, although favorable to mediocrity, forbids preëminence. "A chilly, cheerless, everlasting shade" prevents the fullest individual development; and even if the woodman's axe had spared the older trees, their high-drawn trunks, no longer shielded by the dense array of their brethren, were sure to be overthrown by the winds. Had the aboriginal inhabitants been tillers of the ground, our White Oaks had long since spread their broad brawny arms, and emulated their more renowned brethren in the parks of England. The "Charter Oak" at Hartford, so conspicuous in the colonial history of Connecticut, and a few others of equal size, but less note, were probably mere saplings at the first settlement of the country. "The Wadsworth Oak," in Geneseo, N. Y., however, may claim a higher antiquity. It stands in an old "Indian clearing," on the bank of the Genesee River, which, we are sorry to say, is gradually undermining its roots and threatening its destruction; — a catastrophe which we beseech the worthy proprietor of that princely estate to avert, by a seasonable embankment. A note in an earlier volume of this Review[1]

[1] Vol. xliv. p. 345, note.

assigns to this noble tree the age of at least five hundred years; — a credible estimate, notwithstanding the girth of the tree is somewhat overstated in that account. Its circumference at the smallest part of the trunk (four feet above the ground), — which is always the proper point for measurement, — instead of from twenty-four to twenty-seven, is only twenty-two feet four inches; although near the base, owing to the influence of the spreading roots, its girth is considerably greater.

But of all American species, the invaluable Live Oak of our southern coasts will probably be found to attain the greatest longevity; although it seldom becomes a very large, or, at any rate, a very tall tree. Like the finest European Oaks, its branches spread very widely, and contain a prodigious quantity of timber. "The trunk of the Live Oak," says Mr. Bartram, in his delightful "Travels in Florida," "is generally [on the St. John's River] from twelve to eighteen feet in girth, and rises ten or twelve feet erect from the earth; some I have seen eighteen or twenty; then divides itself into three, four, or five great limbs which continue to grow in nearly a horizontal direction, each limb forming a gentle curve, or arch, from its base to its extremity. I have stepped above fifty paces, on a straight line, from the trunk of one of these trees to the extremity of the limbs."

The younger Michaux mentions a tree felled near Charleston, whose trunk was twenty-four feet in circumference; and we learn that another individual of still greater size is still flourishing on the plantation of Mr. Middleton, near that city. According to Mr. Nuttall,[1] the tree sometimes acquires the diameter of eight or nine feet in west Florida. All these trees must have attained a great age; for this heavy and almost incorruptible wood is of extremely slow growth. May we not hope that some competent observer will collect the requisite information upon this subject, before all the larger trunks have yielded to their impending fate?

The Olive grows much more slowly than the Oak, and as its wood is very compact and durable, it is not surprising

[1] "N. Am. Sylva," Supplement, i. p. 16.

that it should furnish instances of extraordinary longevity. In comparative youth, the stem increases in diameter only at the rate of an eighth of an inch in a year. Therefore the Olive at Pescio, mentioned by De Candolle, having a trunk of twenty-four feet in girth, should be seven hundred years old; even supposing it to have grown, throughout, at the ordinary rate for younger trees; while the still larger tree at Beaulieu, near Nice, described by Risso, and recently measured by Berthelot, doubtless the oldest of the race in Europe, should be more than a thousand years old. Although now in a state of decrepitude, it still bears an abundant crop of fruit, or at least did so, as late as the year 1828.[1] It is not improbable, therefore, that those eight venerable trees, which yet survive upon the Mount of Olives, may have been in existence, as tradition asserts, at the time of our Saviour's passion.

Let us now direct our attention to the class of coniferous trees, among which, on account of the resinous matters that commonly pervade their wood and tend to preserve it from decay, as well as for other reasons which we will not stop to explain, instances of longevity may be expected to occur not inferior to those already noticed.

We begin with the classical cypress (*Cupressus sempervirens*), so celebrated in all antiquity for the incorruptibility of its wood and its funeral uses; doubtless, one of the longest lived trees of southern Europe and of the East. Hunter, in his edition of Evelyn, about a century ago, mentions the fine avenue of Cypresses "Los Cupressos de la Reyna Sultana," which adorns the garden of the Generaliffe at Granada. Under their shade, according to the well-known legend, the last Moorish king of Granada surprised his wife with one of the Abencerrages, which led to the massacre of thirty-six princes of that race. This was, of course, before the year 1492, the date of the final expulsion of the Moors. These enduring memorials of frailty and revenge were still flourishing in perennial vigor in 1831, when they were examined by

[1] Risso, "Hist. Nat. Europ. Merid.," ex Moquin-Tandon, "Teratol. Veg.," p. 105.

Mr. Webb. Supposing them to have been only forty or fifty years old at the occurrence of the event to which they owe their celebrity, — surely a reasonable supposition, as they were then large trees, according to the legend, — they have now reached the age of about four hundred years. They are probably much older than this.

But these and all other Cypresses known in Europe are striplings in comparison with the tree at Somma, in Lombardy, which Loudon has figured in his Arboretum (p. 2470), from an original drawing furnished by Signor Manetti of Monza. The tree is greatly reverenced by the inhabitants of that part of Lombardy, who have a tradition that it was planted in the year of our Saviour's birth. Even Napoleon is said to have treated it with some deference, and to have deviated from a direct line to avoid injuring it, when laying down the plan for the great road over the Simplon. Its trunk was twenty feet in girth, according to the Abbé Belèze's measurement, in 1832, or twenty-three feet at the height of a foot from the ground, as Signor Manetti states. Since the Cypress only attains the circumference of fourteen or fifteen feet in four hundred years or more, and after that must increase with extreme slowness, we may, perhaps, place some credit in the popular tradition respecting the age of this tree, or in the testimony of the Abbé Belèze, who informed Mr. Loudon that his brother assured him, that there is an ancient chronicle extant at Milan, which proves this tree to have been in existence in the time of Julius Cæsar!

To the same class, also, belongs the goodly Cedar of Lebanon (*Cedrus Libani*), from which the sacred writers have derived so many forcible and noble images. It is generally employed as an emblem of perennial vigor and longevity. The most plausible derivation of the name is from the Arabic "kedroum" or "kèdre," signifying "power"; and the most characteristic description of the tree, with its widespread horizontal branches and close-woven leafy canopy, is that given by the prophet Ezekiel, where it is assumed as a type of the grandeur and strength of the Assyrian empire.

"Behold, the Assyrian was a Cedar in Lebanon, with fair

branches, and with a shadowing shroud, and of an high stature; and his top was among the thick boughs. . . . Thus was he fair in his greatness, in the length of his branches; for his root was by great waters. The Cedars in the garden of God could not hide him; the Fir-trees were not like his boughs, and the Chestnut-trees were not like his branches; nor any tree in the garden of God like unto him in beauty." (Ezekiel, xxxi. 3, 7, 8.)

The celebrated grove near the summit of Mount Lebanon, to which there are particular allusions in Holy Writ, was first described in modern times by Belon, who visited it about the year 1550. The majestic old Cedars of this grove — at that time the sole, as they are still the finest, known representatives of the species — were then, as now, venerated by the Maronite Christians, who firmly believed them to have been coeval with Solomon, if not planted by his own hands, and made an annual pilgrimage to the spot, at the festival of the Transfiguration; the Patriarch celebrating high mass under one of the oldest Cedars, and very properly anathematizing all who should presume to injure them. The larger trees were described and measured by Rauwolf, an early German traveler, in 1574; by Thevenot, in 1655; and more particularly by Maundrell, in 1696; by La Roque, in 1722; by Dr. Pococke, in 1744, and by Labillardière, in 1787; since which time, De Candolle states, that all the older trees have been destroyed. But we have not been able to find the authority for this statement, and have reason to doubt its correctness. Although the number of large trees has diminished in every succeeding age, yet several recent visitors mention a few large trunks of equal size with those described by the earlier travelers. Indeed, M. Laure, an officer of the French Marine, who with the Prince de Joinville visited Mount Lebanon in the autumn of 1836, says, that all but one of the sixteen old Cedars mentioned by Maundrell are still alive, although in a decaying state; and that one of the healthiest, but perhaps the smallest trunks, measured thirty-three French feet, or about thirty-six English feet, in circumference, which, by the way, is nearly the girth of the largest that Maundrell

measured. We have little faith, however, in this particular identification; nor do we place confidence in the rate of growth of old Cedars, as deduced from the measurement of these trees at different periods. For, could we be sure that any two of these measurements were actually taken from the same trunk, it is still very unlikely that they were made at the same height from the ground, — a matter of great consequence, but which is left out of view in the records of the early travelers. But the girth of the larger trees being known by various measurements, and the average rate of growth of young Cedars being approximately determined from individuals that have grown in Europe, of well ascertained age and size, — such, for instance, as those in the Chelsea Botanic Garden, near London, planted in 1683, and the fine tree which adorns the hill in the Jardin des Plantes at Paris, and which was brought from England in 1734 by Bernard de Jussieu, — carried, it is said, in the crown of his hat for greater security, whose trunk, at its centennial anniversary, had just attained the circumference of ten feet, — we only need to know the thickness of the outer layers of these remarkable old trunks, or, in other words, their actual and recent rate of increase, in order to form a highly probable estimate of their age. By a few careful incisions into these trunks, the next traveler into the now frequented East, who feels interested in such questions, might supply this remaining desideratum, without real injury to these renowned natural monuments, or just exposure to the Patriarch's anathema.

From such very imperfect data as we now possess, De Candolle deems the trees measured by Rauwolf to have been at least six hundred years old; which would give the age of nearly nine hundred years to any of the number that may still survive. This estimate may fall considerably below the truth; but our present knowledge will not warrant the assumption of a higher one. Doubtless, this remarkable forest has existed from primeval times, while the oldest individuals, from age to age, have decayed and disappeared. But venerable as are the present representatives, which La Martine so grandiloquently apostrophizes, and conceives to have existed

in the days of Solomon, "yet few comparatively have the days of the years of their life been, and have not attained unto the days of the years of the life of their fathers," the real patriarchs of the world-renowned grove.

The Yew has, probably, a well founded claim to its reputation as the longest-lived tree of northern Europe; and its longevity appears the less surprising, when the closeness and incorruptibility of the wood are considered, as well as its extreme slowness of growth. A Yew

> "Of vast circumference and gloom profound"

is truly, as Wordsworth has it,

> "a living thing,
> Produced too slowly ever to decay;
> Of form and aspect too magnificent
> To be destroyed."

The frequent occurrence of ancient Yews in English churchyards is simply and beautifully explained by Mr. Bowman;[1] — the Yew, being indisputably indigenous to Great Britain, and being, from its perennial verdure, its longevity, and the durability of its wood, at once an emblem and an example of immortality, its branches would be employed by our Pagan ancestors, on their first arrival, as the best substitute for the Cypress, to deck the graves of the dead, and for other sacred purposes; and the innocent custom, like others of heathen origin, would naturally be retained and engrafted upon Christianity at its first introduction.

From the inspection of various trunks of two or three hundred years old, De Candolle drew the conclusion that the trunk of the Yew increases in diameter at the rate of a little more than a line — the twelfth of an inch — in a year for the first one hundred and fifty years, and at a little less than this rate during the next century or two. De Candolle proposed, therefore, to estimate the age of ancient Yews by assuming a line per annum as their average growth in diameter. Their age would in this way be readily computed by measuring their circumference, and thence obtaining the radius in lines; the tree being reckoned as many years old as there are lines in

[1] In "Mag. Nat. Hist.," 2d ser., i. p. 86.

its diameter. Since all trees grow the more slowly as they advance in years, this method would seem to be a safe one, if we were well assured that the average rate of growth has been correctly assumed. But extended observation upon Yews in England has shown that young trees often grow much more rapidly than De Candolle supposed; so that, from the application of his rule to Yews not more than four or five hundred years old, we should be liable greatly to exaggerate their age. But it is also found that still older trees grow so much more slowly, that the rule may be applied to very ancient Yews with reasonable probability that the estimate will fall beneath the truth, and make them appear younger than they really are. The greater the circumference of the tree, the less the danger that its more rapid early growth will falsify the estimate. The adoption of this rule leads, however, to rather startling conclusions.

The computed age of the famous Yews of Fountains' Abbey, near Ripon, in Yorkshire, is to a great extent sustained by the history of the abbey itself, as chronicled by Hugh, a monk of Kirkstall, whose narrative — still preserved, it is said, in the library of the Royal Society — forms the basis of the well known account in Burton's "Monasticon." This monastery, the noble ruins of which are now overlooked by the venerable trees that watched its erection, was founded in the year 1132, by Thurstan, Archbishop of York, for certain monks, whose consciences, being too tender to allow them to indulge in the relaxed habits of their own order, made them desirous of adopting the more rigid rule of the Cistercians, then recently introduced into England.

"At Christmas," therefore, says Burton, "the Archbishop, being at Ripon, assigned to these monks some land in the patrimony of St. Peter, about three miles west of that place, for the erecting of a monastery. The spot of ground had never been inhabited, unless by wild beasts, being overgrown with woods and brambles, lying between two steep hills and rocks, covered with wood on all sides, more proper for a retreat for wild beasts than for the human species. . . . Richard, the Prior of St. Mary's at York, was chosen Abbot by the

monks, being the first of this Monastery of Fountains; with whom they withdrew into this uncouth desert, without any house to shelter them in that winter season, or provisions to subsist on, but entirely depending on Divine Providence. There stood a large Elm in the midst of the vale, on which they put some thatch or straw, and under that they lay, ate, and prayed; the Bishop for a time supplying them with bread, and the rivulet with drink. But it is supposed that they soon changed the shelter of their Elm for that of seven Yew-trees growing on the declivity of the hill on the south side of the abbey; all standing at this present time [1658], except the largest, which was blown down about the middle of the last century. They are of an extraordinary size; the trunk of one of them is twenty-six feet six inches in circumference at the height of three feet from the ground; and they stand so near each other as to form a cover almost equal to a thatched roof. Under these trees, we are told by tradition, the monks resided till they built the monastery; which seems to be very probable, if we consider how little a Yew-tree increases in a year, and to what a bulk these are grown." (Burton, Monast., fol. 141.)

We have Pennant's measurements of one of these trees, taken in 1770, giving it a diameter of eight feet five inches, or 1212 lines. Hence, according to De Candolle's rule, it was then 1200 years old.

The fine Yew at Dryburgh Abbey, which is supposed to have been planted when the abbey was founded, in 1136, and which is in full health and vigor, has a trunk only twelve feet in circumference; its estimated age would, therefore, be less than six hundred years.

The "Ankernyke Yew," near Staines, a witness of the conference between the English barons and King John, and in sight of which Magna Charta was signed (between Runnymede and Ankernyke House), and beneath whose shade the brutal Henry the Eighth first saw gospel light in Anna Boleyn's eyes, measures twenty-seven feet eight inches in circumference, and should therefore be 1100 years old, which is about the age that tradition assigns to it. The trunk of the

"Darley Yew" in Derbyshire, having a mean diameter of nine feet five inches, would, by this rule, be 1356 years old. The Yew in Tisbury churchyard, Dorsetshire, the trunk of which measures thirty-seven feet in circumference, would now be almost 1600 years old. The same computation, applied to the "superannuated Yew-tree of Braburne churchyard, Kent," which, by the measurements of Evelyn himself and of Sir George Carteret, was fifty-eight feet eleven inches in circumference in the year 1660, would give it the respectable age of 2540 years at that time! This tree has long ago disappeared. But it did not greatly exceed in size the Yew still extant in Fortingal churchyard, in Perthshire, Scotland, situated in a wild district among the Grampian Mountains, which forms a good collateral witness to the credibility of Evelyn's account. The trunk of the "Fortingal Yew" was fifty-two feet in circumference, when measured by the Hon. Daines Barrington in 1769;[1] or fifty-six feet six inches, according to Pennant's somewhat later measurement;[2] the discrepancy being, no doubt, attributed to the fact that the two measurements were taken at different heights. In Barrington's time, the surface was nearly entire at the base, although upon one side all the interior had decayed. Afterwards, the cavity reached the opposite surface; and the trunk at length separated into two distinct semicircular portions, dead and decaying within, but alive and growing at the circumference, between which the rustic funeral processions were long accustomed to pass on their way to the grave. In this condition it is figured by Strutt, as the first illustration of his "Sylva Scotica"; but he has omitted to inform us when the sketch was taken. We suspect that it represents the tree as it appeared more than fifty years ago; for, if we rightly apprehend the account given by the excellent Dr. Neill of Edinburgh, who visited the place in the summer of 1833, one of these half-trunks has now disappeared, with the exception of some decayed portions that scarcely rise above the soil; but the other, which still shoots forth branches from the summit,

[1] "Phil. Trans.," lix. p. 37.

[2] "Tours in Scotland," in Pinkerton's Gen. Coll., vol. iii.

"gives a diameter of more than fifteen feet; so that it is easy to conceive that the circumference of the bole, when entire, should have exceeded fifty feet." "Considerable spoliations," Dr. Neill further observes, "have evidently been committed on the tree since 1769; large arms have been removed, and masses of the trunk itself carried off by the country people, with the view of forming 'quechs' or drinking-cups, and other relics, which visitors were in the habit of purchasing. Happily, further depredations have been prevented by means of an iron rail, which now surrounds the sacred spot; and this venerable Yew, which in all probability was a flourishing tree at the commencement of the Christian era, may yet survive for centuries to come."[1]

But we must not forget the typical representatives of the class of coniferous trees, the stately Pines and Firs; several species of which attain a great size, and especially an unexampled height. Indeed, their mode of growth — their straight, regularly tapering trunks, carried steadily upwards by the continued prolongation of the leading shoot, as well as the small lateral extension of their branches — is extremely favorable to loftiness of stature, and to full development in the midst of the forest. In such trees our own country abounds. We need not dwell upon so familiar an object as our own White Pine, which, like Saul, "from his shoulders upwards, higher than any of the people," lifts its kingly form above its forest brethren, to the altitude of from one hundred and fifty to at least one hundred and eighty feet.

"Not a prince,
In all that proud old world beyond the deep,
E'er wore his crown as loftily, as he
Wears his green coronal of leaves."

The White Pine is, par excellence, a New England tree, and has ever been identified with our commercial prosperity. The colonists of Massachusetts Bay, at a very early period, selected it as their cognizance, and when they first assumed the rights of a free people, they stamped its image on their coins. It does not seem to flourish on foreign soil; as we

[1] Jameson's "Edinb. Phil. Jour." (1833), xv. p. 343.

infer from Loudon's description, and the ill-favored figure which he gives as an illustration of its general appearance in English parks and pleasure-grounds ;[1] no less than from Gilpin's complaint of its "meagreness in foliage."[2]

Yet even the White Pine is overtopped by the Douglas Spruce (*Pinus Douglasi*), which forms the principal part of the gloomy forests of Oregon. The extraordinary height which this species attains was first recorded by Lewis and Clarke ; who state that the trunk is very commonly twenty-seven, and often thirty-six feet, in circumference, at six feet above the earth's surface ; and rises to the height of two hundred and thirty feet — one hundred and twenty of that height without a limb. One which was measured by a member of their party is said to have been forty-two feet in girth, at a height beyond the reach of an ordinary man, and was estimated to reach the altitude of three hundred feet![3] This account, so far as respects the general height of the tree, has been amply confirmed by succeeding travelers, and especially by that enterprising botanist, the late Mr. Douglas, whose name the species bears, and to whom its discovery is generally attributed.[4] Mr. Douglas was really the first to make known the Lambert Pine (*Pinus Lambertiana*) to the scientific world ; a species which grows on the southern frontiers of Oregon Territory and in northern California ; the height of which is the more extraordinary, as the trees do not form a thick forest, but are rather sparsely scattered over the plains.

[1] "Arb. Brit.," iv. p. 2881 f., 2196.

[2] "Forest Scenery," i. p. 87. — The natural and economical history of this important tree has already been fully recorded on the pages of this Journal. Vol. xliv. p. 339, and vol. lviii. p. 300.

[3] "History of the Expedition of Lewis and Clarke," ii. p. 155. — More surprising still, and, as to the height compared with the diameter of the trunk, to us nearly incredible, is their account of a fallen tree of the same species on Wappatoo Island, which, they state, "measured 318 feet in length, although its diameter was only three feet!" (Op. cit. ii. p. 225.)

[4] We have not found Lewis and Clarke's account anywhere cited or alluded to, except by the accurate (former) editor of the "American Almanac," in the volume for 1838, p. 108.

To give our readers some idea of the hardships which this indefatigable collector endured, and the risks at which our nurseries have been stocked with the trees, and our gardens with the now familiar flowers of Oregon and California, we extract from the journal of Douglas a portion of the account of his visit to a group of these Lambert Pines; merely remarking that it seems to afford a fair specimen of the perils which he continually incurred. Poor fellow! to have the life at last stamped out of him by a mad bullock in a pit, while pursuing his researches upon one of the Sandwich Islands!

"Thursday, the 25th. Weather dull, cold, and cloudy. When my friends in England are made acquainted with my travels, I fear they will think that I have told nothing but my miseries. This may be true; but I now know, as they may do also, if they choose to come here on such an expedition, that the objects of which I am in quest cannot be obtained without labor, anxiety of mind, and no small risk of personal safety, of which latter statement my this day's adventures are an instance. I quitted my camp early in the morning, to survey the neighboring country, leaving my guide to take charge of the horses until my return in the evening, when I found that he had done as I wished, and in the interval dried some wet paper which I had desired him to put in order. About an hour's walk from my camp I met an Indian, who, on perceiving me, instantly strung his bow, placed on his left arm a sleeve of raccoon skin, and stood on the defensive. Being quite satisfied that this conduct was prompted by fear, and not by hostile intentions, the poor fellow having probably never seen such a being as myself before, I laid my gun at my feet on the ground, and waved my hand for him to come to me, which he did, slowly and with great caution. I then made him place his bow and quiver of arrows beside my gun, and, striking a light, gave him a smoke out of my own pipe, and a present of a few beads. With my pencil I made a rough sketch of the cone and Pine-tree which I wanted to obtain, and drew his attention to it, when he instantly pointed with his hand to the hills fifteen or twenty miles distant towards the south; and when I expressed my

intention of going thither, he cheerfully set about accompanying me. At mid-day I reached my long-wished-for Pines, and lost no time in examining them, and endeavoring to collect specimens and seeds. New and strange things seldom fail to make strong impressions, and are therefore frequently overrated; so that, lest I should never again see my friends in England to inform them verbally of this most beautiful and immensely grand tree, I shall here state the dimensions of the largest I could find among several that had been blown down by the wind. At three feet from the ground, its circumference is fifty-seven feet nine inches; at one hundred and thirty-four feet, seventeen feet five inches; the extreme length two hundred and forty-five feet.[1] The trunks are commonly straight, and the bark remarkably smooth for such large timber, of a whitish or light-brown color, and yielding a great quantity of bright amber gum. The tallest stems are generally unbranched for two thirds of the height of the tree; the branches rather pendulous, with cones hanging from their points, like sugar-loves in a grocer's shop. These cones are, however, only seen on the loftiest trees, and the putting myself in possession of three of these (all I could obtain) nearly brought my life to a close. As it was impossible either to climb the tree or hew it down, I endeavored to knock off the cones by firing at them with ball, when the report of my gun brought eight Indians, all of them painted with red earth, armed with bows, arrows, bone-tipped spears, and flint knives. They appeared anything but friendly. I endeavored to explain to them what I wanted, and they seemed satisfied, and sat down to smoke; but presently I perceived one of them

[1] We take this to be the correct account. But, by an error in copying, as we suppose, the length of this same tree is given at only two hundred and fifteen feet, in the memoir inserted in the 16th volume of the "Transactions of the Linnæan Society"; whence it has been copied into Lambert's great work on Pines, Loudon's Arboretum, the "American Almanac" for 1838, and Hooker's "Flora Boreali-Americana." There is another apparent discrepancy between the two accounts. In the Linnæan Transactions, the timber is said to be "white, soft, and light." In his journal, Douglas says, the wood of the large tree he examined was "remarkably fine-grained and heavy."

string his bow, and another sharpen his flint-knife with a pair of wooden pincers, and suspend it on the wrist of the right hand. Further testimony of their intention was unnecessary. To save myself by flight was impossible; so, without hesitation, I stepped back about five paces, cocked my gun, drew one of the pistols out of my belt, and holding it in my left hand and the gun in my right, showed myself determined to fight for my life. As much as possible I endeavored to preserve my coolness; and thus we stood looking at one another without making any movement or uttering a word for perhaps ten minutes, when one, at last, who seemed the leader, gave a sign that they wished for some tobacco: this I signified that they should have, if they fetched me a quantity of cones. They went off immediately in search of them, and no sooner were they all out of sight, than I picked up my three cones and some twigs of the trees, and made the quickest possible retreat, hurrying back to my camp, which I reached before dusk. The Indian who last undertook to be my guide to the trees, I sent off before gaining my encampment, lest he should betray me. How irksome is the darkness of night to one under my present circumstances! I cannot speak a word to my guide, nor have I a book to divert my thoughts, which are continually occupied with the dread lest the hostile Indians should trace me hither and make an attack. I now write lying on the grass, with my gun cocked beside me, and penning these lines by the light of my Columbian candle, namely, an ignited piece of rosiny wood." ("Companion to Botanical Magazine," ii. pp. 130, 131.)

It is to be regretted, although, under the circumstances, it is by no means surprising, that Mr. Douglas did not secure, at the time, complete data for ascertaining the age of the prostrate trunk he measured, which, as he states, was certainly not the largest he saw. But in a block from a smaller trunk, of the same species, he sent to England, "there are fifty-six annual layers in a space of four and a half inches next the outside." If we suppose the large tree to have grown at an equivalent rate throughout, it must have been 1400 years old when overthrown. But if it grew during the first century at

the average rate of our White Pine for the same period, the estimate would be reduced to 1100 years; which is probably much beneath the truth.

But the most stately tree in North America — apparently an evergreen species of Taxodium or American Cypress — was subsequently observed by Douglas in Upper California. "This tree," he says, "gives the mountains a most peculiar, I was almost going to say, awful appearance — something which plainly tells that we are not in Europe. I have repeatedly measured specimens of this tree, two hundred and seventy feet long, and thirty-two feet round at three feet above the ground. Some few I saw upwards of three hundred feet high." [1] Truly these are trees,

> "to equal which the tallest pine,
> Hewn on Norwegian hills, to be the mast
> Of some great ammiral, were but a wand."

This naturally brings us to the proper North American Cypress (*Taxodium distichum*); one of the largest and most remarkable trees of our southern States, but which appears to attain its most ample development in the *tierras templadas* of Mexico. Bartram gives a characteristic description of the tree.

"It generally grows in the water, or in low flat lands, near the banks of great rivers and lakes, that are covered a great part of the year with two or three feet depth of water; and that part of the trunk which is subject to be under water, and four or five feet higher up, is greatly enlarged by prodigious buttresses, or pilasters, which in full grown trees project out on every side to such a distance that several men might easily hide themselves in the hollows between. Each pilaster terminates under ground in a very large, strong, serpentine root, which strikes off and branches every way just under the surface of the earth; and from these roots grow woody cones, called Cypress-knees, four, five, and six feet high, and from six to eighteen inches and two feet in diameter at their bases. The larger ones are hollow, and

[1] Journal of Douglas's second visit to the Columbia, etc., in Hooker, "Compan. to Bot. Mag.," ii. p. 150.

serve very well for bee-hives! A small space of the tree itself is hollow, nearly as high as the buttresses already mentioned. From this place the tree, as it were, takes another beginning, forming a great, straight column, eighty or ninety feet high; when it divides every way around into an extensive, flat, horizontal top, like an umbrella, where eagles have their secure nests, and cranes and storks their temporary resting-places. And what adds to the magnificence of their appearance is the streamers of long moss that hang from the lofty limbs, and float in the winds." (Bartram's "Travels through Carolina, Georgia, etc.," p. 91.)

In favorable situations, the tree sometimes attains the height of one hundred and twenty, or one hundred and forty feet, and a circumference of from twenty to forty feet, when measured quite above the singular dilated base. This is scarcely exceeded by the largest of the celebrated Cypresses in the gardens of Chapultepec, at Mexico, called the "Cypress of Montezuma," and which was already a remarkable tree in the palmy days of that unfortunate monarch, three and a half centuries ago. The girth of its trunk is forty-one feet, according to Mr. Ward,[1] or about forty-five, according to Mr. Exter; but its height is so great in proportion, that the whole mass appears light and graceful.

But this tree is greatly surpassed by the famous "Ahuehuete" (the Mexican name for the species) of the village of Atlisco, in the intendancy of Puebla, which was first described by Lorenzana from personal observation. The worthy Archbishop says that "the cavity of the trunk" — for the tree is hollow — "might contain twelve or thirteen men on horseback; and that, in the presence of the most illustrious Archbishop of Guatemala and the Bishop of Puebla, more than a hundred boys entered it."[2] The girth of the trunk, according to Humboldt, is a little over twenty-three metres, or seventy-six English feet, and the diameter of the cavity about sixteen feet.[3]

[1] "Travels in Mexico," ii. p. 230.

[2] Note to the Third Despatch of Cortes. This note is not found in Mr. Folsom's translation.

[3] "Essai Polit. Nouv. Esp.," ed. 2, ii. p. 54.

Still more gigantic — the Nestor of the race, if not of the whole vegetable kingdom — is the Cypress which stands in the churchyard of the village of Santa Maria del Tule, in the intendancy of Oaxaca, two and a half leagues east of that city, on the road to Guatemala by the way of Tehuantepec. In its neighborhood there are five or six other trees of the kind, which are nearly as large as the "Cypress of Montezuma," but which this one as much surpasses as that does the ordinary denizens of the forest. We possess three independent measurements of this enormous trunk. The first is that given by Humboldt, who states, probably on the authority of his informant, M. Anza, that the trunk is thirty-six metres (one hundred and eighteen English feet) in circumference. In the year 1827, Mr. Poinsett, then our minister at the court of Mexico, transmitted to the American Philosophical Society at Philadelphia a cord which represented the exact circumference of this tree. Its extraordinary length naturally excited some doubts as to the correctness of the measurement; and immediate application was made to Mr. Poinsett for further particulars. He accordingly transmitted a communication from Mr. Exter, an English traveler who had just returned from Oaxaca, and who had carefully examined the tree in question. Mr. Exter's letter was afterwards published in Loudon's "Magazine of Natural History"; and a French translation, accompanied by some interesting comments by the younger De Candolle, appeared in the "Bibliothèque Universelle" for 1831.[1] According to Mr. Exter's measurement, the trunk is forty-six *varas* — one hundred and twenty-two English feet — in circumference; which is nearly in accordance with Humboldt's account. In neither case is the height at which the trunk was measured expressly mentioned. But this point has been duly attended to by a recent scientific observer, M. Galeotti, who visited this celebrated tree in 1839 and in 1840, and whose careful measurement gives to the trunk the circumference of one hundred and five French (equal to one hundred and twelve English)

[1] Tom. xlvi., p. 387.

feet, at the height of four feet above the surface of the soil.[1] The previous measurements, therefore, were taken somewhat nearer the base. The tree as yet shows no signs of decay, although it bears less foliage in proportion to its size than its younger fellows. But we find no authority for Mr. Exter's statement, that this tree was mentioned by Cortès, and that its shade once afforded shelter to his whole European army. Perhaps he had in some way confounded it in his memory with a Cypress which the Conquistador passed on the march to Mexico, and which is still traditionally associated with his name.[2]

Mr. Exter reports, and the observations of recent travelers to some extent confirm the statement, that there are Cypresses near the ruins of Palenque, equal in size to the tree at Santa Maria del Tule. If this be so, they may claim a much higher antiquity than the ruins they overshadow. They must have witnessed the rise, the flourishing existence, the decline, and the final extinction of a race whose whole history has sunk into oblivion; while they are still alive.

By what means can we ascertain the age of large Cypress-trees? Some years since, when Professor Alphonse De Candolle — the son and worthy successor of the botanist who has rendered that name illustrious — attempted to answer this question, the only evidence within his reach was drawn from the rate at which trees of the kind had grown in France during half a century. He inferred that the American Cypress, in its early days, increases at the rate of about a foot in diameter every fifty years; and the estimate, although surely much too low for trees planted in favorable open situations (which have even been known to add annually an inch to their diameter for a series of years, both in Europe and in the United States), is yet quite as high as our own observations will allow for those which grow in their native forests. This rate would give to the Cypress of Montezuma the age of about seven centuries, and would render that at Oaxaca scarcely coeval with

[1] "Bulletin de l'Acad. Roy. des Sciences de Bruxelles," 1843, tom. x. p. 123.

[2] See Prescott's "History of the Conquest," i. p. 404.

the Christian era. Perhaps this is as great an age as we are warranted in assuming for the Cypress of Montezuma; but old trunks increase so much the more slowly as they advance in age, that we must certainly assign a vastly higher antiquity to the trees of Atlisco and Santa Maria del Tule. Yet far the most important element in the calculation is wanting; namely, the actual present rate of growth of these monstrous trunks, or of other old trees of the same species. In default of this essential evidence, De Candolle has instituted a comparison between these trees and the famous Baobabs of Senegal, upon which we place no great reliance, but from which he infers that the great Cypress of Santa Maria del Tule, if really the growth of a single trunk, is from four to six thousand years old, and perhaps dates its existence as far back as the actual creation of the world.[1]

We trust that the next intelligent traveler who visits this most ancient living monument, or any other Cypress of remarkable size, will not fail to complete the evidence that is needed, as the full solution of this curious problem may throw light upon some interesting questions respecting the physical history of the world. One or more lateral incisions, not at all endangering the existence of the tree, would at once reveal its actual growth for the last few centuries. And if made at proper points, and carried to a sufficient depth, they might enable the judicious operator to disprove or confirm the surmise, that this huge bole may consist of the trunks of two or three original trees, long since united and blended into one. This conjecture is by no means very improbable, although there is nothing in the external appearance of the trunk to confirm it.[2]

Meanwhile, the Cypresses of our southern States, although of more moderate dimensions, afford important assistance in this inquiry. It is generally known that old trees of the

[1] Alphonse De Candolle, in "Bibl. Univ.," xlvi. p. 393. Aug. Pyr. De Candolle, "Phys. Veg.," ii. p. 1000.

[2] In opposition to the remark of M. Anza, cited by Humboldt (Essai Polit., the Engl. transl., ii. p. 190), we may adduce the account of Mr. Exter, and the negative testimony of M. Galeotti.

kind grow very slowly; but there are no accounts on record, so far as we can learn, respecting their rate of growth. Our own observations, though not so extended as could be wished, incline us to adopt the standard which De Candolle assumed for the Yew; namely, the twelfth of an inch for the annual increase of old Cypresses in diameter, when growing in their native forests. But we would only apply this rule to trunks of large size, and with all the precautions that have already been mentioned; for the Cypress grows, or at least may grow, quite rapidly for the first century or two; but when old, it appears to increase quite as slowly as the Yew. We have counted sixty layers of the wood in the space of an inch. A fine section of a Cypress-trunk, which grew near Wilmington, in North Carolina, now lies before us, which, on an average radius of twenty-seven inches, or diameter of fifty-four inches, exhibits six hundred and seventy annual layers. It has, therefore, grown throughout at the average rate of less than the twenty-fourth of an inch a year, measured on the radius, or the twelfth of an inch on the diameter. The trunk was thirteen inches in diameter at the expiration of its first century, and twenty-seven inches about the close of the second; it added seven inches to its diameter during the third century, and a nearly equal amount during the fourth; and for the remaining three hundred and seventy years, it grew at a still slower, but, on the whole, nearly equable rate.

Now it is deemed a safe mode, as we have already shown, to employ the rate of growth deduced from comparatively young trees for the determination of the age of larger and older trunks of the same species. Not only is our estimate, in all such cases, likely to fall below the truth, but the larger the trunk in question the less the danger of exaggeration. Let us apply to the Mexican Cypresses the data furnished by our Wilmington tree. If the Cypress of Montezuma has grown, on the average, even a little more rapidly than the trunk before us, — has increased in diameter at the mean rate of an inch in twelve years, — it must now be fully two thousand years old. But if we suppose it to have grown at twelve times this rate (which is the maximum for young Cypresses

under the most favorable circumstances) during the whole of the first century, we should thereby reduce the estimated age to a thousand years. By the same computation, the Cypress at Atlisco would be 3480 years old; or 2390 years, if we allow it the maximum rate of growth for the first century. So, likewise, the great Cypress at Santa Maria del Tule would be 5124 years of age, or 4024 years, with the aforesaid deduction. The latter accords perfectly with De Candolle's minimum estimate; and it is the lowest age that, in the present state of our knowledge, can possibly be assigned to this prodigious tree, upon the supposition that its trunk is really single.

We are obliged to pass unnoticed those trees of unknown species, but of surprising size, which the learned and enthusiastic Professor Martius visited in the interminable woods that border the Amazon, and of which he has recently published such a spirited account.[1] Their trunks were so huge that the outstretched arms of fifteen men were required to grasp them; and so lofty, as to mock every effort for obtaining even a leaf or flower, by which the species might be determined. As to their age, Martius offers only a conjectural estimate.

The Baobab, or Monkey-Bread (*Adansonia digitata*), of Senegal and the Cape de Verde Islands, has long afforded the most celebrated instances of vegetable longevity. The tree is remarkable for the small height which it attains, compared with the diameter of the trunk or the length of its branches. Trunks which are seventy or eighty feet in circumference rise to the height of only ten or twelve feet, when they divide into a great number of extremely large branches, fifty or sixty feet in length, which, spreading widely in every direction, form a hemisphere or hillock of verdure, perhaps one hundred and fifty feet in diameter, and only seventy in elevation. To this peculiarity, rather than to the nature of the wood, which is light and soft, the great longevity of the tree is probably owing, its form opposing an effectual resist-

[1] "Flora Brasiliensis," Tab. Physiog., ix.; "Arbores ante Christum natum enatæ."

ance to the tempests which would overthrow ordinary trees. Its roots spread in a similar manner beneath the soil. When laid bare by a torrent that has washed away the earth, they have been traced to a distance of more than a hundred feet without reaching their extremity. The history of these Baobabs, possibly of the very trees which Adanson's account has rendered famous, reaches back to the discovery of that part of the African coast, and of the Cape de Verde Islands, by Cadamosto, in 1455; who, in his narrative, mentions the singular disproportion between the height and the girth of these trees.[1] But they were first fully described by the French naturalist Adanson, who examined them about a century ago. The largest trunks that Adanson measured were eighty-five feet in circumference, or twenty-seven in diameter. Golberry is said to have measured one that was over a hundred feet in girth. Quite recently, M. Perrottet has met with many Baobabs in Senegambia, varying from sixty to ninety feet in circumference, yet still in a green old age, and showing no signs of decrepitude. There can be no doubt, therefore, respecting the prodigious size which these trees attain; and there is great reason to believe that they are among the oldest denizens of our planet. Indeed, their age is plausibly estimated at five or six thousand years. And the younger De Candolle has placed so much confidence in this estimate that he has employed it as a standard of comparison in the case of the Mexican Cypresses which we have just considered. If the evidence were really as direct as is generally thought, we could interpose no serious objection to such a conclusion. But a critical examination proves that the whole account given by recent writers, upon Adanson's authority, is strangely at variance with his own statements.

The current narrative is substantially and briefly as follows: — that Adanson observed, at the Madelaine Islands, near Cape de Verde, some Baobab-trees of thirty feet in

1 "Arbores vero ibi sunt tantæ magnitudinis, ut earum ambitus sit pedum xvii, licet eminentia altitudinis non quadret magnitudini; non enim altius tolluntur quam pedes xx," etc. (A. Cadamusti, Navig., c. xliii., in Grynæus, Nov. Orb., p. 45.)

diameter, upon the trunks of which he found inscriptions that had been made by former visitors three centuries before; that, by cutting through three hundred annual layers, he discovered the vestiges of these inscriptions upon the wood, thus proving that they were actually made at the date assigned; that, by measuring the thickness of these layers, he ascertained the actual increase of the trunk during the last three centuries; that, having thus obtained the rate of growth in old age, and having, by actual inspection of young trunks, learned the rate of growth during the first hundred years, he deduced from these combined data the almost inevitable conclusion, that the trees in question were five or six thousand years old.[1]

Let us compare this with Adanson's own statements, from which it purports to have been taken. His first account, which comprises all the principal facts in the case, is given in the "Voyage au Sénégal," prefixed to his volume on natural history of that country, which was published soon after his return to France, in 1753. Adanson simply relates, that, on his visit to the Madelaine Islands, he found Baobab-trees of five or six feet in diameter, which bore European names and dates, deeply engraven upon the bark. Two of these he took the trouble to renew, one of which was dated in the fifteenth, the other in the sixteenth century. The characters were about six inches in length, and as in breadth they occupied but a small part of the circumference of the trunk, Adanson reasonably inferred that they were not engraven in the early youth of these trees. He had previously seen, on the island of Senegal, trees of the kind, which were sixty-three and sixty-five feet in circumference; but he does not intimate that he inspected the layers of wood in any case. He merely remarks that these inscriptions might furnish some evidence respecting the age which Baobabs sometimes attained; "For," says he, "if we suppose that the inscriptions were engraven even in the early years of these trees, and that

[1] See Alphonse De Candolle, in "Bibl. Univ.," xlvi. p. 389. (Aug. Pyr. De Candolle, Phys. Veg., ii. p. 1003. Moquin-Tandon, Teratol. Veg., p. 107.)

they have grown to six feet in diameter in the course of two centuries, we may calculate how many centuries they would require to attain the full diameter of twenty-five feet." [1] Soon afterwards, Adanson communicated to the Royal Academy of Sciences of Paris a full account of the Baobab; which was published in the volume of Memoirs of that society for the year 1761; and, lastly, he wrote the article "Baobab" for the supplement to the great French Encyclopædia, published in the year 1776. These accounts, although more detailed, embody no essential additions to what has already been given. He says that the trees in question were two in number, upon the bark of which the names of Europeans were engraved, with dates, some posterior to the year 1600; and others, as far back as 1555, were probably the work of those who accompanied Thevet, who, in his voyage to antarctic lands, saw some of these trees that same year.[2] Some of the dates appeared to be anterior to 1500, but these were somewhat equivocal. Neglecting, therefore, the indistinct dates in the fourteenth century, continues Adanson, and even allowing that the inscriptions were made when the trees were very young, which is highly improbable, as they occupied less than an eighth of the entire circumference, it is evident, that, if the Baobab has attained six feet in diameter between 1555 and 1749, that is, in two hundred years, it would require more than eight centuries to attain the diameter of twenty-five feet, supposing the growth to continue at a uniform rate. But Adanson goes on to say that trees grow the more slowly as they advance in age; so that such an estimate would fall

[1] "Voyage au Sénégal," Paris, 1757, p. 66.

[2] "Aupres du promontoire Verd, y a trois petites isles prochaines de terre ferme, autres que celles, que nous appellõs Isles de Cap Verd, dont nous parlerons cy apres, assez belles, pour les beaux arbres, qu'elles produissent; toutesfois elles ne sont habitées. . . . En l'une de ces isles se trouve un arbre, lequel porte feuilles semblables a celles de noz figuiers; le fruit est lõg de deux pieds ou envirõ, et gros en proportion," etc. (Thevet, "Singularités de la France Antarctique;" Anvers, 1558, p. 18.) Thevet proceeds to describe the fruit, its edible character, its furnishing food for monkeys, etc., so as to leave no doubt as to its being a Baobab.

below the truth. As to its rate of growth when young, he states that the tree acquires the diameter of an inch or an inch and a half in the first year; the diameter of a foot in ten years, and about a foot and a half in thirty years; but so far from having extended these data, and employed them in the manner which is attributed to him, he says, that, although it might be desirable thus to employ them, a sound geometry teaches that they are quite insufficient for the purpose. Hence, instead of attempting any precise determination, he merely offers the probable conjecture, that these largest Baobabs may have been in existence several thousand years, or nearly from the period of the universal deluge; which would give them a claim to be considered the most ancient living monuments in the world.[1]

We cannot learn that Adanson ever made any further statements upon the subject; and, as he never revisited the African coast, he cannot have collected additional facts. His original writings plainly show that he never pretended to have obtained the data and made the estimates which have so long been attributed to him. To whom belongs the credit of falsifying his testimony we are unable to ascertain, as the authors above mentioned do not cite their immediate authority; — perhaps to one M. Duchesne, whose name the elder De Candolle has casually alluded to, as having drawn up a table, exhibiting the diameter of the Baobab at different periods, doubtless upon the very plan that Adanson pointed out and condemned. We are only surprised that such accurate and judicious writers as the De Candolles, father and son, should have relied upon second-hand authorities in any case where the originals were accessible, and especially in what they term "the most celebrated case of extreme longevity that has yet been observed with precision."[2]

[1] "Mém. Acad. Sciences," 1761, p. 231; and "Encycl. Suppl.," vol. i. p. 798.

[2] A passage which has met our eye in Mirbel's "Elémens de Physiologie Végétale," i. p. 116, shows that no such data as those which have been, as we suppose, falsely assumed, were known to that author down to the year 1815.

We close our enumeration, already too protracted, with a case of longevity, perhaps transcending that of the oldest Baobabs, or of the Mexican Cypresses; namely, the famous Dragon-tree (*Dracæna Draco*) of the city of Orotava, in Teneriffe. This tree has been visited by many competent observers; and among others, by that prince of scientific travelers, the veteran Humboldt, who has given a good figure of it, as it appeared about seventy years ago, from a drawing made by M. Ozonne in 1776. A later and much fuller account was published about twenty years since, by M. Berthelot,[1] who has assiduously devoted many years to the study of the civil and natural history of the Canary Islands; and a fine figure of the mutilated trunk, as it appeared after the terrible storm of the 21st of July, 1819, forms one of the most striking pictorial illustrations of that elaborate and excellent work, the "Histoire Naturelle des Iles Canaries," by P. Barker Webb, Esq., and M. Berthelot.

The trunk is by no means equal in size to some of the trees already noticed. It is only fifty feet in girth at the base, and not more than sixty or seventy in elevation. But, at the discovery of Teneriffe in 1402, nearly four and a half centuries ago, this Dragon-tree was nearly as large as at the present day, and had been immemorially an object of veneration among the Guanches. After the conquest, at the close of the fifteenth century, the trunk was employed as a boundary in dividing the lands, and as such is mentioned in ancient documents. It had changed very little since that period, except that the centre had been hollowed by slow decay, until the summer of 1819, when a third of its spreading top was carried away by a tempest. But it still continues to vegetate; and its remaining branches are still annually crowned, — as they have been each returning autumn, perhaps for hundreds of centuries, with its beautiful clusters of white, lily-like blossoms, — emblems of "the eternal youth of nature."

The Dragon-tree, like its allies the Palms, and unlike ordinary trees, does not increase in diameter by annual concentric layers. The usual means of investigation are here of

[1] In "Nova Acta Acad. Nat. Cur.," xiii., 1827, p. 781.

no account; and, apart from historic evidence, we can only form a somewhat conjectural estimate of the age of this celebrated trunk, by a comparison with young trees of the same species, which are known to grow with extreme slowness. M. Berthelot, who has attempted the comparison under the most favorable circumstances, — having lived many years upon the island, — declares that the calculations which he has made, upon the supposition that the trunk has increased in size even at the rate of young Dragon-trees up to within the last eight hundred or one thousand years, have more than once confounded his imagination. We cannot but assign the very highest antiquity to a tree like this, which the storms and casualties of four centuries have scarcely changed.

Upon the whole, we cannot resist the conclusion, that many trees have far survived what we are accustomed to consider their habitual duration; that even in Europe, where man has so often and so extensively changed the face of the soil, as his wants or caprices have dictated, some trees, favored by fortune, have escaped destruction for at least one or two thousand years; while in other, and particularly in some tropical countries, either on account of a more favorable climate, or because they have been more respected, or haply more neglected, by the inhabitants, a few may with strong probability be traced back to twice that period; and, perhaps, almost to that epoch which the monuments both of history and geology seem to indicate as that of the last great revolution of the earth's surface. After making every reasonable allowance for errors of observation and too sanguine inference, and assuming, in the more extraordinary cases, those estimates which give minimum results, we must still regard some of these trees, not only as the oldest inhabitants of the globe, but as more ancient than any human monument, — as exhibiting a living antiquity, compared with which the mouldering relics of the earliest Egyptian civilization, the pyramids themselves, are but structures of yesterday.

THE FLORA OF JAPAN.[1]

It is interesting to notice that, notwithstanding the comparative proximity of Japan to western North America, fewer of its species are represented there than in far distant Europe. Also, — showing that this difference is not owing to the separation by an ocean, — that far more Japanese plants are represented in eastern North America than in either. It is, indeed, possible that my much better knowledge of American botany than of European may have somewhat exaggerated this result in favor of Atlantic North America as against Europe, but it could not as against western North America.

If we regard the identical species only, in the several floras, the preponderance is equally against western as compared with eastern North America, but is more in favor of Europe. For the number of species in the Japanese column[2] which likewise occur in western North America is about 120; in eastern North America, 134; in Europe, 157.

Of the 580 Japanese entries, there are which have corresponding

European representatives, a little above 0.48 per cent.; of identical species, 0.27.

Western North American representatives, about 0.37 per cent.; of identical species, 0.20.

[1] Extract from the concluding part of a "Memoir on the Botany of Japan, in its relations to that of North America, and of other parts of the Northern Temperate Zone." (Memoirs of the American Academy of Arts and Science, new series, vi. 1859.)

It is this paper which fixed the attention of the scientific world upon Professor Gray and established his reputation as a philosophical naturalist. — C. S. S.

[2] The column in a tabular view of the distribution of Japanese plants and their nearest allies in the northern temperate zone.

Eastern North American representatives, about 0.61 per cent.; of identical species, 0.23.

So geographical continuity favors the extension of identical species; but still eastern North America has more in common with Japan than western North America has.

The relations of this kind between the floras of Japan and of Europe are obvious enough; and the identical species are mostly such as extend continuously — as they readily may — throughout Russian Asia, some few only to the eastern confines of Europe, but most of them to its western borders. To exhibit more distinctly the features of identity between the floras of Japan and of North America, and also the manner in which these are distributed between the eastern and western portions of our continent, — after excluding those species which range around the world in the northern hemisphere, or the greater part of it, or (which is nearly the same thing in the present view) which are unknown in Europe, — I will enumerate the remaining peculiar species which Japan possesses in common with America: —

In Japan.	In W. N. America.	In E. N. America.
Anemone Pennsylvanica		A. Pennsylvanica
(Coptis asplenifolia ?)	C. asplenifolia	
(Trautvetteria palmata)	T. palmata	T. palmata
Caulophyllum thalictroides		C. thalictroides
Diphylleia cymosa		D. cymosa
Brasenia peltata	(B. peltata)	B. peltata
Geranium erianthum	G. erianthum	
Rhus Toxicodendron	R. Toxicodendron, var.	R. Toxicodendron
Vitis Labrusca (Thunb.)		V. Labrusca
Thermopsis fabacea	T. fabacea	
Prunus Virginiana ?		P. Virginiana
Spiræa betulæfolia	S. betulæfolia	S. betulæfolia
Photinia arbutifolia, in Bonin.	P. arbutifolia	
Pyrus rivularis ?	P. rivularis	
Ribes laxiflorum	R. laxiflorum	
(Penthorum sedoides, China)		P. sedoides
Cryptotænia Canadensis		C. Canadensis
Heracleum lanatum	H. lanatum	H. lanatum

(Archemora rigida ?)		A. rigida
(Archangelica Gmelini)	A. Gmelini	A. Gmelini
Cymopterus littoralis ?	C. littoralis	
Osmorrhiza longistylis	O. longistylis	O. longistylis
Echinopanax horridus	E. horridus	
Aralia quinquefolia		A. quinquefolia
Cornus Canadensis	C. Canadensis	C. Canadensis
Viburnam plicatum		V. plicatum (lantanoides)
*Achillea Sibirica	*A. Sibirica	
*Artemisia borealis	*A. borealis	*A. borealis
Vaccinium macrocarpon	V. macrocarpon	V. macrocarpon
Menziesia ferruginea	M. ferruginea	M. ferruginea
(Boschniakia glabra ?)	B. glabra	
*Pleurogyne rotata	*P. rotata	*P. rotata
(Asarum Canadense ?)		A. Canadense
*Polygonum Bistorta	P. Bistorta	
Rumex persicarioides	R. persicarioides	R. persicarioides
Liparis liliifolia		L. liliifolia
Pogonia ophioglossoides		P. ophioglossoides
Iris setosa	*I. setosa	
Trillium erectum, var.		T. erectum
(Smilacina trifolia)		S. trifolia
Polygonatum giganteum		P. giganteum
(Streptopus roseus)	S. roseus	S. roseus
Veratum viride	V. viride	V. viride
Juncus xiphioides	J. xiphioides	
(Cyperus Iria)		C. Iria
Carex rostrata		C. rostrata
Carex stipata	C. stipata	C. stipata
Carex macrocephala	C. macrocephala	
Sporobolus elongatus	S. elongatus	S. elongatus
Agrostis scabra	A. scabra	A. scabra
Festuca pauciflora	F. pauciflora	
Adiantum pedatum	A. pedatum	A. pedatum
Onoclea sensibilis		O. sensibilis
Osmunda cinnamomea		O. cinnamomea
Lycopodium lucidulum		L. lucidulum
(Lycopodium dendroideum)	L. dendroideum	L. dendroideum

The names inclosed in parentheses are of species which I have not seen from Japan: some of them inhabit the adjacent mainland; some are imperfectly identified. Those marked * are high northern species in America.

Of these fifty-six extra-European species, thirty five inhabit

western, and forty-one eastern North America. And fifteen are western and not eastern; twenty-one eastern and not western; and twenty common to both sides of the continent. Eight or ten of these fifty-six species extend eastward into the interior of Asia.

On the other hand, the only species which I can mention as truly indigenous both to Japan and to Europe, but not recorded as ranging through Asia, are: —

Euonymus latifolius, *Valeriana dioica*, *Pyrola media*, *Fagus sylvatica*, *Streptopus amplexifolius*, *Blechnum spicant*, *Athyrium fontanum*.

Two of these species extend across the northern part of the American continent and on to the Asiatic; another occurs on the northwest coast of America; and another, the Fagus, is represented in eastern America by a too closely related species. It is noteworthy that not one of these seven plants is of a peculiarly European genus, or even a Europæo-Siberian genus; while of the fifty-six species of the Americo-Japanese region wanting in Europe, twenty are of the extra-European genera, seventeen are of genera restricted to the North American, east Asian, and Himalayan regions (except that Brasenia has wandered to Australia); fourteen of the genera (most of them monotypic) are peculiar to America and Japan or the districts immediately adjacent; one is peculiar to our northwest coast and Japan; and eight are monotypic genera wholly peculiar (Brasenia excepted) to the Atlantic United States and Japan. Add to these the similar cases of other American species (nearly all of them particularly Atlantic-American) which have been detected in the Himalayas or in northern Asia, — such as *Menispermum Canadense* (*Dauricum*, DC.), *Amphicarpæa monoica? Clitoria Mariana*, *Osmorrhiza brevistylis*, *Monotropa uniflora*, *Phryma leptostachya*, *Tipularia discolor?* etc., — and it will be almost impossible to avoid the conclusion that there has been a peculiar intermingling of the eastern American and eastern Asian floras, which demands explanation.

The case might be made yet stronger by reckoning some subgeneric types as equivalent to generic in the present view.

and by distinguishing those species or genera which barely enter the eastern borders of Europe; *e. g.*, *Cimicifuga fœtida*, *Mœnringia lateriflora*, *Geum strictum*, *Spiræa salicifolia*, etc.

It will be yet more strengthened, and the obvious conclusion will become irresistible, when we take the nearly allied, as well as the identical, species into account. And also when we consider that, after excluding the identical species, only fifteen per cent. of the entries in the European column of the detailed tabular view are in italic type (*i. e.* are closely representative of Japanese species); while there are twenty-two per cent. of this character in the American column.

For the latter, I need only advert to some instances of such close representation, as of

Trollius patulus	by	T. Americanus,
Aguilegia Burgeriana	"	A. Canadensis,
Rhus vernicifera	"	R. venenata,
Celastras scandens	"	C. articulatus,
Negundo cissifolium	"	N. aceroides,
Sophora Japonica	"	S. affinis,
Sanguisorba tenuifolia	"	S. Canadensis,
Astilbe Thunbergii and Japonica	"	A. decandra,
Mitchella undulata	"	M. repens,
Hamamelis Japonica	"	H. Virginica,
Clethra barbinervis	"	C. acuminata,
Rhododendron brachycarpum	"	R. Catawbiense,
Amsonia elliptica	"	Tabernæmontana,
Saururus Loureiri	"	S. cernuus,

and many others of the same sort, — several of which, when better known, may yet prove to be conspecific; while an equally large number could be indicated of species which, altogether more positively different, are yet no less striking counterparts.

To demonstrate the former proposition, I have only to contrast the extra-American genera common to Europe and Japan with the extra-European genera common to North America and Japan. The principal European genera of this category are Adonis, Epimedium, Chelidonium, Malachium, Lotus, Anthriscus, Hedera, Asperula, Rubia, Carpesium, Ligularia, Lampsana, Picris, Pæderota, Ajuga, Thymus, Nepeta,

Lamium, Ligustrum, Kochia? Daphne, Thesium, Buxus, Mercurialis, Cephalanthera, Paris, Asparagus, — to which may as well be added Pæonia and Bupleurum, the former having a representative on the mountains, and the latter in the arctic regions, of western America, but both absent from the rest of our continent. Excepting Pæderota and Buxus (the latter a rather doubtful native of eastern Asia), none of these genera are peculiar to Europe, but all extend throughout Asia and elsewhere over large parts of the world.

The following incomplete list of North American genera or peculiar subgeneric types represented in Japan and its vicinity, but unknown in Europe, presents a very different appearance. Those which are absent from the flora of western North America are italicized.

Trautvetteria
Cimicifuga (barely reaches Europe)
Illicium
Magnolia
Cocculus and *Menispermum?*
Mahonia
Caulophyllum
Diphylleia
Brasenia
Nelumbium
Dicentra
Stuartia (& *Gordonia?*)
Zanthoxylum
Cissus
Ampelopsis
Berchemia
Æsculus
Sapindus
Negundo
Thermopsis
Wistaria
Desmodium
Lespedeza
Rhynchosia
Sophora
Photinia
Philadelphus
Penthorum
Hamamelis
Liquidambar
Cryptotænia
Cymopterus?
Archemora
Osmorrhiza
Aralia and § Ginseng
Echinopanax
Diervilla
Mitchella
Oldenlandia
(Siegesbeckia, in Mexico)
Cacalia (reaches E. Europe)
Gaultheria
Leucothoë
Pieris
Clethra
Menziesia
Symplocos
Ardisia
Boschniakia
Catalpa
Tecoma
Dicliptera
Asarum, § *Heterotropa*
Phytolacca
Benzoin and *Sassafras?*
Tatranthera
Saururus
Pachysandra
Laportea
Pilea
Bœhmeria
Microptelea
Maclura
Juglans
Abies, § Tsuga
Chamæcyparis
Torrea
Arisæma
Arctiodracon
Pogonia
Arethusa
Dioscorea
Aletris
Coprosmanthus
Trillium
Clintonia
Streptopus, § *Hekorima*
Chamælirium?
Sporobolus

Astilbe	*Leptandra*	*Arundinaria*
Mitella	*Callicarpa*	Adiantum
Hydrangea	*Cedronella*	*Onoclea*
Itea	*Amsonia*	

Here are about ninety extra-European genera or forms, sixty-four of which are absent from western North America out of the tropics (the latter comprising a very large part of the most striking representative species), and almost as many more are divided between North America and extra-tropical (chiefly northern and eastern) Asia. About forty of the latter genera are groups of single, or of two or few closely related species, peculiar, or nearly peculiar, to the regions just mentioned.

This list should be supplemented by those additional North American genera which have one or more closely representative species in the Himalayan region only, such as Podophyllum, Pyrularia, etc.; and also by the numerous cases in which eastern American plants are represented in the Himalayo-Japanese region by strikingly cognate, although not congeneric species; such as our Macrotys by Pityrosperma; Schizandra by Kadsura and Sphærostema; Neviusia by Kerria and Rhodotypus; Calycanthus by Chimonanthus; *Cornus florida* by Benthamia; Prosartes by Disporum; Helonias by Heloniopsis; and so of others, which have been mentioned in the former part of this memoir, and exhibited in the accompanying tabular view.

I had long ago, in Silliman's Journal, presented some data illustrative of this remarkable parallelism, and also more recently in my "Statistics of the Flora of the Northern United States" (vol. xxii., second series); where I had noticed the facts, — (1) that a large percentage of our extra-European types are shared with eastern Asia; and (2) that no small part of these are unknown in western North America. But Mr. Bentham was first to state the natural conclusion from all these data, — though I know not if he has even yet published the remark, — namely, that the interchange between the temperate floras even of the western part of the Old World and of the New has mainly taken place via Asia. Notwithstand-

ing the few cases which point in the opposite direction (*e. g. Eriocaulon septangulare*, Spartina, Subularia, *Betula alba*), the general statement will be seen to be well sustained. Also, in the "Journal of the Proceedings of the Linnæan Society," ii. p. 34, Mr. Bentham "calls to mind how frequently large American genera (such as Eupatorium, Aster, Solidago, Solanum, etc.) are represented in eastern Asia by a small number of species, which gradually diminish or altogether disappear as we proceed westward toward the Atlantic limits of Europe; whilst the types peculiar to the extreme west of Europe (excluding of course the arctic flora) are wholly deficient in America. These are among the considerations which suggest an ancient continuity of territory between America and Asia, under a latitude, or at any rate with a climate, more meridional than would be effected by a junction through the chains of the Aleutian and the Kurile Islands."

I shall presently state why connection in a more meridional latitude need not be supposed.

The deficiency in the temperate American flora of forms at all peculiar to western Europe is almost complete, and is most strikingly in contrast with the large number of eastern American forms repeated or represented in eastern Asia. Of genera divided between eastern North America and Europe, I can mention only Ostrya, Narthecium, Psamma, the maritime Cakile, and perhaps Scolopendrium. Hottonia might have been added, but for a species accredited to Java. And if we extend the range across our continent, we add only Cercis and Lœflingia. Of the ampler genera at all characteristic of the European flora, I can enumerate from the flora of the northern United States nothing more important than Helianthemum and Valerianella, two or three species of each (but those of the former hardly congeners of the European ones), adding that Hieracia and perhaps Cirsia are somewhat more plentiful in eastern than in western America. Let it also be noted, that there are even fewer western European types in the Pacific than in the Atlantic United States, notwithstanding the similarity of the climate!

That representation by allied species of genera peculiar or

nearly peculiar to two regions furnishes evidence of similar nature and of equal pertinency with representation by identical species, will hardly be doubted. Whether or not susceptible of scientific explanation, it is certain that related species of phænogamous plants are commonly associated in the same region, or are found in comparatively approximate (however large) areas of similar climate.[1] Remarkable exceptions may indeed be adduced, but the fact that they are remarkable goes to confirm the proposition. Indeed, the general expectation of botanists in this regard sufficiently indicates the common, implicit opinion. The discovery of a new Sarracenia or of a new Halesia in the Atlantic United States, or of a new Eschscholtzia, Platystemon, or Calais west of the Rocky Mountains, would excite no surprise. A converse discovery, or the detection of any of these genera in a remote region, would excite great surprise. The discovery of numerous closely related species thus divided between two widely separated districts might not, in the present state of our knowledge, suggest former continuity, migration, or interchange;

[1] The fundamental and most difficult question remaining in natural history is here presented — the question whether this actual geographical association of congeneric or other nearly related species is primordial, and therefore beyond all scientific explanation, or whether even this may be to a certain extent a natural result. The only noteworthy attempt at a scientific solution of the problem, aiming to bring the variety as well as the geographical association of existing species more within the domain of cause and effect, is that of Mr. Darwin and (later) of Mr. Wallace, — partially sketched in their short papers "On the Tendency of Species to form Varieties, and on the Perpetuation of Varieties and Species by natural Means of Selection," in the "Journal of the Proceedings of the Linnæan Society," vol. iii. (Zoölogy), p. 45. The views there suggested must bear a prominent part in future investigations into the distribution and probable origin of species. It will hardly be doubted that the tendencies and causes indicated are really operative; the question is as to the extent of their operation. But I am already disposed, on these and other grounds, to admit that what are termed closely related species may in many cases be lineal descendants from a pristine stock, just as domesticated races are; or, in other words, that the limits of occasional variation in species (if by them we mean primordial forms) are wider than is generally supposed, and that derivative forms when segregated may be as constantly reproduced as their originals.

but that of identical species peculiar to the two inevitably would.

Why should it? Evidently because the natural supposition is that individuals of the same kind are descendants from a common stock, or have spread from a common centre; and because the progress of investigation, instead of eliminating this preconception from the minds of botanists, has rather confirmed it. Every other hypothesis has derived its principal support from difficulties in the application of this. A review of what has been published upon the subject of late years makes it clear that the doctrine of the local origin of vegetable species has been more and more accepted, although, during the same period, species have been shown to be much more widely dispersed than was formerly supposed. Facts of the latter kind, and the conclusions to which they point, have been most largely and cogently brought out by Dr. Hooker, and are among the very important general results of his extensive investigations. And the best evidence of the preponderance of the theory of the local origin of species — notwithstanding the great increase of facts which at first would seem to tell the other way — is furnished by the works of the present De Candolle upon geographical botany. This careful and conscientious investigator formerly adopted and strenuously maintained Schouw's hypothesis of the double or multiple origin of species. But in his great work, the "Géographie Botanique Raisonnée," published in the year 1855, he has in effect discarded it, and this not from any theoretical objections to that view, but because he found it no longer needed to account for the general facts of distribution. This appears from his qualified though dubious adherence to the hypothesis of a double origin, as a *dernier ressort*, in the few and extraordinary cases which he could hardly explain in any other way. His decisive instance, indeed, is the occurrence of the eastern American *Phryma leptostachya* in the Himalaya Mountains.

The facts presented in the present memoir effectually dispose of this subsidiary hypothesis, by showing that the supposed single exception belongs to a not uncommon case.

Indeed, so many species are now known to be common to eastern and northern Asia and eastern North America, — some of them occurring also in northwestern America and some not, — and so many genera are divided between these two regions, that the antecedent improbability of such occurrence is done away, and more cases of the kind may be confidently expected. However others may regard them, it is clear that De Candolle would now explain these cases in accordance with the general views of distribution adopted by him, under which they naturally fall, — so abandoning the notion of a separate creation.

I know not whether any botanist continues to maintain Schouw's hypothesis. But its elements have been developed into a different and more comprehensive doctrine, that of Agassiz, which should now be contemplated. It may be denominated the autochthonal hypothesis.

In place of the ordinary conception, that each species originated in a local area, whence it has been diffused, according to circumstances, over more or less broad tracts, — in some cases becoming widely discontinuous in area through climatic or other physical changes operating during a long period of time, — Professor Agassiz maintains, substantially, that each species originated where it now occurs, probably in as great a number of individuals occupying as large an area, and generally the same area, or the same discontinuous areas as at the present time.

This hypothesis is more difficult to test, because more ideal than any other. It might suffice for the present purpose to remark, that, in referring the actual distribution, no less than the origin, of existing species to the Divine will, it would remove the whole question out of the field of inductive science. Regarded as a philosophical question, Maupertius's well-known "principle of least action" might be legitimately urged against it, namely, "that it is inconsistent with our idea of Divine wisdom that the Creator should use more power than was necessary to accomplish a given end." This philosophical principle holds so strictly true in all the mechanical adaptations of the universe, as Professor Pierce has

shown, that we cannot think it inapplicable to the organic world also, and especially to the creation of beings endowed with such enormous multiplying power, and such means and facilities for dissemination, as most plants and animals. Why then should we suppose the Creator to do that supernaturally which would be naturally effected by the very instrumentalities which he has set in operation?

Viewed, however, simply in its scientific applications to the question under consideration (the distribution of plants in the temperate zone of the northern hemisphere), the autochthonal hypothesis might be tested by inquiring whether the primitive or earliest range of our species could possibly have remained unaffected by the serious and prolonged climatic vicissitudes to which they must needs have been subject; and whether these vicissitudes, and their natural consequences, may not suffice to explain the partial intermingling of the floras of North America and northern Asia, upon the supposition of the local origin of each species. Let us bring to the inquiry the considerations which Mr. Darwin first brought to bear upon such questions, and which have been systematically developed and applied by the late Edward Forbes, by Dr. Hooker, and by Alphonse De Candolle.

No one now supposes that the existing species of plants are of recent creation, or that their present distribution is the result of a few thousand years. Various lines of evidence conspire to show that the time which has elapsed since the close of the tertiary period covers an immense number of years; and that our existing flora may in part date from the tertiary period itself. It is now generally admitted that about twenty per cent. of the Mollusca of the middle tertiary (miocene epoch), and forty per cent. of the pliocene species on the Atlantic coast still exist; and it is altogether probable that as large a portion of the vegetation may be of equal antiquity. From the nature of the case, the direct evidence as respects the flora could not be expected to be equally abundant. Still, although the fossil plants of the tertiary and the post-tertiary of North America have only now begun to be studied, the needful evidence is not wanting.

On our northwestern coast, in the miocene of Vancouver's Island, among a singular mixture of species referable to Salix, Populus, Quercus, Planera, Diospyros, Salisburia, Ficus, Cinnamomum, Personia, or other *Proteaceæ*, and a Palm (the latter genera decisively indicating a tropical or subtropical climate), Mr. Lesquereux has identified one existing species, a tree characteristic of the same region ten or fifteen degrees farther south, namely, the Redwood or *Sequoia sempervirens*. In beds at Somerville referred to the lower or middle pliocene by Mr. Lesquereux, this botanist has recently identified the leaves of *Persea Carolinensis*, *Prunus Caroliniana*, and *Quercus myrtifolia*, now inhabiting the warm seacoast and islands of the southern States.[1]

The pliocene quadrupeds of Nebraska also show that the climate east of the Rocky Mountains at this epoch was much warmer than now. About the upper Missouri and Platte there were then several species of Camel (Procamelus) and allied Ruminantia and a Rhinoceros, besides a Mastodon, an Elephant, some Horses and their allies, not to mention a corresponding number of carnivorous animals. These herbivora probably fed in a good degree upon herbage and grasses of still existing species. For herbs and grasses are generally capable of enduring much greater climatic changes, and are therefore likely to be even more ancient, than trees. These animals must have had at least a warm-temperate climate to live in: so that in latitude 40°-43° they could not have been anywhere near the northern limit of the temperate flora of those days; indeed the temperate flora, which now in western Europe touches the Arctic Circle, must then have reached equally high latitudes in central or western North America. In other words, the temperate floras of America and Asia must then have been conterminous (with small oceanic separation), and therefore have commingled, as conterminous floras of similar climate everywhere do.

At length, as the post-tertiary opened, the glacier epoch

[1] These and other data, obligingly communicated by Mr. Lesquereux, have been published in the May number of the "American Journal of Science and Arts," 3 ser., xvii.

came slowly on, — an extraordinary refrigeration of the northern hemisphere, in the course of ages carrying glacial ice and arctic climate down nearly to the latitude of the Ohio. The change was evidently so gradual that it did not destroy the temperate flora, at least not those enumerated above as existing species. These and their fellows, or such as survive, must have been pushed on to lower latitudes as the cold advanced, just as they now would be if the temperature were to be again lowered; and between them and the ice there was doubtless a band of subarctic and arctic vegetation, — portions of which, retreating up the mountains as the climate ameliorated and the ice receded, still scantily survive upon our highest Alleghanies, and more abundantly upon the colder summits of the mountains of New York and New England; — demonstrating the existence of the present arctic-alpine vegetation during the glacial era; and that the change of climate at its close was so gradual that it was not destructive to vegetable species.

As the temperature rose, and the ice gradually retreated, the surviving temperate flora must have returned northward *pari passu*, and — which is an important point — must have advanced much farther northward, and especially northwestward, than it now does; so far, indeed, that the temperate floras of North America and of eastern Asia, after having been for long ages most widely separated, must have become a second time conterminous. Whatever doubts may be entertained respecting the existence of our present vegetation generally before the glacial era, its existence immediately after that period will hardly be questioned. Here, therefore, may be adduced the direct evidence recently brought to light by Mr. Lesquereux, who has identified our Live Oak (*Quercus virens*), Pecan (*Carya olivæformis*), Chinquapin (*Castanea pumila*), Planer-tree (*Planera aquatica*), Honey-Locust (*Gleditschia triacanthos*), *Prinos coriaceus*, and *Acorus Calamus*, — besides an Elm and a Ceanothus doubtfully referable to existing species, — on the Mississippi, near Columbus, Kentucky, in beds which Mr. Lesquereux regards as anterior to the drift. Professor D. D. Owen has indicated their position "as about

one hundred and twenty feet lower than the ferrugineous sand in which the bones of the *Megalonyx Jeffersonii* were found." So that they belong to the period immediately succeeding the drift, if not to that immediately preceding it. All the vegetable remains of this deposit, which have been obtained in a determinable condition, have been referred, either positively or probably, to existing species of the United States flora, most of them now inhabiting the region a few degrees farther south.

If, then, our present temperate flora existed at the close of the glacial epoch, the evidence that it soon attained a high northern range is ready to our hand. For then followed the second epoch of the post-tertiary, called the fluvial by Dana, when the region of the St. Lawrence and Lake Champlain was submerged, and the sea there stood five hundred feet above its present level; when the higher temperate latitudes of North America, and probably the arctic generally, were less elevated than now, and the rivers vastly larger, as shown by the immense upper alluvial plains, from fifty to three hundred feet above their present beds; and when the diminished breadth and lessened height of northern land must have given a much milder climate than the present.

Whatever the cause, the milder climate of the fluvial epoch is undoubted. Its character, and therefore that of the vegetation, is decisively shown, as geologists have remarked, by the quadrupeds. While the Megatherium, Mylodon, Dicotyles, etc., demonstrate a warmer climate than at present in the southern and middle United States, the *Elephas primigenius*, ranging from Canada to the very shores of the Arctic Ocean, equally proves a temperate climate and a temperate flora in these northern regions. This is still more apparent in the species of the other continent, where, in Siberia, not only the *Elephas primigenius*, but also a Rhinoceros roamed northward to the arctic sea-coast. The quadrupeds that inhabited Europe in the same epoch are well known to indicate a warm temperate climate as far north as Britain, in the middle, if not the later post-tertiary. North America then had its herds of Mastodons, Elephants, Buffaloes or Bisons of dif-

ferent species, Elks, Horses, Megalonyx, the Lion, etc.; and, from the relations between this fauna and that of Europe, there is little doubt that the climate was as much milder than the present on this as on the other side of the ocean. All the facts known to us in the tertiary and post-tertiary, even to the limiting line of the drift, conspire to show that the difference between the two continents as to temperature was very nearly the same then as now, and that the isothermal lines of the northern hemisphere curved in the directions they now do.

A climate such as these facts demonstrate for the fluvial epoch would again commingle the temperate floras of the two continents at Behring's Straits, and earlier — probably through more land than now — by way of the Aleutian and Kurile Islands. I cannot imagine a state of circumstances under which the Siberian Elephant could migrate, and temperate plants could not.

The fluvial was succeeded by the "terrace epoch," as Dana names it, "a time of transition towards the present condition, bringing the northern part of the continent up to its present level and down to its present cool temperature," [1] — giving the arctic flora its present range, and again separating the temperate floras of the New and of the Old World to the extent they are now separated.

Under the light which these geological considerations throw upon the question, I cannot resist the conclusion, that the extant vegetable kingdom has a long and eventful history, and that the explanation of apparent anomalies in the geographical distribution of species may be found in the various and prolonged climatic or other physical vicissitudes to which they have been subject in earlier times; that the occurrence of certain species, formerly supposed to be peculiar to North America, in a remote or antipodal region affords itself no presumption that they were originated there, and that the interchange of plants between eastern North America and eastern Asia is explicable upon the most natural and generally received hy-

[1] For the collocation and communication of the geological data here presented, I am indebted to the kindness of my friend, Professor Dana.

pothesis (or at least offers no greater difficulty than does the arctic flora, the general homogeneousness of which round the world has always been thought compatible with local origin of the species), and is perhaps not more extensive than might be expected under the circumstances. That the interchange has mainly taken place in high northern latitudes, and that the isothermal lines have in earlier times turned northward on our eastern, and southward on our northwest coast, as they do now, are points which go far towards explaining why eastern North America, rather than Oregon and California, has been mainly concerned in this interchange, and why the temperate interchange, even with Europe, has principally taken place through Asia.

Brasenia peltata. — To the remarks upon the known range of this species, I have now to add the interesting fact, that it exists upon the northwestern coast of America, having been gathered by Dr. Pickering, in Wilkes's South Sea Exploring Expedition, in a stream which falls into Gray's Harbor, lat. 47°. It must be local on the western side of the continent, or it would have been met with before. When this remarkable plant was known to occur only in eastern North America and eastern Australia, it made the strongest case in favor of double creation that perhaps has ever been adduced. But since it has been found to occur throughout the eastern Himalayas and in Japan, and has now been detected in northwestern America also, the case seems to crown the conclusions to which this memoir arrives. (Note to reprint in "American Journal of Science and Arts," 3 ser., xviii. 199.)

SEQUOIA AND ITS HISTORY.[1]

THE session being now happily inaugurated, your presiding officer of the last year has only one duty to perform before he surrenders his chair to his successor. If allowed to borrow a simile from the language of my own profession, I might liken the President of this Association to a biennial plant. He flourishes for the year in which he comes into existence, and performs his appropriate functions as presiding officer. When the second year comes round, he is expected to blossom out in an address and disappear. Each President, as he retires, is naturally expected to contribute something from his own investigations or his own line of study, usually to discuss some particular scientific topic.

Now, although I have cultivated the field of North American botany, with some assiduity, for more than forty years, have reviewed our vegetable hosts, and assigned to no small number of them their names and their place in the ranks, yet, so far as our own wide country is concerned, I have been to a great extent a closet botanist. Until this summer I had not seen the Mississippi, nor set foot upon a prairie.

To gratify a natural interest, and to gain some title for addressing a body of practical naturalists and explorers, I have made a pilgrimage across the continent. I have sought and viewed in their native haunts many a plant and flower which for me had long bloomed unseen, or only in the *hortus siccus*. I have been able to see for myself what species and what forms constitute the main features of the vegetation of each successive region, and record — as the vegetation unerringly does — the permanent characteristics of its climate.

[1] The address of the retiring President of the American Association for the Advancement of Science. Delivered at Dubuque, Iowa, August, 1872. (Proceedings American Association, xxi. 1.)

Passing on from the eastern district, marked by its equably distributed rainfall, and therefore naturally forest-clad, I have seen the trees diminish in number, give place to wide prairies, restrict their growth to the borders of streams, and then disappear from the boundless drier plains; have seen grassy plains change into a brown and sere desert, — desert in the common sense, but hardly anywhere botanically so; have seen a fair growth of coniferous trees adorning the more favored slopes of a mountain range high enough to compel summer showers; have traversed that broad and bare elevated region shut off on both sides by high mountains from the moisture supplied by either ocean, and longitudinally intersected by sierras which seemingly remain as naked as they were born; and have reached at length the westward slopes of the high mountain barrier which, refreshed by the Pacific, bear the noble forests of the Sierra Nevada and the Coast Range, and among them trees which are the wonder of the world. As I stood in their shade, in the groves of Mariposa and Calaveras, and again under the canopy of the commoner Redwood, raised on columns of such majestic height and ample girth, it occurred to me that I could not do better than to share with you, upon this occasion, some of the thoughts which possessed my mind. In their development they may, perhaps, lead us up to questions of considerable scientific interest.

I shall not detain you with any remarks — which would now be trite — upon the size or longevity of these far-famed Sequoia trees, or of the Sugar Pines, Incense-Cedar, and Firs associated with them, of which even the prodigious bulk of the dominating Sequoia does not sensibly diminish the grandeur. Although no account and no photographic representation of either species of the far-famed Sequoia trees gives any adequate impression of their singular majesty — still less of their beauty, — yet my interest in them did not culminate merely or mainly in considerations of their size and age. Other trees, in other parts of the world, may claim to be older. Certain Australian Gum-trees (Eucalypti) are said to be taller. Some, we are told, rise so high that they might even

cast a flicker of shadow upon the summit of the pyramid of Cheops. Yet the oldest of them doubtless grew from seed which was shed long after the names of the pyramid-builders had been forgotten. So far as we can judge from the actual counting of the layers of several trees, no Sequoia now alive can sensibly antedate the Christian era.

Nor was I much impressed with an attraction of man's adding. That the more remarkable of these trees should bear distinguishing appellations seems proper enough; but the tablets of personal names which are affixed to many of them in the most visited groves — as if the memory of more or less notable people of our day might be made more enduring by the juxtaposition — do suggest some incongruity. When we consider that a hand's breadth at the circumference of any one of the venerable trunks so placarded has recorded in annual lines the lifetime of the individual thus associated with it, one may question whether the next hand's breadth may not measure the fame of some of the names thus ticketed for adventitious immortality. Whether it be the man or the tree that is honored in the connection, probably either would live as long, in fact and in memory, without it.

One notable thing about these Sequoia trees is their isolation. Most of the trees associated with them are of peculiar species, and some of them are nearly as local. Yet every Pine, Fir, and Cypress in California is in some sort familiar, because it has near relatives in other parts of the world. But the Redwoods have none. The Redwood — including in that name the two species of "Big-trees" — belongs to the general Cypress family, but is *sui generis*. Thus isolated systematically, and extremely isolated geographically, and so wonderful in size and port, they more than other trees suggest questions.

Were they created thus local and lonely, denizens of California only; one in limited numbers in a few choice spots on the Sierra Nevada, the other along the Coast Range from the Bay of Monterey to the frontiers of Oregon? Are they veritable Melchizedeks, without pedigree or early relationship, and possibly fated to be without descent?

Or are they now coming upon the stage — or rather were

they coming but for man's interference — to play a part in the future?

Or are they remnants, sole and scanty survivors of a race that has played a grander part in the past, but is now verging to extinction? Have they had a career, and can that career be ascertained or surmised, so that we may at least guess whence they came, and how, and when?

Time was, and not long ago, when such questions as these were regarded as useless and vain, — when students of natural history, unmindful of what the name denotes, were content with a knowledge of things as they now are, but gave little heed as to how they came to be so. Now, such questions are held to be legitimate, and perhaps not wholly unanswerable. It cannot now be said that these trees inhabit their present restricted areas simply because they are there placed in the climate and soil of all the world most congenial to them. These must indeed be congenial, or they would not survive. But when we see how Australian Eucalyptus trees thrive upon the Californian coast, and how these very Redwoods flourish upon another continent; how the so-called Wild Oat (*Avena sterilis* of the Old World) has taken full possession of California; how that cattle and horses, introduced by the Spaniard, have spread as widely and made themselves as much at home on the plains of La Plata as on those of Tartary; and that the Cardoon-thistle seeds, and others they brought with them, have multiplied there into numbers probably much exceeding those extant in their native lands; indeed, when we contemplate our own race, and our own particular stock, taking such recent but dominating possession of this New World; when we consider how the indigenous flora of islands generally succumbs to the foreigners which come in the train of man; and that most weeds (*i. e.*, the prepotent plants in open soil) of all temperate climates are not "to the manner born," but are self-invited intruders, — we must needs abandon the notion of any primordial and absolute adaptation of plants and animals to their habitats, which may stand in lieu of explanation, and so preclude our inquiring any further. The harmony of Nature and its admirable perfection need not be regarded as

inflexible and changeless. Nor need Nature be likened to a statue, or a cast in rigid bronze, but rather to an organism, with play and adaptability of parts, and life and even soul informing the whole. Under the former view, Nature would be "the faultless monster which the world ne'er saw," but inscrutable as the Sphinx, whom it were vain, or worse, to question of the whence and whither. Under the other, the perfection of Nature, if relative, is multifarious and ever renewed; and much that is enigmatical now may find explanation in some record of the past.

That the two species of Redwood we are contemplating originated as they are and where they are, and for the part they are now playing, is, to say the least, not a scientific supposition, nor in any sense a probable one. Nor is it more likely that they are destined to play a conspicuous part in the future, or that they would have done so, even if the Indian's fires and the white man's axe had spared them. The Redwood of the coast (*Sequoia sempervirens*) had the stronger hold upon existence, forming as it did large forests throughout a narrow belt about three hundred miles in length, and being so tenacious of life that every large stump sprouts into a copse. But it does not pass the Bay of Monterey, nor cross the line of Oregon, although so grandly developed not far below it. The more remarkable *Sequoia gigantea* of the Sierra exists in numbers so limited that the separate groves may be reckoned upon the fingers, and the trees of most of them have been counted, except near their southern limit, where they are said to be more copious. A species limited in individuals holds its existence by a precarious tenure; and this has a foothold only in a few sheltered spots, of a happy mean in temperature, and locally favored with moisture in summer. Even there, for some reason or other, the Pines with which they are associated (*Pinus Lambertiana* and *P. ponderosa*), the Firs (*Abies grandis* and *A. magnifica*), and even the Incense-Cedar (*Libocedrus decurrens*) possess a great advantage, and, though they strive in vain to emulate their size, wholly overpower the Sequoias in numbers. "To him that hath shall be given." The force of numbers eventually wins. At

least in the commonly visited groves *Sequoia gigantea* is invested in its last stronghold, can neither advance into more exposed positions above, nor fall back into drier and barer ground below, nor hold its own in the long-run where it is, under present conditions; and a little further drying of the climate, which must once have been much moister than now, would precipitate its doom. Whatever the individual longevity, certain if not speedy is the decline of a race in which a high death-rate afflicts the young. Seedlings of the big trees occur not rarely, indeed, but in meagre proportion to those of associated trees; and small indeed is the chance that any of these will attain to "the days of the years of their fathers." "Few and evil" are the days of all the forest likely to be, while man, both barbarian and civilized, torments them with fires, fatal at once to seedlings, and at length to the aged also. The forests of California, proud as the State may be of them, are already too scanty and insufficient for her uses. Two lines, such as may be drawn with one sweep of a brush over the map, would cover them all. The coast Redwood — the most important tree in California, although a million times more numerous than its relative of the Sierra — is too good to live long. Such is its value for lumber and its accessibility, that, judging the future by the past, it is not likely, in its primeval growth, to outlast its rarer fellow-species.

Happily man preserves and disseminates as well as destroys. The species will doubtless be preserved to science, and for ornamental and other uses, in its own and other lands; and the more remarkable individuals of the present day are likely to be sedulously cared for, all the more so as they become scarce.

Our third question remains to be answered: Have these famous Sequoias played in former times and upon a larger stage a more imposing part, of which the present is but the epilogue? We cannot gaze high up the huge and venerable trunks, which one crosses the continent to behold, without wishing that these patriarchs of the grove were able, like the long-lived antediluvians of Scripture, to hand down to us,

through a few generations, the traditions of centuries, and so tell us something of the history of their race. Fifteen hundred annual layers have been counted, or satisfactorily made out, upon one or two fallen trunks. It is probable that close to the heart of some of the living trees may be found the circle that records the year of our Saviour's nativity. A few generations of such trees might carry the history a long way back. But the ground they stand upon, and the marks of very recent geological change and vicissitude in the region around, testify that not very many such generations can have flourished just there, at least in an unbroken series. When their site was covered by glaciers, these Sequoias must have occupied other stations, if, as there is reason to believe, they then existed in the land.

I have said that the Redwoods have no near relatives in the country of their abode, and none of their genus anywhere else. Perhaps something may be learned of their genealogy by inquiring of such relatives as they have. There are only two of any particular nearness of kin; and they are far away. One is the Bald Cypress, our southern Cypress (Taxodium), inhabiting the swamps of the Atlantic coast from Maryland to Texas, thence extending — with, probably, a specific difference — into Mexico. It is well known as one of the largest trees of our Atlantic forest-district, and although it never — except perhaps in Mexico, and in rare instances — attains the portliness of its western relatives, yet it may equal them in longevity. The other relative is Glyptostrobus, a sort of modified Taxodium, being about as much like our Bald Cypress as one species of Redwood is like the other.

Now species of the same type, especially when few, and the type peculiar, are, in a general way, associated geographically, *i. e.*, inhabit the same country, or (in a large sense) the same region. Where it is not so, where near relatives are separated, there is usually something to be explained. Here is an instance. These four trees, sole representatives of their tribe, dwell almost in three separate quarters of the world: the two Redwoods in California, the Bald Cypress in Atlantic North America, its near relative, Glyptostrobus, in China.

It was not always so. In the tertiary period, the geological botanists assure us, our own very Taxodium or Bald Cypress, and a Glyptostrobus, exceedingly like the present Chinese tree, and more than one Sequoia, coexisted in a fourth quarter of the globe, namely, in Europe! This brings up the question: Is it possible to bridge over these four wide intervals of space and the much vaster interval of time, so as to bring these extraordinarily separated relatives into connection? The evidence which may be brought to bear upon this question is various and widely scattered. I bespeak your patience while I endeavor to bring together, in an abstract, the most important points of it.

Some interesting facts may come out by comparing generally the botany of the three remote regions, each of which is the sole home of one of these genera, *i. e.*, Sequoia in California, Taxodium in the Atlantic United States,[1] and Glyptostrobus in China, which compose the whole of the peculiar tribe under consideration.

Note then, first, that there is another set of three or four peculiar trees, in this case of the Yew family, which has just the same peculiar distribution, and which therefore may have the same explanation, whatever that explanation be. The genus Torreya, which commemorates our botanical Nestor and a former president of this association, Dr. Torrey, was founded upon a tree rather lately discovered (that is, about thirty-five years ago) in northern Florida. It is a noble, Yew-like tree, and very local, being, so far as known, nearly confined to a few miles along the shores of a single river. It seems as if it had somehow been crowded down out of the Alleghanies into its present limited southern quarters; for in cultivation it evinces a northern hardiness. Now another species of Torreya is a characteristic tree of Japan; and one very like it, if not the same, inhabits the mountains of north-

[1] The phrase "Atlantic United States" is here used throughout in contradistinction to Pacific United States. To the former of course belongs, botanically and geographically, the valley of the Mississippi and its tributaries up to the eastern border of the great woodless plains, which constitute an intermediate region.

ern China, — belongs, therefore, to the eastern Asiatic temperate region, of which northern China is a part, and Japan, as we shall see, the portion most interesting to us. There is only one more species of Torreya, and that is a companion of the Redwoods in California. It is the tree locally known under the name of the California Nutmeg. Here are three or four near brethren, species of the same genus, known nowhere else than in these three habitats.

Moreover, the Torreya of Florida is associated with a Yew; and the trees of this grove are the only Yew-trees of eastern North America; for the Yew of our northern woods is a decumbent shrub. A Yew-tree, perhaps the same, is found with Taxodium in the temperate parts of Mexico. The only other Yews in America grow with the Redwoods and the other Torreya in California, and extend northward into Oregon. Yews are also associated with Torreya in Japan; and they extend westward through Mandchuria and the Himalayas to western Europe, and even to the Azores Islands, where occurs the common Yew of the Old World.

So we have three groups of coniferous trees which agree in this peculiar geographical distribution, with, however, a notable extension of range in the case of the Yew: first, the Redwoods, and their relatives, Taxodium and Glyptostrobus, which differ so as to constitute a genus for each of the three regions; second, the Torreyas, more nearly akin, merely a different species in each region; third, the Yews, still more closely related while more widely disseminated, of which it is yet uncertain whether they constitute seven, five, three, or only one species. Opinions differ, and can hardly be brought to any decisive test. However it be determined, it may still be said that the extreme differences among the Yews do not surpass those of the recognized variations of the European Yew, the cultivated races included.

It appears to me that these several instances all raise the very same question, only with different degrees of emphasis, and, if to be explained at all, will have the same kind of explanation.

Continuing the comparison between the three regions with

which we are concerned, we note that each has its own species of Pines, Firs, Larches, etc., and of a few deciduous-leaved trees such as Oaks and Maples; all of which have no peculiar significance for the present purpose, because they are of genera which are common all round the northern hemisphere. Leaving these out of view, the noticeable point is that the vegetation of California is most strikingly unlike that of the Atlantic United States. They possess some plants, and some peculiarly American plants in common, — enough to show, as I imagine, that the difficulty was not in the getting from the one district to the other, or into both from a common source, but in abiding there. The primordially unbroken forest of Atlantic North America, nourished by rainfall distributed throughout the year, is widely separated from the western region of sparse and discontinuous tree-belts of the same latitude on the western side of the continent, where summer rain is wanting, or nearly so, by immense treeless plains and plateaux of more or less aridity, traversed by longitudinal mountain ranges of similar character. Their nearest approach is at the north, in the latitude of Lake Superior, where, on a more rainy line, trees of the Atlantic forest and that of Oregon may be said to interchange. The change of species and of the aspect of vegetation in crossing, say on the forty-seventh parallel, is slight in comparison with that on the thirty-seventh or near it. Confiding our attention to the lower latitude, and under the exceptions already specially noted, we may say that almost every characteristic form in the vegetation of the Atlantic States is wanting in California, and the characteristic plants and trees of California are wanting here.

California has no Magnolia nor Tulip trees, nor Star-anise-tree; no so-called Papaw (Asimina); no Barberry of the common single-leaved sort; no Podophyllum or other of the peculiar associated genera; no Nelumbo nor White Water-lily; no Prickly Ash nor Sumach; no Loblolly-bay nor Stuartia; no Basswood nor Linden-trees; neither Locust, Honey-locust, Coffee-trees (Gymnocladus), nor Yellow-wood (Cladrastis); nothing answering to Hydrangea or Witch-

hazel, to Gum-trees (Nyssa and Liquidambar), Viburnum or Diervilla; it has few Asters and Golden-rods; no Lobelias; no Huckleberries and hardly any Blueberries; no Epigæa, the charm of our earliest eastern spring, tempering an icy April wind with a delicious wild fragrance; no Kalmia nor Clethra, nor Holly, nor Persimmon; no Catalpa-tree, nor Trumpet-creeper (Tecoma); nothing answering to Sassafras, nor to Benzoin-tree, nor to Hickory; neither Mulberry nor Elm; no Beech, true Chestnut, Hornbeam, nor Ironwood, nor a proper Birch-tree; and the enumeration might be continued very much further by naming herbaceous plants and others familiar only to botanists.

In their place California is filled with plants of other types, — trees, shrubs, and herbs, of which I will only remark that they are, with one or two exceptions, as different from the plants of the eastern Asiatic region with which we are concerned (Japan, China, and Mandchuria), as they are from those of Atlantic North America. Their near relatives, when they have any in other lands, are mostly southward, on the Mexican plateau, or many as far south as Chili. The same may be said of the plants of the intervening great plains, except that northward and in the subsaline vegetation there are some close alliances with the flora of the steppes of Siberia. And along the crests of high mountain ranges the arctic-alpine flora has sent southward more or less numerous representatives through the whole length of the country.

If we now compare, as to their flora generally, the Atlantic United States with Japan, Mandchuria, and northern China, — *i. e.*, eastern North America with eastern north Asia, half the earth's circumference apart, — we find an astonishing similarity. The larger part of the genera of our own region, which I have enumerated as wanting in California, are present in Japan or Mandchuria, along with many other peculiar plants, divided between the two. There are plants enough of the one region which have no representatives in the other. There are types which appear to have reached the Atlantic States from the south; and there is a larger infusion of subtropical Asiatic types into temperate China and Japan;

among these there is no relationship between the two countries to speak of. There are also, as I have already said, no small number of genera and some species which, being common all round or partly round the northern temperate zone, have no special significance because of their occurrence in these two antipodal floras, although they have testimony to bear upon the general question of geographical distribution. The point to be remarked is, that many, or even most, of the genera and species which are peculiar to North America as compared with Europe, and largely peculiar to Atlantic North America as compared with the Californian region, are also represented in Japan and Mandchuria, either by identical or by closely similar forms. The same rule holds on a more northward line, although not so strikingly. If we compare the plants, say of New England and Pennsylvania (lat. 45°–47°), with those of Oregon, and then with those of north-eastern Asia, we shall find many of our own curiously repeated in the latter, while only a small number of them can be traced along the route even so far as the western slope of the Rocky Mountains. And these repetitions of east American types in Japan and neighboring districts are in all degrees of likeness. Sometimes the one is undistinguishable from the other; sometimes there is a difference of aspect, but hardly of tangible character; sometimes the two would be termed marked varieties if they grew naturally in the same forest or in the same region; sometimes they are what the botanist calls representative species, the one answering closely to the other, but with some differences regarded as specific; sometimes the two are merely of the same genus, or not quite that, but of a single or very few species in each country; when the point which interests us is, that this peculiar limited type should occur in two antipodal places, and nowhere else.

It would be tedious, and, except to botanists, abstruse, to enumerate instances; yet the whole strength of the case depends upon the number of such instances. I propose therefore, if the Association does me the honor to print this discourse, to append in a note a list of the more remarkable

ones.[1] But I would here mention certain cases as specimens.

Our *Rhus Toxicodendron*, or Poison Ivy, is very exactly repeated in Japan, but is found in no other part of the world, although a species much like it abounds in California. Our other poisonous Rhus (*R. venenata*), commonly called Poison Dogwood, is in no way represented in western America, but has so close an analogue in Japan that the two were taken for the same by Thunberg and Linnæus, who called them both *R. Vernix*.

Our northern Fox-grape, *Vitis Labrusca*, is wholly confined to the Atlantic States, except that it reappears in Japan and that region.

The original Wistaria is a woody leguminous climber with showy blossoms, native to the middle Atlantic States; the other species, which we so much prize in cultivation, *W. Sinensis*, is from China, as its name denotes, or perhaps only from Japan, where it is certainly indigenous.

Our Yellow-wood (Cladrastis) inhabits a very limited district on the western slope of the Alleghanies. Its only and very near relative, Maackia, is in Mandchuria.

The Hydrangeas have some species in our Alleghany region; all the rest belong to the Chino-Japanese region and its continuation westward. The same may be said of Philadelphus, except that there are one or two mostly very similar species in California and Oregon.

Our Blue Cohosh (Caulophyllum) is confined to the woods of the Atlantic States, but has lately been discovered in Japan.[2] A peculiar relative of it, Diphylleia, confined to the higher Alleghanies, is also repeated in Japan, with a slight difference, so that it may barely be distinguished as another species. Another relative is our Twin-leaf (Jeffersonia) of the Alleghany region alone; a second species has lately turned up in Mandchuria. A relative of this is Podophyllum, our Mandrake, a common inhabitant of the Atlantic United States, but found nowhere else. There is one other species of it, and that is in the Himalayas. Here are four most

[1] See Appendix, I.

[2] Appendix, II.

peculiar genera of one family, each of a single species in the Atlantic United States, which are duplicated on the other side of the world, either in identical species, or in an analogous species, while nothing else of the kind is known in any other part of the world.

I ought not to omit Ginseng, the root so prized by the Chinese, which they obtained from their northern provinces and Mandchuria, and which is now known to inhabit Corea and northern Japan. The Jesuit Fathers identified the plant in Canada and the Atlantic States, brought over the Chinese name by which we know it, and established the trade in it, which was for many years most profitable. The exportation of Ginseng to China probably has not yet entirely ceased. Whether the Asiatic and the Atlantic American Ginsengs are to be regarded as of the same species or not is somewhat uncertain, but they are hardly, if at all, distinguishable.

There is a shrub, Elliottia, which is so rare and local that it is known only at two stations on the Savannah River, in Georgia. It is of peculiar structure, and was without near relative until one was lately discovered in Japan (Tripetaleia), so like it as hardly to be distinguishable except by having the parts of the blossom in threes instead of fours, — a difference which is not uncommon in the same genus, or even in the same species.

Suppose Elliottia had happened to be collected only once, a good while ago, and all knowledge of the limited and obscure locality were lost; and meanwhile the Japanese form came to be known. Such a case would be parallel with an actual one. A specimen of a peculiar plant (*Shortia galacifolia*) was detected in the herbarium of the elder Michaux, who collected it (as his autograph ticket shows) somewhere in the high Alleghany Mountains, more than eighty years ago. No one has seen the living plant since or knows where to find it, if haply it still flourishes in some secluded spot. At length it is found in Japan; and I had the satisfaction of making the identification.[1] One other relative is also known in Japan; and another, still unpublished, has just been detected in Thibet.

[1] "Amer. Jour. Science," 1867, p. 402; "Proc. Amer. Acad.," viii. p. 244.

Whether the Japanese and the Alleghanian plants are exactly the same or not, it needs complete specimens of the two to settle. So far as we know, they are just alike; and even if some difference were discerned between them, it would not appreciably alter the question as to how such a result came to pass. Each and every one of the analogous cases I have been detailing — and very many more could be mentioned — raises the same question, and would be satisfied with the same answer.

These singular relations attracted my curiosity early in the course of my botanical studies, when comparatively few of them were known, and my serious attention in later years, when I had numerous and new Japanese plants to study in the collections made by Messrs. Williams and Morrow, during Commodore Perry's visit in 1853, and especially by Mr. Charles Wright, in Commodore Rodgers's expedition in 1855. I then discussed this subject somewhat fully, and tabulated the facts within my reach.[1]

This was before Heer had developed the rich fossil botany of the arctic zone, before the immense antiquity of existing species of plants was recognized, and before the publication of Darwin's now famous volume on the "Origin of Species" had introduced and familiarized the scientific world with those now current ideas respecting the history and vicissitudes of species with which I attempted to deal in a moderate and feeble way.

My speculation was based upon the former glaciation of the northern temperate zone, and the inference of a warmer period preceding and perhaps following. I considered that our own present vegetation, or its proximate ancestry, must have occupied the arctic and subarctic regions in pliocene times, and that it had been gradually pushed southward as the temperature lowered and the glaciation advanced, even beyond its present habitation; that plants of the same stock and kindred, probably ranging round the arctic zone as the present arctic species do, made their forced migration southward upon widely different longitudes, and receded more or less as the climate

[1] "Mem. Amer. Acad.," vol. vi. pp. 377–458 (1859).

grew warmer; that the general difference of climate which marks the eastern and the western sides of the continents — the one extreme, the other mean — was doubtless even then established, so that the same species and the same sorts of species would be likely to secure and retain foothold in the similar climates of Japan and the Atlantic United States, but not in intermediate regions of different distribution of heat and moisture; so that different species of the same genus, as in Torreya, or different genera of the same group, as Redwood, Taxodium, and Glyptostrobus, or different associations of forest trees, might establish themselves each in the region best suited to their particular requirements, while they would fail to do so in any other. These views implied that the sources of our actual vegetation and the explanation of these peculiarities were to be sought in, and presupposed, an ancestry in pliocene or still earlier times, occupying the higher northern regions. And it was thought that the occurrence of peculiarly North American genera in Europe in the tertiary period (such as Taxodium, Carya, Liquidambar, Sassafras, Negundo, etc.), might be best explained on the assumption of early interchange and diffusion through north Asia, rather than by that of the fabled Atlantis.

The hypothesis supposed a gradual modification of species in different directions under altering conditions, at least to the extent of producing varieties, sub-species, and representative species, as they may be variously regarded; likewise the single and local origination of each type, which is now almost universally taken for granted.

The remarkable facts in regard to the eastern American and Asiatic floras which these speculations were to explain have since increased in number, more especially through the admirable collections of Dr. Maximowicz in Japan and adjacent countries, and the critical comparisons he has made and is still engaged upon.

I am bound to state that, in a recent general work[1] by a distinguished European botanist, Professor Grisebach, of

[1] "Die Vegetation der Erde nach ihrer klimatischen Anordnung." 1871.

Göttingen, these facts have been emptied of all special significance, and the relations between the Japanese and the Atlantic United States flora declared to be no more intimate than might be expected from the situation, climate, and present opportunity of interchange. This extraordinary conclusion is reached by regarding as distinct species all the plants common to both countries between which any differences have been discerned, although such differences would probably count for little if the two inhabited the same country, thus transferring many of my list of identical to that of representative species; and then by simply eliminating from consideration the whole array of representative species, *i. e.*, all cases in which the Japanese and the American plant are not exactly alike. As if, by pronouncing the cabalistic word species, the question were settled, or rather the greater part of it remanded out of the domain of science; as if, while complete identity of forms implied community of origin, anything short of it carried no presumption of the kind; so leaving all these singular duplicates to be wondered at, indeed, but wholly beyond the reach of inquiry.[1]

Now the only known cause of such likeness is inheritance: and as all transmission of likeness is with some difference in individuals, and as changed conditions have resulted, as is well known, in very considerable differences, it seems to me that, if the high antiquity of our actual vegetation could be rendered probable, not to say certain, and the former habitation of any of our species or of very near relatives of them in high northern regions could be ascertained, my whole case would be made out. The needful facts, of which I was ignorant when my essay was published, have now been for some years made known, — thanks, mainly, to the researches of Heer upon ample collections of arctic fossil plants. These are confirmed and extended by new investigations, by Heer and Lesquereux, the results of which have been indicated to me by the latter.[2]

[1] See Appendix, II.

[2] Reference should also be made to the extensive researches of Newberry upon the tertiary and cretaceous floras of the western United

The Taxodium, which everywhere abounds in the miocene formations in Europe, has been specifically identified, first by Gœppert, then by Heer, with our common Cypress of the southern States. It has been found fossil in Spitzbergen, Greenland, and Alaska, — in the latter country along with the remains of another form, distinguishable, but very like the common species; and this has been identified by Lesquereux in the miocene of the Rocky Mountains. So there is one species of tree which has come down essentially unchanged from the tertiary period, which for a long while inhabited both Europe and North America, and also, at some part of the period, the region which geographically connects the two (once doubtless much more closely than now), but which has survived only in the Atlantic United States and Mexico.

The same Sequoia which abounds in the same miocene formations in northern Europe has been abundantly found in those of Iceland, Spitzbergen, Greenland, Mackenzie River, and Alaska. It is named *S. Longsdorfii*, but is pronounced to be very much like *S. sempervirens*, our living Redwood of the Californian coast, and to be the ancient representative of it. Fossil specimens of a similar, if not the same, species have recently been detected in the Rocky Mountains by Hayden, and determined by our eminent palæontological botanist Lesquereux; and he assures me that he has the common Redwood itself from Oregon in a deposit of tertiary age. Another Sequoia (*S. Sternbergii*), discovered in miocene deposits in Greenland, is pronounced to be the representative of *S. gigantea*, the Big Tree of the Californian Sierra. If the

States. See especially Professor Newberry's Paper in the "Boston Journal of Natural History," vol. vii. No. 4, describing fossil plants of Vancouver's Island, etc.; his "Notes on the Later Extinct Floras of North America," etc., in "Annals of the Lyceum of Natural History," vol. ix., April, 1868; "Report on the Cretaceous and Tertiary Plants collected in Raynolds and Hayden's Yellowstone and Missouri Exploring Expedition, 1859-1860," published in 1869; and an interesting article entitled "The Ancient Lakes of Western America, their Deposits and Drainage," published in "The American Naturalist," January, 1871.

The only document I was able to consult was Lesquereux's Report on the Fossil Plants, in Hayden's Report of 1872.

Taxodium of the tertiary time in Europe and throughout the arctic regions is the ancestor of our present Bald Cypress, — which is assumed in regarding them as specifically identical, — then I think we may, with our present light, fairly assume that the two Redwoods of California are the direct or collateral descendants of the two ancient species which so closely resemble them.

The forests of the arctic zone in tertiary times contained at least three other species of Sequoia, as determined by their remains, one of which, from Spitzbergen, also much resembles the common Redwood of California. Another, "which appears to have been the commonest coniferous tree on Disco," was common in England and some other parts of Europe. So the Sequoias, now remarkable for their restricted station and numbers, as well as for their extraordinary size, are of an ancient stock: their ancestors and kindred formed a large part of the forests, which flourished throughout the polar regions, now desolate and ice-clad, and which extended into low latitudes in Europe. On this continent one species, at least, had reached to the vicinity of its present habitat before the glaciation of the region. Among the fossil specimens already found in California, but which our trustworthy palæontological botanist has not yet had time to examine, we may expect to find evidence of the early arrival of these two Redwoods upon the ground which they now, after much vicissitude, scantily occupy.

Differences of climate, or circumstances of migration, or both, must have determined the survival of Sequoia upon the Pacific, and of Taxodium upon the Atlantic coast. And still the Redwoods will not stand in the east, nor could our Taxodium find a congenial station in California. Both have probably had their opportunity in the olden time, and failed.

As to the remaining near relative of Sequoia, the Chinese Glyptostrobus, a species of it, and its veritable representative, was contemporaneous with Sequoia and Taxodium, not only in temperate Europe, but throughout the arctic regions from Greenland to Alaska. According to Newberry, it was abundantly represented in the miocene flora of the temperate zone of our own continent, from Nebraska to the Pacific.

Very similar would seem to have been the fate of a more familiar gymnospermous tree, the Gingko or Salisburia. It is now indigenous to Japan only. Its ancestor, as we may fairly call it, — since, according to Heer, "it corresponds so entirely with the living species that it can scarcely be separated from it," — once inhabited northern Europe and the whole arctic region round to Alaska, and had even a representative farther south, in our Rocky Mountain district. For some reason, this and Glyptostrobus survive only on the shores of eastern Asia.

Libocedrus, on the other hand, appears to have cast in its lot with the Sequoias. Two species, according to Heer, were with them in Spitzbergen. *L. decurrens*, the Incense Cedar, is one of the noblest associates of the present Redwoods. But all the rest are in the southern hemisphere, two at the southern extremity of the Andes, two in the South Sea Islands. It is only by bold and far-reaching suppositions that they can be geographically associated.

The genealogy of the Torreyas is still wholly obscure; yet it is not unlikely that the Yew-like trees, named Taxites, which flourished with the Sequoias in the tertiary arctic forests, are the remote ancestors of the three species of Torreya, now severally in Florida, in California, and in Japan.

As to the Pines and Firs, these were more numerously associated with the ancient Sequoias of the polar forests than with their present representatives, but in different species, apparently more like those of eastern than of western North America. They must have encircled the polar zone then, as they encircle the present temperate zone now.

I must refrain from all enumeration of the angiospermous or ordinary deciduous trees and shrubs, which are now known, by their fossil remains, to have flourished throughout the polar regions when Greenland better deserved its name and enjoyed the present climate of New England and New Jersey. Then Greenland and the rest of the north abounded with Oaks, representing the several groups of species which now inhabit both our eastern and western forest districts; several Poplars, one very like our Balsam Poplar, or Balm of

Gilead-tree; more Beeches than there are now, a Hornbeam, and a Hop-Hornbeam, some Birches, a Persimmon, and a Planer-tree, near representatives of those of the Old World, at least of Asia, as well as of Atlantic North America, but all wanting in California; one Juglans like the Walnut of the Old World, and another like our Black Walnut; two or three Grapevines, one near our southern Fox Grape or Muscadine, another near our northern Frost Grape; a Tilia, very like our Basswood of the Atlantic States only; a Liquidambar; a Magnolia, which recalls our *M. grandiflora;* a Liriodendron, sole representative of our Tulip-tree; and a Sassafras, very like the living tree.

Most of these, it will be noticed, have their nearest or their only living representatives in the Atlantic States, and when elsewhere, mainly in eastern Asia. Several of them, or of species like them, have been detected in our tertiary deposits, west of the Mississippi, by Newberry and Lesquereux. Herbaceous plants, as it happens, are rarely preserved in a fossil state, else they would probably supply additional testimony to the antiquity of our existing vegetation, its wide diffusion over the northern and now frigid zone, and its enforced migration under changes of climate.[1]

Concluding, then, as we must, that our existing vegetation is a continuation of that of the tertiary period, may we suppose that it absolutely originated then? Evidently not. The preceding cretaceous period has furnished to Carruthers in Europe a fossil fruit like that of the *Sequoia gigantea* of the famous groves, associated with Pines of the same character as those that accompany the present tree; has furnished to Heer,

[1] There is at least one instance so opportune to the present argument that it should not pass unnoticed, although I had overlooked the record until now. *Onoclea sensibilis* is a Fern peculiar to the Atlantic United States (where it is common and widespread) and to Japan. Professor Newberry identified it several years ago in a collection obtained by Dr. Hayden of miocene fossil plants of Dacota Territory, which is far beyond its present habitat. He moreover regards it as probably identical with a fossil specimen "described by the late Professor E. Forbes, under the name of *Filicites Hebridicus*, and obtained by the Duke of Argyll from the Island of Mull."

from Greenland, two more Sequoias, one of them identical with a tertiary species, and one nearly allied to *Sequoia Langsdorfii*, which in turn is a probable ancestor of the common Californian Redwood; has furnished to Newberry and Lesquereux in North America the remains of another ancient Sequoia, a Glyptostrobus, a Liquidambar which well represents our Sweet Gum, Oaks analogous to living ones, leaves of a Plane-tree, which are also in the tertiary and are scarcely distinguishable from our own *Platanus occidentalis*, of a Magnolia and a Tulip-tree, and "of a Sassafras undistinguishable from our living species." I need not continue the enumeration. Suffice it to say that the facts justify the conclusion which Lesquereux — a scrupulous investigator — has already announced: "that the essential types of our actual flora are marked in the cretaceous period, and have come to us after passing, without notable changes, through the tertiary formations of our continent."

According to these views, as regards plants at least, the adaptation to successive times and changed conditions has been maintained, not by absolute renewals, but by gradual modifications. I, for one, cannot doubt that the present existing species are the lineal successors of those that garnished the earth in the old time before them, and that they were as well adapted to their surroundings then, as those which flourish and bloom around us are to their conditions now. Order and exquisite adaptation did not wait for man's coming, nor were they ever stereotyped. Organic nature, — by which I mean the system and totality of living things, and their adaptation to each other and to the world, — with all its apparent and indeed real stability, should be likened, not to the ocean, which varies only by tidal oscillations from a fixed level to which it is always returning, but rather to a river, so vast that we can neither discern its shores nor reach its sources, whose onward flow is not less actual because too slow to be observed by the ephemeræ which hover over its surface, or are borne upon its bosom.

Such ideas as these, though still repugnant to some, and not long since to many, have so possessed the minds of the

naturalists of the present day, that hardly a discourse can be pronounced or an investigation prosecuted without reference to them. I suppose that the views here taken are little, if at all, in advance of the average scientific mind of the day. I cannot regard them as less noble than those which they are succeeding.

An able philosophical writer, Miss Frances Power Cobbe, has recently and truthfully said : [1]

" It is a singular fact, that when we can find out how anything is done, our first conclusion seems to be that God did not do it. No matter how wonderful, how beautiful, how intimately complex and delicate has been the machinery which has worked, perhaps for centuries, perhaps for millions of ages, to bring about some beneficent result, if we can but catch a glimpse of the wheels its divine character disappears."

I agree with the writer that this first conclusion is premature and unworthy, — I will add, deplorable. Through what faults or infirmities of dogmatism on the one hand, and skepticism on the other, it came to be so thought, we need not here consider. Let us hope, and I confidently expect, that it is not to last ; that the religious faith which survived without a shock the notion of the fixity of the earth itself may equally outlast the notion of the absolute fixity of the species which inhabit it ; that in the future even more than in the past, faith in an order, which is the basis of science, will not — as it cannot reasonably — be dissevered from faith in an Ordainer, which is the basis of religion.

APPENDIX.

I.

In the following table the names in the left-hand column are from my " Manual of the Botany of the Northern United States," and from Dr. Chapman's " Flora of the Southern United States," the two together comprehending the flora of the Atlantic United States east of the Mississippi River. Alpine plants on the one hand, and subtropical plants on the other, are excluded.

[1] " Darwinism in Morals," in " Theological Review," April, 1871.

The entries in the middle column, when there are any, are of identical or representative species occurring in Oregon or California.

Those in the right-hand column are of such species in Japan, or other parts of northeastern Asia, including the Himalayas and Siberia as far west as the Altai Mountains.

When these are not identical, or so closely related to the American species that the one may be said strictly to represent the other, also when genera or parts of genera are adduced merely as representing the same type in these respective regions, the names are included in parentheses.

Species which extend through Europe into northeastern Asia, and therefore nearly round the temperate zone, are also left out of view, the object being to consider the peculiar relations of the floras of eastern North America and eastern temperate Asia. The table has been drawn up off-hand, from the means within reach. Probably the example might be considerably increased.

EXTRA-EUROPEAN (TEMPERATE) GENERA AND SPECIES OF THE ATLANTIC UNITED STATES (*i. e.*, EAST OF THE MISSISSIPPI) REPRESENTED BY IDENTICAL OR STRICTLY REPRESENTATIVE SPECIES, OR ELSE BY LESS INTIMATELY RELATED SPECIES (THE LATTER INCLUDED IN PARENTHESES).

	1. *In the Pacific United States.*	2. *In Northeastern Asia, Japan to Altai and the Himalayas.*
Anemone Pennsylvanica.		Anemone dichotoma = Pennsylvanica.
" parviflora.		Anemone parviflora?
Ranunculus alismæfolius.	Ranunculus alismæfolius.	Ranunculus alismæfolius.
" Cymbalaria.	" Cymbalaria.	" Cymbalaria.
" Gmelini.	" Gmelini.	" Gmelini.
" Pennsylvanicus.	" Pennsylvanicus.	" Pennsylvanicus.
Trautvetteria palmata.	Trautvetteria palmata.	Trautvetteria palmata.
Hydrastis Canadensis.		Hydrastis Jesoenis.
Trollius Americanus.	Trollius Americanus var.	Trollius patulus var. = Americanus, Ledeb.
Aconitum uncinatum.		Aconitum uncinatum ex Hook. f.
Actæa spicata, var. rubra.	Actæa spicata, var. arguta.	Actæa spicata, var. rubra.
" alba.		" alba?
Cimicifuga Americana.		Cimicifuga fœtida, barely occurs in N. Europe also.
Cimicifuga racemosa and cordifolia.	(Cimicifuga elata.)	(Cimicifuga Dahurica and § Pityrosperma, 3 spp.)
Illicium Floridanum and parviflorum.		Illicium anisatum, religiosum, etc.
Schizandra coccinea.		(Schizandra nigra, etc.)
Magnolia, 7 spp.		(Magnolia, 8–12 spp.)
Menispermum Canadense.		Menispermum Dahuricum.
Caulophyllum thalictroides.[1]		Caulophyllum thalictroides.
Diphyllæia cymosa.		Diphylleia Grayi.
Jeffersonia diphylla.		Jeffersonia = Plagiorhegma dubium.
Podophyllum peltatum.		(Podophyllum Emodi.)
Brasenia peltata.	Brasenia peltata.	Brasenia peltata.
Nelumbium luteum.		Nelumbium speciosum.
Stylophorum diphyllum.		(Stylophorum Japonicum and lactucoides.)

[1] See Appendix, II.

Dicentra eximia.	Dicentra eximia or formosa	(Dicentra spp.)
Corydalis aurea.	Corydalis aurea var.	Corydalis aurea var., etc.
Viola Selkirkii.		Viola Selkirkii.
" Canadensis.	Viola Canadensis var.	" Canadensis var.
Claytonia, Virginica and Caroliniana.	Claytonia lanceolata.	(Claytonia, spp. Siberia.)
Elodes Virginica.		Elodes Virginica.
" petiolata.		" petiolata.
Tilia Americana (American type).		Tilia sp., American type, one of which reaches Hungary.
Tilia heterophylla.		
Stuartia, 2 spp.		(Stuartia, 3 spp.)
Xanthoxylum spp.		(Xanthoxylum spp.)
Rhus venenata.		Rhus vernicifera, etc.
" Toxicodendron.	(Rhus diversiloba.)	" Toxicodendron.
Vitis Labrusca.		Vitis Labrusca.
" indivisa.		" humulifolia.
Ampelopsis quinquefolia.		(Ampelopsis tricuspidata.)
Berchemia volubilis.		(Berchemia racemosa, etc.)
Sageretia Michauxii.		(Sageretia theæsans.)
Celastrus scandens.		(Celastrus, 5 spp.)
Æsculus glabra.		Æsculus Chinensis and Hippocastanum.
" flava and Pavia.		(Æsculus dissimilis.)
" parviflora.	(Æsculus Californica.)	(" Punduana Wall.)
Acer spicatum.		Acer spicatum var.
" Pennsylvanicum.		" tegmentosum.
Negundo aceroides.	Negundo aceroides Californicum.	(Negundo cissifolium and spp.)
Wistaria frutescens.		Wistaria Sinensis and spp.
Desmodium, many spp.		(Desmodium, several spp.)
Lespedeza spp.		(Lespedeza, spp.)
Rhynchosia spp.		(Rhynchosia, sp.)
Amphicarpæa monoica.		(Amphicarpæa, 5 spp.)
Thermopsis, 3 spp.	(Thermopsis fabacea & sp.)	Thermopsis fabacea.
Cladrastis tinctoria.		(Maackia Amurensis.)
Cassia spp.		(Cassia spp.)
Gleditschia triacanthos and monosperma.		Gleditschia Chinensis, etc.
Neptunia lutea.		(Neptunia spp.)
Spiræa (Neillia) opulifolia.	Spiræa opulifolia.	(Neillia spp., Himalayas.)
" corymbosa.	" betulæfolia.	Spiræa betulæfolia.
Neviusa Alabamensis.		(Stephanandra, Kerria.)
Geum macrophyllum.	Geum macrophyllum.	Geum Japonicum.
Potentilla Pennsylvanica.	Potentilla Pennsylvanica.	Potentilla Pennsylvanica.
Rubus triflorus.		Rubus triflorus, var. Japonicus.
" strigosus.	Rubus strigosus.	" strigosus.
Pyrus Americana and sambucifolia.	Pyrus sambucifolia.	Pyrus Americana and sambucifolia.
Amelanchier Canadensis and vars.	Amelanchier Canadensis var.	Amelanchier Canadensis var.
Calycanthus, 3 spp.	Calycanthus occidentalis.	(Chimonanthus fragrans.)
Ribes Cynosbati.	(Ribes spp.)	Ribes Cynosbati.
" lacustre.	" setosum.	" lacustre.
" prostratum.	" laxiflorum.	" laxiflorum.
Philadelphus, 2 spp.	Philadelphus, 2 spp.	Philadelphus spp.
Itea Virginica.		(Itea spp.)
Hydrangea, 3 spp.		Hydrangea, many spp.
Astilbe decandra.		Astilbe Thunbergii and spp.
Boykinia aconitifolia.	Boykinia occidentalis and elata.	(Boykinia? = Saxifr. tellimioides Maxim.)
Mitella nuda.	Mitella nuda.	Mitella nuda, Siberia.
" diphylla.	Mitella § Mitellastra, etc., spp.	(Mitella § Mitellastra, sp.)
Tiarella cordifolia.	Tiarella unifoliata.	Tiarella polyphylla.
Penthorum sedoides.		Penthorum sedoides ? = Chinense and humile.
Hamamelis Virginica.		Hamamelis Japonica, etc.
Fothergilla alnifolia.		(Corylopsis spp., etc.)
Heracleum lanatum.	Heracleum lanatum.	Heracleum lanatum.
Archangelica Gmelini.	Archangelica Gmelini.	Archangelica Gmelini.
Sium lineare.		Sium cicutæfolium.
Cryptotænia Canadensis.		Cryptotænia Canadensis.
Archemora, 2 spp.		(Peucedanum ? Sieboldii.)

Osmorrhiza longistylis and brevistylis.	Osmorrhiza longistylis, etc.	Osmorrhiza longistylis, etc.
Aralia spinosa.		Aralia spinosa var.
" racemosa.	Aralia humilis.	" edulis, etc.
" nudicaulis.		(" cordata.)
" (Ginseng) quinquefolia.		" repens, Ginseng, etc.
Cornus Canadensis.	Cornus Canadensis.	Cornus Canadensis.
" florida.	" Nuttallii.	Benthamia spp.
" stolonifera.		" alba.
Diervilla, 2 spp.		(Diervilla § Weigela, spp.)
Triosteum, 2 spp.		Triosteum sinuatum and Himalaicum.
Viburnum lantanoides.		Viburnum lantanoides (and related species.)
" dentatum & pubescens.	Viburnum ellipticum.	Viburnum dilatatum, etc.
Mitchella repens.		Mitchella undulata.
Adenocaulon bicolor.	Adenocaulon bicolor.	Adenocaulon adhærescens.
Boltonia, spp.		(Boltonia spp.)
Aster § Biotia, corymbosus and spp.		Aster § Biotia, corymbosus and spp.
Aster § Conyzopsis, angustus.	Aster angustus.	Aster angustus = Brachyactis ciliata, Ledeb.
Artemisia Canadensis.	Artemisia Canadensis.	Artemisia Canadensis ? = comutata.
" biennis.	" biennis.	Artemisia biennis.
" frigida.	" frigida.	" frigida.
Senecio pseudo-arnica.		Senecio pseudo-arnica.
Nabalus spp.		(Nabalus ochroleucus acerifolius)
Cacalia spp.		(Cacalia spp.)
Mulgedium pulchellum.	Mulgedium pulchellum.	Mulgedium Sibiricum.
Vaccinium § Oxycoccus macrocarpum.		Vaccinium macrocarpum, forma ambigua.[1]
" " erythrocarpum.		" Japonicum (ab erythrocarpo vix differt).
" § Batodendron, 2 spp.		(" § Batodendron, spp.)
" § Cyanococcus, 15 spp.		(" § Cyanococcus, one sp. near Pennsylvanicum.
" ovalifolium.	Vaccinium ovalifolium.	" ovalifolium.
Chiogenes hispidula.		Chiogenes hispidula.
Epigæa repens.		Epigæa Asiatica.
Gaultheria procumbens.		Gaultheria pyroloides.
Leucothoe axillaris and Catesbæi.		Leucothoe Keiskei.
Leucothoe racemosa and recurva.		(" Grayana and Tschonoskii.)
Andromeda § Portuna floribunda.		(Andromeda § Portuna sp.)
Andromeda § Pieris spp.		(" § Pieris spp.)
Clethra, 2 spp.		(Clethra sp.)
Menziesia ferruginea, var. globularis.	Menziesia globularis.	Menziesia pentandra and others.
Rhododendron Catawbiense.	Rhododendron Californicum.	(Rhododendron brachycarpum.)
" maximum.		(" Metternichii.)
" punctatum.		(" Keiskei.)
Rhodora Canadensis.		(" spp.)
Azalea, 4 spp.	(Azalea occidentalis).	(Azalea spp.)
Elliottia racemosa.		Tripetaleia paniculata and bracteata.

[1] " Ob flores revera terminales, bracteolas lineari-lanceolatas scariosas et folia acuta." Dr. Maximowicz (in " Mel. Biolog. Diagn." decas 12) refers this to *V. Oxycoccus*, instead of to *V. macrocarpum*, which is " semper bene distinctum floribus axillaribus, bracteolis ovatis foliaceis et foliis obtusis." But in one of my specimens the axis of the umbel is continued into a leafy shoot, as in *V. macrocarpum;* and the bracteolæ vary from linear to ovate, and from thin and scarious to chartaceous or coriaceous in both species; they are never (so far as I know) " foliaceous " in *V. macrocarpum*, but the bracteæ sometimes are. The leaves are sometimes acutish in the latter, and also very obtuse in *V. Oxycoccus;* in the Japanese specimens under consideration they are often half an inch in length. I must add that in the length of the filaments they accord with the character which I assigned to *V. Oxycoccus* in the Manual. In fact, a form combining the characters of the two species survives in Japan.

Pyrola elliptica.		Pyrola elliptica.
Monotropa uniflora.		Monotropa uniflora.
Shortia galacifolia.		Shortia galacifolia = Schizocodon uniflorus.
Ilex § Prinos spp.		(Ilex § Prinos spp.)
Diospyros Virginiana.		(Diospyros spp.)
Symplocos sp.		(Symplocos spp.)
Tecoma radicans.		Tecoma grandiflora.
Catalpa bignonioides.		Catalpa Kæmpferi.
Veronica Virginica.		Veronica Virginica.
Callicarpa Americana.		(Callicarpa; 3 spp.)
Phryma Leptostachya.		Phryma Leptostachya.
Lycopus Virginicus.	Lycopus Virginicus.	Lycopus parviflorus.
Teucrium Canadense.		Teucrium Japonicum.
Hedeoma, 4 spp.		(Hedeoma sp.)
Lophanthus spp.	Lophanthus spp.	(Lophanthus sp.)
Scutellaria (nuculis alatis) nervosa.		Scutellaria (nuculis alatis) Guilielmi.[1]
Halenia deflexa.		Halenia Sibirica, and spp.
Phlox subulata.	(Phlox Douglasii and spp.)	Phlox Sibirica.
Gelsemium sempervirens.		(Gelsemium elegans.)
Mitreola, 2 spp.		Mitreola oldenlandioides.
Apocynum androsæmifolium.	Apocynum androsæmifolium.	(Apocynum venetum.)
Amsonia Tabernæmontana.		(Amsonia elliptica.)
Asarum Virginicum and arifolium.	Asarum caudatum.	Asarum variegatum and Blumei.
Asarum Canadense.		" caulescens and Sieboldii.
Phytolacca decandra.		Phytolacca Kæmpferi, etc.
Corispermum hyssopifolium.		Corispermum hyssopifolium.
Polygonum arifolium.		Polygonum perfoliatum.
" sagittatum.		" sagittatum and Sieboldii.
Sassafras officinale.		(Lindera triloba, etc.)
Lindera Benzoin, etc.		" hypoglauca, etc.
Tetranthera geniculata.	(Tetranthera Californica.)	(Tetranthera spp.)
Pyrularia oleifera.		(Pyrularia = Sphærocarpa spp.)
Saururus cernuus.		Saururus Loureiri.
Stillingia spp.		(Stillingia spp.)
Pachysandra procumbens.		Pachysandra terminalis.
Planera aquatica.		Planera Japonica (and Richardi).
Maclura aurantiaca.		Maclura gerontogæa.
Pilea pumila.		Pilea pumila.
Laportea Canadensis.		Laportea evitata, etc.
Bœhmeria cylindrica.		(Bœhmeria spp.)
Parietaria debilis.		Parietaria debilis.
Juglans nigra.	(Juglans rupestris.)	(Juglans regia.)
" cinerea.		Juglans Mandchurica, stenocarpa.
Corylus rostrata.	Corylus rostrata var.	Corylus rostrata, var. Mandchurica.
Betula glandulosa.		Betula glandulosa.
" nigra.		(" ulmifolia, etc.)
Alnus maritima.		Alnus maritima.
Myrica cerifera.	Myrica Californica.	(Myrica Nagi.)
Pinus resinosa.		(Pinus densiflora, etc.)
" Strobus.	Pinus monticola.	" excelsa.

[1] *Scutellaria Guilielmi*, n. sp. Perillomioides: slender, branched from the base, stoloniferous? leaves membranaceous, minutely pubescent, crenately dentate, the lower round-cordate and slender-petioled, the others ovate or oblong with rounded or truncate base and short-petioled, the floral similar but gradually smaller; flowers solitary in the axils; peduncles about the length of the calyx; corolla ("light purple," only three lines long) hardly more than twice the length of the calyx, its lips of nearly equal length; nutlets surrounded by an abrupt and reflexed denticulate wing, upper face of the disk muricate, the lower as if squamellate. *S. hederacea?* Gray, in Perry's "Japan Exped." iii. p. 316, and "Bot. Contrib. Proc. Amer. Acad." viii. p. 370, not of Kunth and Bouché. It appears from a note by Vatke, in "Bot. Zeit.," 1872, p. 717, that *S. hederacea* is identical with the Tasmanian *S. humilis*, and its nutlets were originally described as echinulate-tuberculate, and by implication wingless. So our plant may be named in honor of Dr. S. W. Williams, who first collected a little of it at Simoda, Japan. Better and fruiting specimens were gathered on the Loo-Choo Islands, by Charles Wright.

Abies Canadensis.	Abies Mertensiana.	Abies Tsuga and diversifolia.
Thuja occidentalis.	Thuja gigantea, etc.	Thuja Japonica.
Taxodium distichum.		(Glyptostrobus heterophyllus.)
Cupressus (Chamæcyparis) thuyoides.	C. Nutkaensis.	Cupressus pisifera, obtusa, etc.
Taxus Canadensis.	Taxus brevifolia.	Taxus cuspidata.
Torreya taxifolia.	Torreya Californica.	Torreya nucifera and grandis.
Arisæma, 3 spp.		Arisæma, 9 spp.
Symplocarpus fœtidus.	(Lysichiton Camtschatcense.)	Symplocarpus fœtidus? and Lysichiton Camtschatcense.
Listera australis.		(Listera Japonica, etc.)
Arethusa bulbosa.		(Arethusa Japonica.)
Pogonia ophioglossoides.		Pogonia ophioglossoides.
Microstylis ophioglossoides.		(Microstylis Japonica.)
Liparis liliifolia.		Liparis liliifolia?
Cypripedium acaule.		(Cypripedium Japonicum.)
Habenaria virescens.		Habenaria fucescens.
Aletris farinosa and aurea.		Aletris Japonica.
Iris cristata.		Iris tectorum = cristata, Miq.
Dioscorea villosa.		(Dioscorea spp.)
Smilax hispida.		Smilax Sieboldii.
" herbacea and pedunc- ularis.		" herbacea = Nipponica.
Smilax tamnifolia.		(" higoensis.)
Croomia pauciflora.		Croomia pauciflora.
Trillium grandiflorum.	Trillium obovatum.	Trillium obovatum.
" erectum.		" erectum var.
Tofieldia glutinosa and pubens.		(Tofieldia Japonica and nutans.)
Helonias bullata.		Heloniopsis pauciflora, brevis- capa, Japonica.
Chamælirium luteum.		Chamælirium luteum.
Zygadenus, 3 spp.	Zygadenus glaucus, etc.	(Zygadenus Japonicus.)
Streptopus roseus.	Streptopus roseus.	Streptopus roseus.
Prosartes lanuginosa.	Prosartes Hookeri, etc.	Prosartes viridescens, etc.
Clintonia borealis.	Clintonia uniflora.	Clintonia Udensis.
Polygonatum giganteum.		Polygonatum giganteum.
Smilacina trifolia.		Smilacina trifolia.
" racemosa.	Smilacina racemosa var.	" Japonica.
" stellata.	" stellata.	" Davarica.
Erythronium Americanum and albidum.	Erythronium grandiflorum.	Erythronium grandiflorum.
Narthecium Americanum.		Narthecium Asiaticum.
Scirpus Eriophorum.		Scirpus Eriophorum.
Carex rostrata.		Carex rostrata.
Carex stipata	Carex stipata.	Carex stipata.
Zizania aquatica.		Zizania=Hydropyrum latifolium.
Arundinaria macrosperma.		(Arundinaria Japonica.)
Avena striata and Smithii.		Avena collosa.
Adiantum pedatum.	Adiantum pedatum.	Adiantum pedatum.
Pellæa gracilis.		Pellæa Stelleri = gracilis.
Aspidium fragrans.		Aspidium fragrans.
Asplenium thelypteroides.		Asplenium thelypteroides.
Camptosorus rhizophyllus.		Camptosorus Sibiricus.
Onoclea sensibilis.		Onoclea sensibilis.
Osmunda cinnamonea.		Osmunda cinnamonea.
" Claytoniana.		" Claytoniana.
Lygodium palmatum.		(Lygodium Japonicum.)
Botrychium Virginicum.		Botrychium Virginicum.
Lycopodium lucidulum.		Lycopodium lucidulum.
" dendroideum.		" dendroideum.

It appears that two thirds of the middle column is blank; namely, that only a third of the species or forms which are more or less peculiar to temperate Atlantic North America (*i. e.*, east of the Mississippi and south of the Great Lakes and the St. Lawrence) and to temperate eastern Asia, are represented in Oregon and California. Moreover, eighty of the genera here treated of are peculiar

to North America and temperate Asia; and sixty-three (*i. e.*, more than three quarters) of these are not met with in western North America.

This table may be compared, or rather contrasted, with the following one.

Extra-European Plants of Temperate Eastern Asia which are represented identically or by some near Relative in Oregon (south of Lat. 48°) or California (Arctic-alpine Plants excluded), but not in the Atlantic United States: —

Thalictrum sparsiflorum.	Thalictrum sparsiflorum.
Ranunculus affinis.	Ranunculus affinis.
Coptis occidentalis.	Coptis occidentalis.
" brachypetala and Teeta.	" asplenifolia.
Aconitum delphinifolium.	Aconitum delphinifolium.
Pœonia spp.	(Pœonia Rossii.)
Berberis § Mahonia spp.	(Berberis § Mahonia spp.
Epimedium § Aceranthus sp.	Vancouveria hexandra.
Achlys Japonica.	Achlys triphylla.
Corydalis pæoniæfolia.	Corydalis pæoniæfolia.
Mœhringia umbrosa.	Mœhringia macrophylla.
Linum perenne.	Linum perenne.
Thermopsis fabacea.	Thermopsis fabacea.
Astragalus adsurgens.	Astragalus adsurgens.
Chamærhodos erecta.	Chamærhodos erecta.
Spiræa callosa.	Spiræa Nobleana.
Rubus spectabilis.	Rubus spectabilis.
Pyrus rivularis?	Pyrus rivularis.
Cratægus sanguinea.	Cratægus Douglasii.
Rosa Kamtschatica.	Rosa Kamtschatica.
Photinia arbutifolia.	(Photinia serrulata.)
Saxifraga Sibirica.	Saxifraga Sibirica.
Mitella § Mitellaria spp.	(Mitella § Mitellaria spp.)
Glehnia littoralis.	Glehnia littoralis.
Oplopanax horrida.	Oplopanax horrida.
Echenais carlinoides.	Echenais carlinoides.
Lonicera Maximowiczii.	Lonicera Breweri.
Gaultheria adenothrix.	Gaultheria Myrsinites.
Rhododendron ovatum and semibarbatum.	Rhododendron albiflorum.
Pyrola subaphylla.	Pyrola aphylla.
Villarsia Crista-Galli.	Villarsia Crista-Galli.
Lycopus lucidus.	Lycopus lucidus.
Boschniakia glabra.	Boschniakia glabra.
Echinospermum patulum.	Echinospermum patulum.
" Redowskii.	" Redowskii.
Hottuynia cordata.	Anemiopsis Californica.
Quercus spp.	(Quercus densiflora.)
Castanopsis spp.	(Castanopsis chrysophylla.)
Lysichiton Camtschatcense.	Lysichiton Camtchatcense.
Erythronium grandiflorum.	Erythronium grandiflorum.
Carex macrocephala.	Carex macrocephala.
Triticum ægilopoides.	Triticum ægilopoides.
Elymus Sibiricus.	Elymus Sibiricus.
Abies Menziesii.	Abies Menziesii.
Woodwardia radicans.	Woodwardia radicans.

The entries are only forty-five; and the representation, when at all close, is by identical or nearly identical species. Only seven of the genera here noted are peculiar to northeastern Asia and northwestern America: namely, Glehnia, Oplopanax, and Lysichiton, each of a single species common to both coasts; Achlys, of which there is a Japanese species said to differ from the American; Bosch-

niakia, of a common high northern species, and a peculiar one in California; Echinais, of one or two Asiatic species, one of them lately found in California and Colorado, but possibly of recent introduction; and Castanopsis, a rather large and characteristic east Asian genus, represented by a single but very distinct species in Oregon and California.

Small, under the circumstances, as is the number of cognate plants or forms in these two floras, it is large in comparison with those which are peculiar to the United States and Europe, excluding, as before, all Arctic-alpine species. The following seem to be the principal: —

Anemone nemorosa, of which there is a peculiar Pacific form, perhaps reaching the eastern borders of Asia.

Myosorus minimus, which may be a recently introduced plant.

Cakile, a maritime genus.

Saxifraga aizoides.

Bellis integrifolia, which may be compared with the European *B. annua.*

Lobelia Dortmanna.

Primula Mistassinica.

Centunculus lanceolatus, a mere form of *C. minimus.*

Hottonia inflata, which represents *H. palustris.*

Utricularia minor.

Salicornia Virginica, the *S. mucronata* of Bigelow and probably of Lagasca also.

Corema Conradi, representing the Portuguese *C. alba.*

Vallisneria spiralis, which appears to be absent from northern Asia.

Spiranthes Romanzoviana, with its single station on the Irish coast. It extends across the American continent well northward, but seemingly not into the adjacent parts of Asia.

Eriocaulon septangulare, restricted in the Old World to a few stations on west British coasts.

Carex extensa, *C. flacca* (or *Barrattii*), and one or two others.

Cinna arundinacea, var. *pendula.*

Leersia oryzoides.

Spartina stricta and *S. juncea.*

Equisetum Telmateia.

Lycopodium inundatum.

Calluna vulgaris, which holds as small and precarious a tenure on this continent as *Spiranthes Romanzoviana* does in Europe.

Barely two dozen; and three or four of these are more or less maritime. Only two or three of them extend west of the Mississippi Valley.

Narthecium is not in the list, a form or near ally of the European and Atlantic-American species having been detected in Japan; the genus is unknown on the Pacific side of our continent.

II.

Since the foregoing tables were prepared, a letter from Mr. Dall (who has returned from an arduous and successful exploration of the Alaskan region, made under the authority of the United States Coast Survey) informs me that his party met with Caulophyllum upon one of the Shumagin Islands. These islands lie off the southern shore of the peninsula of Alaska, about in latitude 55°, longitude 160°. No specimen occurs in the beautiful collection of dried plants made in this expedition, mainly by Mr. Harrington; nor indeed any other plants which affect so southern a range as our Caulophyllum. Yet the plant may well have been rightly identified; although it should be seen by botanists before any conclusions are drawn from it. But the occurrence of an intermediate station like this would probably lead Professor Grisebach to rank the north Asiatic Caulophyllum no longer as a representative species, but as identical with our Atlantic plant, as Miquel and Maximowicz, as well as myself, have already done upon evidence derived from the specimens.

Then, — upon Professor Grisebach's idea that, while identical species are to be referred to a single origin and the disseverance accounted for through means and causes now in operation, representative species have somehow arisen independently under similar climates, — Caulophyllum must be explained as a case of migration, but Diphylleia (in the same predicament, only with a perceptible difference between the two plants) as a case of double origination. So of the *Shortia galacifolia* and the *Schizocodon uniflorus*, of which the corolla and stamens in both are still wanting. If these, when found, should prove to be exactly alike in the two, the very difficult problem of accounting for the world-wide separation under present circumstances is to be encountered; if a difference appears, the problem is to consider how, and upon what, similar climates can have acted to have originated almost identical species upon opposite sides of the world. Professor Grisebach's views imply that "each species has arisen under the influence of physical and other external conditions," and that gradual alterations in a climate somehow pro-

duce adaptive "changes in organization"; wherefore, as the President of the Linnæan Society has aptly remarked,[1] "We have a right to ask of him, What is the previous organization upon which he imagines climate to have worked to produce allied species in one region and representative species in distant regions?" The difference here between Grisebach's conception and our own is, that we consider climate and other external conditions to have acted upon common ancestors in each case; but he apparently declines to conjecture what they acted upon.

In conclusion I may advert to one instance, in which it would appear, either that widely different climates have originated the same or closely similar species, or else that one and the same species (one of those common to the United States and Japan) has been dispersed over the globe in a manner and to an extent that place it beyond the reach of explanations limited to the results of forces still in activity and means of dispersion still available. *Brasenia peltata* inhabits: 1. The Atlantic United States, from Canada to Texas; 2. Oregon, or rather Washington Territory, a single known station at Gray's Harbor, on the Pacific, latitude 47°; and Clear Lake, in California, latitude 39°; 3. Japan; 4. Khasya and Bhotan, altitude 4–6000 feet; 5. Australia, Moreton Bay, etc.; 6. West Africa, in a lake in Angola!

[1] Address of George Bentham, Esq., President of the Linnæan Society, etc., read May 24, 1872.

DO VARIETIES WEAR OUT OR TEND TO WEAR OUT?[1]

This question has been argued from time to time for more than half a century, and is far from being settled yet. Indeed, it is not to be settled either way so easily as is sometimes thought. The result of a prolonged and rather lively discussion of the topic about forty years ago in England, in which Lindley bore a leading part on the negative side, was, if we rightly remember, that the nays had the best of the argument. The deniers could fairly well explain away the facts adduced by the other side, and evade the force of the reasons then assigned to prove that varieties were bound to die out in the course of time. But if the case were fully reargued now, it is by no means certain that the nays would win it. The most they could expect would be the Scotch verdict "not proven." And this not because much, if any, additional evidence of the actual wearing out of any variety has turned up since, but because a presumption has been raised under which the evidence would take a bias the other way. There is now in the minds of scientific men some reason to expect that certain varieties would die out in the long run, and this might have an important influence upon the interpretation of the facts that would be brought forward. Curiously enough, however, the recent discussions to which our attention has been called seem, on both sides, to have overlooked this matter.

But, first of all, the question needs to be more specifically stated if any good is to come from a discussion of it. There are varieties and varieties. They may, some of them, disappear or deteriorate, but yet not wear out — not come to an end from any inherent cause. One might even say, the younger they are the less chance of survival unless well cared

[1] New York Tribune, semi-weekly edition, December 8, 1874.

for. They may be smothered out by the adverse force of superior numbers; they are even more likely to be bred out of existence by unprevented cross-fertilization, or to disappear from mere change of fashion. The question, however, is not so much about reversion to an ancestral state, or the falling off of a high-bred stock into an inferior condition. Of such cases it is enough to say that, when a variety or strain of animal or vegetable is led up to unusual fecundity or size or product of any organ, for our good, and not for the good of the plant or animal itself, it can be kept so only by high feeding and exceptional care; and that with high feeding and artificial appliances come vastly increased liability to disease, which may practically annihilate the race. But then the race, like the bursted boiler, could not be said to wear out, while if left to ordinary conditions, and allowed to degenerate back into a more natural, if less useful state, its hold on life would evidently be increased rather than diminished.

As to natural varieties or races under normal conditions, sexually propagated, it could readily be shown that they are neither more nor less likely to disappear from any inherent cause than the species from which they originated. Whether species wear out, *i. e.*, have their rise, culmination, and decline from any inherent cause, is wholly a geological and very speculative problem, upon which, indeed, only vague conjectures can be offered. The matter actually under discussion concerns cultivated domesticated varieties only, and, as to plants, is covered by two questions.

First: Will races propagated by seed, being so fixed that they come true to seed, and purely bred (not crossed with any other sort), continue so indefinitely, or will they run out in time — not die out, perhaps, but lose their distinguishing characters? Upon this, all we are able to say is, that we know no reason why they should wear out or deteriorate from any inherent cause. The transient existence or the deterioration and disappearance of many such races are sufficiently accounted for otherwise; as in the case of extraordinarily exuberant varieties, such as mammoth fruits or roots, by increased liability to disease, already adverted to, or by the failure of

the high feeding they demand. A common cause, in ordinary cases, is cross-breeding, through the agency of wind or insects, which is difficult to guard against. Or they go out of fashion and are superseded by others thought to be better, and so the old ones disappear.

Or, finally, they revert to an ancestral form. As offspring tend to resemble grandparents almost as much as parents, and as a line of close-bred ancestry is generally prepotent, so newly originated varieties have always a tendency to reversion. This is pretty sure to show itself in some of the progeny of the earlier generations, and the breeder has to guard against it by rigid selection. But the older the variety is — that is, the longer the series of generations in which it has come true from seed — the less the chance of reversion: for now, to be like the immediate parent, is also to be like a long line of ancestry; and so all the influences concerned — that is, both parental and ancestral heritability — act in one and the same direction. So, since the older a race is the more reason it has to continue true, the presumption of the unlimited permanence of old races is very strong.

Of course the race itself may give off new varieties: but that is no interference with the vitality of the original stock. If some of the new varieties supplant the old, that will not be because the unvaried stock is worn out or decrepit with age, but because in wild nature the newer forms are better adapted to the surroundings, or, under man's care, better adapted to his wants or fancies.

The second question, and one upon which the discussion about the wearing out of varieties generally turns, is: Will varieties propagated from buds, *i. e.*, by division, grafts, bulbs, tubers and the like, necessarily deteriorate and die out? First, Do they die out as a matter of fact? Upon this, the testimony has all along been conflicting. Andrew Knight was sure that they do, and there could hardly be a more trustworthy witness.

"The fact," he says, fifty years ago, "that certain varieties of some species of fruit which have been long cultivated cannot now be made to grow in the same soils and under the

same mode of management which was a century ago so perfectly successful, is placed beyond the reach of controversy. Every experiment which seemed to afford the slightest prospect of success was tried by myself and others to propagate the old varieties of the Apple and Pear which formerly constituted the orchards of Herefordshire, without a single healthy or efficient tree having been obtained; and I believe all attempts to propagate these varieties have, during some years, wholly ceased to be made."

To this it was replied, in that and the next generation, that cultivated vines have been transmitted by perpetual division from the time of the Romans, and that several of the sorts, still prized and prolific, are well identified, among them the ancient Græcula, considered to be the modern Corinth or Currant grape, which has immemorially been seedless; that the old Nonpareil apple was known in the time of Queen Elizabeth; that the White Beurré pears of France have been propagated from earliest times; and that Golden pippins, St. Michael pears, and others said to have run out, were still to be had in good condition.

Coming down to the present year, a glance through the proceedings of pomological societies and the debates of farmers' clubs, bring out the same difference of opinion. The testimony is nearly equally divided. Perhaps the larger number speak of the deterioration and failure of particular old sorts; but when the question turns on "wearing out," the positive evidence of vigorous trees and sound fruits is most telling. A little positive testimony outweighs a good deal of negative. This cannot readily be explained away, while the failure may be, by exhaustion of soil, incoming of disease, or alteration of climate or circumstances. On the other hand it may be urged, that, if a variety of this sort is fated to become decrepit and die out, it is not bound to die out all at once or everywhere at the same time. It would be expected first to give way wherever it was weakest, from whatever cause. This consideration has an important bearing upon the final question, Are old varieties of this kind on the way to die out on account of their age or any inherent limit of vitality?

Here, again, Mr. Knight took an extreme view. In his essay in the "Philosophical Transactions," published in the year 1810, he propounded the theory, not merely of a natural limit to varieties from grafts and cuttings, but even that they would not survive the natural term of the life of the seedling trees from which they were originally taken. Whatever may have been his view of the natural term of the life of a tree, and of a cutting being merely a part of the individual that produced it, there is no doubt that he laid himself open to the effective replies which were made from all sides at the time, and have lost none of their force since. Weeping-Willows, Bread-fruits, Bananas, Sugar-cane, Tiger-lilies, Jerusalem Artichokes, and the like, have been propagated for a long time in this way, without evident decadence.

Moreover, the analogy upon which his hypothesis is founded will not hold. Whether or not one adopts the present writer's conception, that individuality is not actually reached or maintained in the vegetable world, it is clear enough that a common plant or tree is not an individual in the sense that a horse or man, or any one of the higher animals is — that it is an individual only in the sense that a branching zoöphyte or mass of coral is. *Solvitur crescendo:* the tree and the branch equally demonstrate that they are not individuals, by being divided with impunity and advantage, with no loss of life but much increase. It looks odd enough to see a writer like Mr. Sisley reproducing the old hypothesis in so bare a form as this: "I am prepared to maintain that varieties are individuals, and as they are born they must die, like other individuals." "We know that Oaks, Sequoias and other trees live several centuries, but how many we do not exactly know. But that they must die no one in his senses will dispute." Now what people in their senses do dispute is, not that the tree will die, but that other trees, established from cuttings of it, will die with it.

But does it follow from this that non-sexually propagated varieties are endowed with the same power of unlimited duration that are possessed by varieties and species propagated sexually — *i. e.* by seed? Those who think so jump too soon

at their conclusion. For, as to the facts, it is not enough to point out the diseases or the trouble in the soil and in the atmosphere, to which certain old fruits are succumbing, nor to prove that a parasitic fungus (*Peronospora infestans*) is what is the matter with potatoes. For how else would constitutional debility, if such there be, more naturally manifest itself than in such increased liability or diminished resistance to such attacks? And if you say that, anyhow, such varieties do not die of old age, — meaning that each individual attacked does not die of old age, but of manifest disease, — it may be asked in return, What individual man ever dies of old age in any other sense than of a similar inability to resist invasions which in earlier years would have produced no noticeable effect? Aged people die of a slight cold or a slight accident, but the inevitable weakness that attends old age is what makes these slight attacks fatal.

Finally, there is a philosophical argument which tells strongly for some limitations of the duration of non-sexually-propagated forms, one that Knight probably never thought of, but which we should not have expected recent writers to overlook. When Mr. Darwin announced that the principle of cross-fertilization between the individuals of a species is the plan of nature, and is practically so universal that it fairly sustains his inference that no hermaphrodite species continually self-fertilized would continue to exist, he made it clear to all who apprehend and receive the principle, that a series of plants propagated by buds only must have a weaker hold of life than a series reproduced by seed. For the former is the closest kind of breeding. Upon this ground such varieties may be expected ultimately to die out; but the mills of the gods grind so exceeding slow that we cannot say that any particular grist has been actually ground out under human observation.

If it be asked how the asserted principle is proved or made probable, we can here merely say that the proof is wholly inferential. But the inference is drawn from such a vast array of facts that it is wellnigh irresistible. It is the legitimate explanation of those arrangements in nature to secure cross-

fertilization in the species, either constantly or occasionally, which are so general, so varied and diverse, and we may add so exquisite and wonderful, that, once propounded, we see that it must be true. What else, indeed, is the meaning and use of sexual reproduction? Not simply increase in numbers; for that is otherwise effectually provided for by budding propagation in plants and many of the lower animals. There are plants, indeed, of the lower sort, in which the whole multiplication takes place in this way, and with great rapidity. These also have sexual reproduction; but in it two old individuals are always destroyed to make a single new one! Here propagation diminishes the number of individuals fifty per cent. Who can suppose that such a costly process as this, and that all the exquisite arrangements for cross-fertilization in hermaphrodite plants, do not subserve some most important purpose? How and why the union of two organisms, or generally of two very minute portions of them, should reinforce vitality, we do not know and can hardly conjecture. But this must be the meaning of sexual reproduction.

The conclusion of the matter from the scientific point of view is, that sexually propagated varieties, or races, although liable to disappear through change, need not be expected to wear out, and there is no proof that they do; but that non-sexually propagated varieties, though not liable to change, may theoretically be expected to wear out, but to be a very long time about it.

ÆSTIVATION AND ITS TERMINOLOGY.[1]

The term æstivation, to denote the arrangement of the parts of the calyx, corolla, etc., in the bud, as well as that of vernation for leaves in a leaf-bud, was introduced by Linnæus. He did not elaborate the former subject as he did the latter, and the few terms given to the modes he recognized are for the most part defined merely by a reference to their use in vernation. Æstivation as a botanical character is comparatively recent, and its terminology is not yet quite satisfactorily settled. I propose to consider, (1) what the leading modes are, and (2) how they are to be designated.

(1) In the first place, the modes of æstivation may be conveniently divided into two classes, those in which the parts overlap, and those in which they do not.

Of overlapping æstivation, only two principal kinds need be primarily distinguished, namely: 1. where some pieces overlap and others are overlapped, *i. e.*, some have both margins exterior and others both margins interior or covered; 2. where each piece of a circle is overlapped by its neighbor on one side while it overlaps its neighbor on the other. There are mixtures and subordinate modifications of these two, but no third mode.

In æstivation without overlapping, there is, first, the rare case in which the parts of the whorl or cycle never come into contact in the bud; and, secondly, that in which they impinge by their edges only. There is also the case in which both margins of each piece are rolled or bent inward, and the rarer one in which they are turned outward; and the apex of each piece may comport itself in any of these ways. But these dispositions are those of the pieces or leaves taken separately, and the terms applied to them are the same as in vernation or

[1] American Journal of Science and Arts, 3 ser., x. 339. (1875.)

prefoliation, are used in the same sense, and so are not at all peculiar to æstivation or prefloration. The like may be said of a remaining mode, which belongs, however, to a different category, that in which the parts being united into a tube or cup, this is bodily plaited into folds, or otherwise disposed; in which case the margin of the tube or cup, or such lobes as it may have, may exhibit any of the modes of æstivation above indicated.

Without further notice, then, of this last, the plicate or plaited æstivation, and of analogous conformations of the tube or cup of a calyx or corolla, or of the disposition of each piece individually (whether revolute, involute, reflexed, inflexed, and the like), about the terminology of which there is no question, — omitting, likewise, for the latter reason, the case of open æstivation, — there are left three types to deal with:

I. With some pieces of the set wholly exterior in the bud to others.

II. With each piece covered at one margin, and covering by the other.

III. With each piece squarely abutting against its neighbors on either side, without overlapping.

In modes II and III, the pieces are all on the same level and are to be viewed as members of a whorl. In mode I, although they may sometimes be members of a whorl, some parts of which have become external to others in the course of growth, they may, and in many cases must belong either to two or more successive whorls (as in the corolla of *Papaveraceæ*, and even the calyx of *Cruciferæ*, the upper or inner of course covered by the lower or outer), or to the spiral phyllotaxy of alternate leaves.

The type of the latter, and the common disposition when the parts are five, is with two pieces exterior, the third exterior by one edge and interior by the other, and two wholly interior. This is simply a cycle in $\frac{2}{5}$ phyllotaxy, the third piece being necessarily within and covered at one margin by the first, while it is exterior to and with its other margin covers the fifth, this and the fourth being of course wholly interior. So, likewise, when the parts are three, one exterior,

one half exterior, and one interior or overlapped, the æstivation accords with $\frac{1}{3}$ phyllotaxy. When of eight or higher numbers the spiral order is usually all the more manifest. When of four or six, the case is one of whorls (opposite leaves representing the simplest whorl), either of a pair of whorls (as in Epimedium, Berberis, etc.), or a single whorl, the parts of which have overlapped in cyclic order.

(2) As to the terminology. Linnæus in the "Philosophia Botanica" treats only of vernation, there termed "Foliatio." For this the former term was substituted, and that of æstivation for the disposition of petals in a flower-bud, introduced, as I suppose (not having the volume to consult), in the Termini Botanici, published in the sixth volume of the "Amœnitates Academicæ," 1762. I refer to it only through Giseke's edition, 1781. Here the terms are convoluta, imbricata, conduplicata, defined only by reference to the section vernatio, and valvata, unhappily explained by a reference to the glumes of Grasses, also "inæquivalvis; si magnitudine discrepant." Imbricata is the only term besides valvata which directly relates to the arrangement of petals, etc., *inter se;* and the reference takes us back to something "tectus, ut nudus non appareat," covered as with tiles, we may infer. In the "Philosophia Botanica," under the section Foliatio, the definition of imbricata is "quando parallele, superficie recta, sibi invicem incumbunt." This would apply either to mode I, or mode II, according as invicem is understood; but the diagram (tab. x. 6) shows that case I is intended. Convoluta refers to the rolling of a petal or leaf by itself, as does conduplicata to its folding; but Linnæus gives two figures, one of a single rolled-up leaf, the other of one leaf rolled up within another.

Finally, among the modes of vernation indicated by Linnæus, there is one which it is important here to notice, relating as it does to the arrangement of a pair of leaves in the bud, and evidently quite as applicable to a whorl of a larger number of parts than two; *i. e.*:

"Obvoluta, quum margines alterni comprehendunt oppositi folii marginem rectum" (Philosophia Botanica, 105).

Or, in Termini Botanici, "pagina superiore lateribus approximatis ita ut alterum latus distinguat alterum folium."

This, as the definition and the diagram in the "Philosophia Botanica" show, answers in æstivation to mode II. It was early taken up as such by Mirbel (Elem. Phys. Veg. et Bot., 1815, ii. 738, 739), where the polypetalous corolla of Hermannia and Oxalis and the gamopetalous corolla of *Apocyneæ* are cited as examples.

Valvate æstivation, our mode III, is rightly defined by Mirbel in the same place, and still earlier by Brown.

Linnæus made no use of æstivation as a character. Nor did Jussieu, except merely that, in his "Genera Plantarum," the petals of Malvaviscus are said to be convolute.

In De Candolle's "Théorié Elémentaire," 1813 — a still unsurpassed treatise, upon which, next to the "Philosophia Botanica," our botanical glossology rests — neither the word æstivation, nor its synonym, prefloration, is mentioned, and even vernation or prefoliation is equally omitted.

But the history of æstivation as a botanical character began in a work published three years earlier, namely, in R. Brown's "Prodromus Floræ Novæ Hollandiæ," 1810. The preface notes that it was first accurately observed by Grew. In it Brown defines only the valvate mode, "ubi margines foliolorum vel laciniarum integumenti invicem applicati sunt, capsulæ valvularum in modum." In the body of the work, wherever it is important, the æstivation is noted as valvate, imbricate, plicate, induplicate, etc.; and the open æstivation (aperta) is named by him in a subsequent paper.

Being the first to employ æstivation systematically, and to develop its value, Brown's terminology for its modes may well be considered authoritative. And so indeed it is, as far as it goes. But he did not make one important distinction, namely, that between our I and II. Imbricate, in his use, comprises all kinds of overlapping, that of the corolla of *Apocyneæ* and of a Gentian, as well as that of a Primrose. He must have not only noticed the difference, but also appreciated its general importance, notwithstanding the occasional passage of the one into the other. He must have also observed that in

many cases, as in Asclepias for instance, the mode II passes into mode III, the valvate, and may possibly have discerned that under a phyllotaxic view these are more nearly related than either is to mode I. I find, however, only one instance in which he has indicated the distinction, namely, in the character of Burchellia, furnished to the "Botanical Register," t. 435, 1820. Of its corolla it is said: "æstivatione mutuo imbricata contorta." The phrase is interesting, as it seems to recognize the distinction between the mode of overlapping (which is that of our mode II) and the torsion, which only now and then accompanies it. Looking over the "Plantæ Javanicæ Rariores" to see if there is any later use, I find no instance in which Brown has occasion to speak of this mode II; but it occurs in the portion of his associate, Mr. Bennett, who (on p. 212) describes the petals of Sonerila as "æstivatione convoluta." Had this term been thus employed by Brown himself, and at an earlier date, I should regard the terminology of these three modes of æstivation as settled, namely: I, imbricata, II, convoluta, III, valvata. The first and the third are established beyond question, although somewhat remains to be said about the first.

But meanwhile another use has prevailed as respects the second. In De Candolle's "Prodromus," the first general or considerable work after Brown in which terms of æstivation are employed, this mode is almost uniformly characterized as contorta. I cannot at this moment trace the term to its origin. It was probably suggested by the name *Contortæ*, said to have been given by Linnæus to the Apocyneous natural order; and it seemed appropriate to the instances in which the strong convolution of rounded petals, as in Oxalis, or their lobes, as in Phlox, give an appearance like that of twisting, although there is no twist or torsion. But it is to just such cases, in which there is most of seeming twisting on account of the strong convolution, that the term convolute is now and then assigned in the "Prodromus"; as in the character of *Byttneriaceæ*, and that of Malvaviscus. The latter may perhaps be explained by the peculiarity that the petals do not uncoil in anthesis. But in *Apocynaceæ*, in the "Pro-

dromus," the terms convoluta and contorta are seemingly employed synonymously, or nearly so (the latter most frequently); at least I see no difference between the æstivation of Allamanda, said to be contorted, and that of Vinca (rosea), said to be convolute. Endlicher in this regard follows the "Prodromus." In the new "Genera Plantarum" by Bentham and Hooker this mode is most commonly designated as contorta, sometimes as contorto-imbricata, rarely (Philadelphus, etc.) convoluta. I have myself, from a period as early as 1840, employed the term convolute, thinking it unadvisable to have two names for the same thing, and wishing to restrict, if it might be, the term contorted to cases of torsion. Adrien de Jussieu, on the other hand, used convolute (with strict Linnæan propriety) for regular imbrication with a high degree of overlapping, thus giving two names to different degrees of the same thing.

It being conceded, I presume, that the mode II should be specifically distinguished, what name, on the whole, ought it to bear? If we follow prevalent usage, contorta will be the term. But this term was unknown in this sense to the founders of æstivation, Linnæus and Brown; it correctly expresses the real state of things in only a few cases; and where there is torsion, it leads to a most awkward way of expressing it. We have to write — "lobes of the corolla contorted and twisted: corollæ lobi contorti et torti," introducing dextrorsum or sinistrorsum,[1] to express the direction of the overlapping and of the torsion, which are not always the same. So the most current name is the least appropriate. Convoluta is as good a name as can be, and its use in the present sense is not unconformable with the Linnæan use in vernation. When well carried out, three or five or more petals, as the case may be, are simply rolled up together. When the overlapping is slight, there is simply the tendency to convolution. But if, as in other nomenclature, priority gives a paramount

[1] I note with satisfaction that Bentham and Hooker use these terms to signify from left to right, or from right to left, of a person, supposed to stand outside of the closed bud, which is surely the natural position of the observer.

claim, obvoluta will be the proper term, beginning as it did with Linnæus for vernation, and taken up, as it was very early, by Mirbel for æstivation. The only objections to it are, first, that it has never come into systematic use, and, second, that *ob*, in the composition of botanical terms, commonly stands for obversely or inversely. But obvoluta is not burdened with this signification: it is classical for "wrapped round," as is convoluta for rolled together. I conclude that one or the other of these two terms ought to be used.

Finally, although there is little, if any, practical misuse, there is some mis-definition, of the term imbricate as applied to æstivation. Adrien de Jussieu defines it well (in Cours Élémentaire, 308) in the phrase "La préfloraison spirale est aussi nommé imbriquée"; and in noting that when the number stops at five, the pieces fall into two exterior, two interior, and one (the third in the spiral) intermediate, this making what is called "æstivatio quincuncialis."[1] This is clear and to the point. But other authors have had a fancy for distinguishing between quincuncial and imbricate (as if the former were not the typical case of the latter when the parts are five), and so have had to devise something else to answer to imbricate. Alphonse De Candolle (in his Introd. Bot., i. 154, written before phyllotaxy was well understood), after relegating imbricative to the category of a crowd of verticils, and remarking that the quincuncial is sometimes confounded with the imbricate, adds that some confound also under this latter name the case in which there is one exterior piece, one interior, and three covered at one margin but free at the other. I know not where this began; but its latest reproduction is in Le Maout and Decaisne's "Traité Générale" and in the English translation of it. In the diagram the pieces are numbered directly round the circle from 1 to 5, the fifth coming next the first: "so they thus complete one turn of a spiral,"—which shows that Le Maout had vague ideas of phyllotaxy, of

[1] The name quincuncial answers the purpose after definition, and has long been in use; but this arrangement in diagram is wholly unlike the quincunx, with its four pieces or stars in the periphery, or at the angles of a square, and one in the centre.

which he seems to have invented a new ($\frac{1}{5}$) order. Moreover, this is essentially identical with the "cochlear" æstivation of the same work (not of Lindley); and Eichler, in his "Blüthendiagramme," adopts this name (unsuitable though it be) for this particular arrangement, whatever be the position of the inclosed or inclosing petal. A glance shows that this supposed "true imbricate æstivation" is a slight and not very uncommon deviation (by the displacement of what should be the interior margin of one of the petals during growth) of the mode II, variously termed obvolute, convolute, or contorted æstivation. But it is so intermediate between this and the quincuncially imbricate as perhaps to justify Brown in applying the name imbricate generically to all the overlapping modes. I see, since the above was written, that Eichler, in his "Blüthendiagramme," in effect does this. I find also, that Eichler uniformly employs the term convolute, or convolutive, as I have done, instead of contorted. I should hope, rather than immediately expect, that this use would become general.

A PILGRIMAGE TO TORREYA.[1]

ORDERED to go south until I should meet the tardy spring and summer, I was expected to follow the beaten track to east Florida. But I wished rather to avoid the crowd of invalids and pleasure travelers, and turned my attention in preference to western Florida, determined that, if possible, I would make a pious pilgrimage to the secluded native haunts of that rarest of trees, the *Torreya taxifolia.*

All that I knew, or could at the moment learn, was, that this peculiar evergreen Yew-like tree — prized by arboriculturists for its elegance, and dear to us botanists for the name it bears and commemorates — grew on the banks of the Apalachicola River, somewhere near the confluence of the Flint and Chattahoochee, which by their union form it. It was there discovered, nearly forty years ago, by Mr. Hardy B. Croom, and had since been seen, at two or three stations, by his surviving associate Dr. Chapman, of Apalachicola, author of the Southern Flora. Mr. Croom, upon ascertaining that he was the fortunate discoverer of an entirely new type of coniferous trees, desired that it should bear Dr. Torrey's name; and the genus Torreya was accordingly so named and characterized by the Scotch botanist Arnott. It is of the Yew family, in foliage and in male flowers much resembling the Yew itself, but more graceful than the European Yew-tree, wholly destitute of the berry-like cup which characterizes the latter genus, and with the naked seed itself fleshly-coated, and larger than an olive, which it resembles in shape and appearance. One young tree, brought or sent by Mr. Croom himself, has been kept alive at New York — showing its aptitude for a colder climate than that of which it is a native — and has been more or less multiplied by cuttings.[2] Sprigs from this tree

[1] "American Agriculturist," 1875, 262.

[2] "The American Agriculturist" for May states that the tree spoken

or its progeny were appropriately borne by the members of the Torrey Botanical Club, at its founder's funeral, two years ago, and laid upon his coffin. But very few botanists have ever seen the tree growing wild and in its full development. I was desirous to be one of the number.

Among the broad, black lines with which the railway map is chequered, I found one which terminates at Chattahoochee. This was the objective point, and the way to it seemed plain enough, though long. Pilgrimages to famous shrines by railway, in the Old World, are nowadays systematized and made easy. The untried one which I undertook appeared to offer no privation or difficulty, except the uncertainty whether I should be fortunate enough to find the grove which I sought. And, indeed, there was little privation to speak of. It was, however, rather trying to us (*i. e.*, to myself and my companion in travel and life), when, after leaving Savannah on an early April morning, with the assured understanding that we should reach Chattahoochee late that evening, we learned that we were to be left for twenty hours at a small hamlet on the borders of east Florida, named Live Oak, a manifest *lucus a non lucendo*, as there were no Live Oak-trees in the neighborhood, but a prevalent growth of Long-leaved Pines. There was some good botanizing to console us, and, thanks to the railroad conductor for directing us aright, unpretending but truly comfortable quarters for the night. Then, the next day, resuming our journey after a twelve-o'clock dinner, which we were to mend with a supper at Tallahassee, we were at length informed that we were to be supperless; that the stations both of Tallahassee and Quincy were out of town and out of reach of all edibles; that Chattahoochee station, to be reached after ten o'clock, was only a freight-house on the wild and wooded bank of the river, built upon piles in the swamp,

of, or its seed, "was brought from Florida by the late distinguished Major Le Conte." I am confident that this is a mistake, and that Le Conte knew nothing of this tree in its native station. If my recollection is correct, at least two seedling trees were placed in Dr. Torrey's hands by Mr. Croom, one of which was consigned to A. J. Downing, of Newburgh, the ultimate fate of which is unknown to me, the other to Mr. Hogg, senior, which, as "The American Agriculturist" states, is now in Central Park.

reached at ordinary times over a mile of trestles, and now so overflowed that it probably could not be reached at all, certainly not that night; that the train would stop for the night two or three miles back in the woods, where the agent had taken up his abode in a box-car; that the town of Chattahoochee, a mile away, large as it appeared on the map, consisted mainly of a state-prison, and a couple of grocery shops, neither of which was quite proper for passing a night in, even if we could reach it; in fine, that our only course would be to sleep in the car (which made no provision for it), and crave from the agent of the road a share of his breakfast.

The kind and intelligent fellow-travelers as far as Tallahassee and Quincy, who gave us this disheartening information, finding that we were not disposed to stop short of our object, remarked that they had set us down as eminently philosophical people, since we had passed a night at Live Oak and still possessed our souls in patience (a view which a couple who had stopped at the hotel there practically confirmed), and so left us with their good wishes, but evidently faint hopes. The weekly steamboat, which was to call at the landing next day, would eventually relieve us; and so we resolved to make the best of it. The worthy young conductor, who was to sleep in the car also, kindly proffered a share of his supper; but we fortunately had a bottle of cold tea, some crusts of bread ten days old, and wafer-biscuits, upon which we scantily supped, and then, folding around us such drapery and wraps as we had, lay down to sleep upon the couches which the conductor ingeniously arranged for us, by some skillful adjustment of the car-seats. In the morning, after due ablutions made at the tank of the locomotive, we were hospitably welcomed by the agent, General Dickison, and his son, to a much-needed share of their breakfast in the stationary box-car, which served both as bedroom, parlor, and dining-room.

To our great delight we found that General Dickison knew the tree which I was in search of; and it was arranged that his son should conduct me to the locality, not far distant. So striking an evergreen tree could not fail of notice. The people

of the district knew it by the name of "Stinking Cedar" or "Savine" — the unsavory adjective referring to a peculiar unpleasant smell which the wounded bark exhales. The timber is valued for fence-posts and the like, and is said to be as durable as red cedar. I may add that, in consequence of the stir we made about it, the people are learning to call it Torreya. They are proud of having a tree which, as they have rightly been told, grows nowhere else in the world.

My desire for a sight of it was soon gratified. Making our way into the woods north of the railroad track, along the ridges covered with a mixed growth of Pines and deciduous trees, I soon discerned a thrifty young Torreya, and afterwards several of larger size, some of them with male flowers just developed.

As we approached the first one, I told my companion that I expected to find under its shade a peculiar low herb, which I described, but had never yet seen growing wild. And there, indeed, it was, — greatly to the wonderment of my companion — the botanically curious little *Croomia pauciflora*, just as it was found by Mr. Croom, when he also discovered the tree, nearly forty years ago, probably at a station several miles farther south. I was a pupil and assistant of the lamented Torrey when Mr. Croom brought to him specimens, both of the tree and of the berb, both new genera. The former, as I have stated, was named for Dr. Torrey by his correspondent Arnott. The latter was dedicated to its discoverer by Dr. Torrey. I well remember Mr. Croom's remark upon the occasion, that if his name was deemed worthy of botanical honors, it was gratifying to him, and becoming to the circumstances, that it should be borne by the unpretending herb which delighted to shelter itself under the noble Torreya. It is not, as Mr. Croom then supposed, exclusively so found; for it grows also in the central and upper portions of Alabama and Georgia, where Torreya is unknown, but where I fancy it may once have flourished. I cannot here detail the reasons for this supposition.

There is a second Torreya in Japan, founded on Thunberg's *Taxus nucifera*, of which I saw original specimens at the

British Museum, in the winter of 1838–9, and then identified the genus. There is likewise in Japan a second Croomia, very probably in company with the Torreya. A third Torreya inhabits California, but it has no associate Croomia.

I have formerly treated of the peculiar distribution of these genera and species between the United States and Japan, have collocated a large number of equally striking similar instances, and have offered certain speculations in explanation of them. The views maintained have been more and more confirmed, and are now adopted by the leading philosophical botanists.

The few hours devoted to this first search for Torreya, pleasant as they were, yet were too scantily rewarded to satiate my interest. I saw no tree with trunk over six inches in diameter, and found no female blossoms. It was necessary to hasten back to the railway car, to await the expected summons to the steamboat. I bore with me, besides my botanical specimens, a stick of Torreya, suitable for a staff, which I propose to make over to the President of the Torrey Botanical Club, for the official baton. Before long the whistle of the steamboat announced its approach to the landing, and offered us a prospect of a much-needed dinner; the water had fallen sufficiently to allow us to be conveyed to the wharf upon a hand-car, and so we embarked for Apalachicola via Bainbridge. That is, we went up the Flint River about forty miles and thence back in the night, past the place of embarkation.

I will not here give any account of a delightful ten days' episode, beginning with the voyage down the brimming river, bordered with almost unbroken green of every tint, from the dark background of Long-leaved Pines to the tender new verdure of the Liquidambar and other deciduous trees in their freshest development, interspersed with the deep and lustrous hue of *Magnolia grandiflora*, and, when the banks were low, dominated by weird, naked trunks of Southern Cypress (Taxodium), their branches hung with long tufts and streamers of the gray and sombre Southern Moss (Tillandsia) below, while above they were just putting forth their delicate foliage.

Along the lower part of the river, occasional Palmettoes gave a still more tropical aspect. Then followed a week and more at dead and dilapidated, but still charming, Apalachicola, where the post-office opens on Monday evenings, when the steamboat arrives, and closes for a week the next morning, when she departs, — where the climate, thanks to the embracing Gulf, is as delicious in summer as it is bland in winter; where game, the best of fish, and the most luscious oysters are to be had almost for nothing, and blackberries come early in April when the oranges are gone; and where, far from the crowd and bustle of the world, with Bill Fuller for caterer, and his wife Adeline for cook, the choicest fare is to be enjoyed at the cheapest rate. Then there was the pleasure of renewing our acquaintance with Dr. Chapman, and botanizing with him over some of the ground which he has explored so long and so well, of gathering, under his guidance, the stately *Sarracenia Drummondii* in its native habitat, and, not least, acquiring from him fuller information respecting the localities where Torreya grows.

The return voyage up the river was not less enjoyable than the descent. It was so timed that the bold bluff of Aspalaga, where the tree was first found, was reached after sunrise. But it was sad to see that the Torreya trees, which overhung the river here in former days, had been cut away, perhaps for steamboat fuel. So I did not land; but leaving the boat a few miles above, at the upper Chattahoochee landing, while it made the run to Bainbridge and back, I had a long day to devote to Torreya. Following Dr. Chapman's directions, I repaired to the wooded bluff to the north of the road, where I soon found an abundance of the trees, of various ages, interspersed among other growth. The largest tree I saw grew near the bottom of a deep ravine; its trunk just above the base measured almost four feet in circumference, and was proportionally tall. But it was dominated by the noblest *Magnolia grandiflora* I ever set eyes on, with trunk seven and a half feet in girth.

After long search one tree was found with female flowers, or rather with forming fruit, from which a few specimens

were gathered. Seedlings and young trees are not uncommon, and some old stumps were sprouting from the base, in the manner of the Californian Redwood. So this species may be expected to endure, unless these bluffs should be wantonly disforested — against which their distance from the river and the steepness of the ground offer some protection. But any species of very restricted range may be said to hold its existence by a precarious tenure. The known range of this species is not more than a dozen miles in length along these bluffs, although Dr. Chapman has heard of its growing further south, where the bluff trends away from the river. At least the Yew-tree grows there, which Mr. Croom found with the Torreya near Aspalaga, and I heard of it (identifying it by the description) as growing five or six miles away.

Returning to the boat at nightfall, I brought with me thirty or forty seedling Torreyas, which, being too far advanced to be safely sent far north this spring, have been successfully consigned to the excellent Mr. Berckman's care, at Augusta, Georgia. I hope that one or more of them may in due time be planted upon the grave of Torrey.

A word or two of Mr. Croom and his sad fate. His name merely is known to botanists as the discoverer of *Torreya taxifolia* and of *Croomia pauciflora*, and as the author of a monograph of Sarracenia, in which the handsomest species, *S. Drummondii*, was originally described and figured. He was the first, after Chapman in 1836, to find this in blossom, Drummond having seen and collected the leaves only, in a winter visit to Apalachicola. Of the botanists who remember and personally knew him, only Dr. Chapman and myself survive. Mr. Croom, originally, I believe, of Newbern, Lenoir County, North Carolina, had a plantation at Quincy, Florida, and another at Mariana, opposite Aspalaga; and it was in passing from one to the other that he discovered the tree of which I have been discoursing, as well as the herbaceous plant which bears his name. He was an accomplished and most amiable young man, full of enterprise and zeal for botany, and much was expected from him. But, just as he was entering upon his chosen field, and had made prepara-

tions for a thorough exploration of Florida, in connection with his friend Dr. Chapman, he was lost at sea, with his wife and all his children, in the foundering of the ill-fated "Home," between New York and Charleston.

I have been told that two seedling Torreyas which Mr. Croom planted near his house at Quincy, and which had become stately trees, have recently been demolished by the present proprietor; also that a tree of Mr. Croom's planting still flourishes in the grounds of the state-house at Tallahassee.

NOTES ON THE HISTORY OF *HELIANTHUS TUBEROSUS*, THE SO-CALLED JERUSALEM ARTICHOKE.

UNDER this heading the botanical editor of the Journal[1] proposes to offer a few explanatory remarks, introductory to the subjoined letter which he received from Mr. Trumbull in answer to a recent inquiry.

Linnæus, in the "Species Plantarum," gave to *Helianthus tuberosus* the "habitat in Brasilia." In his earlier "Hortus Cliffortianus" the habitat assigned was "Canada." M. Alphonse De Candolle, in his "Géographie Botanique," ii. 824 (1855), refers to this as "decidedly an error, at least as to Canada properly so called," assigns good reasons for the opinion that it did not come from Brazil, nor from Peru (to which the name under which it appeared in cultivation in the Farnese garden seemed to refer), but in all probability from Mexico or the United States. He adds that Humboldt did not meet with it in any of the Spanish colonies.

About this time I received from my friend and correspondent, the late Dr. Short of Kentucky, some long and narrow tubers of *Helianthus doronicoides*, Lam., with the statement that he and some of his neighbors found them good food for hogs, and, if I rightly remember, had planted them for that purpose. They were planted here in the Botanic Garden; after two or three years it was found that some of the tubers produced were thicker and shorter; some of these were cooked along with Jerusalem artichokes, and found to resemble them in flavor, although coarser. Consequently, in the second edition of my "Manual of the Botany of the Northern United States" (1856), it is stated that *H. doronicoides* is most probably the original of *H. tuberosus*. This opinion was

[1] American Journal of Science and Arts, 3 ser., xiii. 347. (1877.)

strengthened year after year by the behavior of the tubers, and by the close similarity of the herbage and flowers of the two plants, as they grew side by side; indeed, as the two patches were allowed to run together in a waste or neglected place, they have become in a measure confounded. Wishing to obtain an unmixed stock, I applied last autumn to Professor J. M. Coulter of Hanover, Indiana, and received from him a good number of tubers from wild plants of the neighborhood, which will now be grown. Some of these were slender, some thicker and shorter, and a few were to all appearance identical with Jerusalem artichokes. If they were really all from one stock, as there is reason to believe, the question of the origin of *Helianthus tuberosus* is wellnigh settled.

We were now interested to know whether our Indians, at least those of the Mississippi Valley, where *H. doronicoides* belongs, were known to cultivate these tubers or to use them for food. Recently a note in the "American Agriculturist" called attention to a sentence in Dr. Palfrey's "History of New England," i. 27, stating that the Indians of that region raised, among other articles of food, "a species of Sunflower, whose esculent tuberous root resembled the artichoke in taste." The venerable historian found himself at the moment unable to refer me to the sources of this statement; but as it was now certain that some record of the kind existed, I applied to Mr. Trumbull, who obligingly and promptly supplied the information required, and placed it at my disposal in the following letter.

HARTFORD, CONN., March 26, 1877.

MY DEAR PROFESSOR GRAY: I cannot refer you to the authority (*totidem verbis*) for Dr. Palfrey's statement that the Indians of New England cultivated "a species of Sunflower, whose esculent tuberous root resembled the artichoke in taste," but there can be, I think, little doubt of the fact. The historical evidence that "artischoki sub terrâ" were cultivated in Canada and in some parts of New England before the coming of Europeans is tolerably clear. The only question, if there be any, is as to species; and this does not appear to have been raised for more than half a century after the

"Jerusalem artichoke" was known to English and Continental botanists.

I can discover no authority whatever, before 1700, for ascribing to the *Helianthus tuberosus*, either a Brazilian or a Mexican origin, except — and the exception is unimportant — in C. Bauhin's identification (in his Pinax, 277) with "Helianthemum Indicum tuberosum" (*H. tuberosus*, L.), of a plant that he had described in his earlier "Prodromus" (ed. 1671, p. 70) as "*Chrysanthemon latifolium Brasilianum*," from a dried specimen sent to him "eo nomine" from the garden of Contarini.

The first trace I find of this species, in Europe, is in the 2d part (cap. 6) of Fabio Colonna's "Ecphrasis minus cognitarum stirpium," published at Rome in 1616. He described it from a plant growing in the garden of Cardinal Farnese. The Sunflower was already well known to European botanists, and had been described and figured by Dodoens (1563) and Lobel (1576) as *Chrysanthemum Peruvianum* and *Flos solis Peruvianus*. With reference to these descriptions, probably, Colonna gave the new species the name of *Aster Peruanus, tuberosâ radice*, otherwise *Solis flos Farnesianus*. (He gave a more particular description of the plant in his annotations to Recchi's Hernandez, "Plant. Mexic. Hist.," 1651, pp. 878, 881, as *Peruanus Solis flos ex Indiis tuberosus*.)

The author of the "Descriptio variorum plantarum, in Horto Farnesiano," published under the name of Tobias Aldinus (Rome, 1625), gave some account of the roots, which he calls "Tubera Indica," of the *Solis flos tuberosus, seu Flos Farnesianus Fabii Columnæ* (p. 91). It may be observed that several of the rarer plants in the Farnese garden, at this time, were from "Canada" and "Virginia." The Passion Flower (admirably figured by Aldinus) is described under its Virginian name, "Maracot" (the "Maracocks" of John Smith and Strachey); and a *Campanula Americana* is otherwise named "*Campanula Virginiana, seu ex Virginiis insulis*."

C. Bauhin, in his "Pinax" (first published in 1623), ed.

1671, p. 276, notes that the "*Helianthemum Indicum tuberosum*" is called "Chrysanthemum è Canada, quibusdam. Canada et Artischoki sub terrâ, aliis. Gigantea, Burgundis."

P. Laurenberg, "Apparat. plant." (Rostock, 1632), names the species "Adenes Canadenses or Flos Solis glandulosus." Ant. Vallot, "Hortus Regis, Paris," 1665 (as cited by Bauhin), gives the names "Canada and Artischoki sub terrâ," and "Canadas," and describes also "*Helenium Canadense altissimum, Vosacan dictum*," which Tournefort distinguishes as "Corona Solis rapunculi radice" (Inst. Herb. 490), and which became *H. strumosus*, L. "Vosacan," by the way, is a French fashion of writing the Algonkin word "wassakone" or "wassakwân," which means a "bright yellow flower." The modern Chippeways give this name to the flowers of the Pumpkin and Squash.

Under whatever name the Jerusalem Artichoke was described, there seems to have been a general agreement among European botanists that it came from Canada. F. Schuyl, "Catal. Horti Lugd. Bat." (Heidelberg, 1672), varies the specific name to "*Chrysanthemum Canadense Arumosum*." P. Amman, "Charac. Plant. Nat." (1676), has "*Helenium Canadense*."

It was introduced to England about 1617. In that year, Mr. John Goodyer, of Maple Durham, Hampshire, "received two small roots thereof, from Mr. Franquevill of London," which were planted, and enabled him, before 1621, to "store Hampshire." He wrote an account of the plant, under date of October 17, 1621, for T. Johnson, — who printed it in his edition of Gerard, 1636 (p. 753). Before this the species had been figured and described by J. Parkinson, in "Paradisus Terrestris" (London, 1629), as "Battatas de Canada," and in his "Theatre of Plants," 1640 (p. 1383), he has the figure — a good one — without the description, under the names "Battatas de Canada, the French Battatas, or Hierusalem Artichoke." Johnson, in Gerard (p. 753), refers to Parkinson's description, and gives the name as "Flos Solis Pyramidalis, Jerusalem Artichoke." It already grew "well and plentifully in many parts of England."

The notices by early voyagers, of ground-nuts, eaten by the Indians, are generally so brief and so vague, that it is not easy to distinguish the three or four species mentioned under that name or its equivalents. The *Solanum tuberosum*, *Apios tuberosa*, *Aralia trifolia*, and a *Cyperus* (*articulatus?*) were all "ground-nuts," or "earth-nuts." We find, however, in a few instances, unmistakable mention of the roots already known in Europe as "Canadian."

Brereton, in his account of Gosnold's voyage to New England in 1602, notes the "great store of ground-nuts" found on all the Elizabeth Islands. They grow "forty together on a string, some of them as big as a hen's egg" (Purchas, iv. 1651). These, doubtless, were the roots of *Apios tuberosa*. But when Champlain, a few years later (1605–6), was in the same region, he observed that the Almouchiquois Indians near Point Mallebarre (Nauset harbor, probably,) had "force des racines qu'els cultivent, lesquelles ont le goût d'artichaut" (Voyages, ed. 1632, p. 84). And it is to these roots, evidently, that Lescarbot alludes, "Histoire de la Nouv. France," 1612 (p. 840): there is, he says, in the country of the Armouchiquois (*i. e.*, New England, west and south of Maine), a certain kind of roots "grosses comme naveaux, très excellentes a manger, ayans un goût retirant aux cardes, mais plus agréable, lesquelles plantées multiplient en telle façon que c'est merveille;" and he thinks these must be the "Afrodilles" described by Pliny.

Sagard-Theodat (Hist. du Canada, 1636, p. 785) mentions the cultivation of the Sunflower by the Hurons — who extracted oil from its seeds, — and names also the "roots that we [the French] call Canadiennes or Pommes de Canada, and that the Hurons call 'Orasqueinta,' which are not very (*assez peu*) common in their country. They eat them raw, as well as cooked, as they eat another sort of root resembling parsnips [*Sium lineare?*], which they call Sondhratates, and which are much better; but they seldom gave us these, and only when they received some present from us or when we visited them in their cabins." He goes on to speak of "patates, fort grosses et très-excellentes," some of which he had

obtained from an English vessel captured by the French; but none of these were to be found in the Huron country, nor could the Indians tell him the name of them; and he regretted that he had not brought some with him, for planting, since "this root, being cut in pieces and planted, quickly grows and multiplies, it is said, like the pommes de Canada" (pp. 781, 782). It is plain that the Huron roots first mentioned were, or that Sagard believed them to be, "Jerusalem Artichokes," — already known as "Canadian."

I find no mention of the artichoke in Virginia or the southern colonies before it was cultivated by Anglo-Americans. The author of "A Perfect Description of Virginia," printed in 1649, says that the English planters have (*inter alia*) "roots of several kindes, Potatoes, Sparagus, Carrets, . . . and Hartichokes." Beverly (Hist. of Virginia, 1722, p. 254) mentions "Batatas Canadensis, or Jerusalem Artichoke," as planted by some of the English, for brewing beer. Yet, the name of one of the esculent roots mentioned by Hariot (Brief and True Report, etc., 1585) ought to belong to some species of Sun Flower — and if to any, to *H. tuberosus*. Hariot names three tuberous roots found in Virginia: "Openauk, a kind of roots of round form, some of the bignes of walnuts, some far greater, which are found in moist and marish grounds growing many together one by another in ropes, or as though they were fastened with a string. Being boiled or sodden, they are very good meate." [C. Bauhin (Prodromus, 89) identifies these with *Solanum tuberosum esculentum*, — and has been followed by later writers. The description seems to me to indicate *Apios tuberosa*.] "Kaishucpenauk, a white kind of roots about the bignes of hen egs and nere of that forme: their taste was not so good to our seeming as of the other, and therefore their place and manner of growing not so much cared for by vs: the inhabitants notwithstanding vsed to boile and eat many." These may be "Virginia potatoes," but their name, if Hariot recorded it correctly, means "Sun-tubers." The etymology is perfectly clear. The other roots described by Hariot, "Okeepenauk are also of round shape, found in dry grounds: some are of

the bignes of a man's head," etc. These must be the "Tubera terræ maxima," of Clayton, "vulgo Tuckahoo," which Gronovius (Fl. Virgin. 205) refers to *Lycoperdon solidum*, L., and for which Rafinesque (Med. Fl. ii. 270) proposed a new genus Tucahus. Kalm describes them (Travels, i. 225) as "Truffles." Fries (El. Fung. ii. 39) assigns them to his *Pachyma cocos*.

Writing in haste and with frequent interruptions, it has been possible to do little more than copy, without condensing or arranging, such notes as I had before me. They have extended to such a length that I must not add even an apology for the superfluous matter. Yours truly,

J. H. TRUMBULL.

It would be interesting to know whence came the French name of these Helianthus tubers, "Topinambour," it being the only thing in the case which, as Mr. Trumbull remarks, "looks to a Brazilian origin, as it seems to be derived (and so Littré gives it) from the Topinamboux Indians of Brazil." The English name, "Jerusalem Artichoke," comes, as is well known, from the Italian "Girasola," *i. e.* Sunflower.

As to the annual Sunflower, or *Helianthus annuus*, said by Linnæus to come from Peru and Mexico, I have for some years been convinced that its original is the *H. lenticularis* of Douglas, which again is probably only a larger form of *H. petiolaris* of Nuttall, natives of the western part of the Mississippi Valley and of the plains to and beyond the Rocky Mountains. It is an interesting confirmation of this opinion that Sagard (as mentioned in the above communication) and Champlain found this Sunflower in cultivation by the Huron Indians, for the sake of the oil of its seeds, which they used for hair-oil.

FOREST GEOGRAPHY AND ARCHÆOLOGY.[1]

It is the forests of the Northern temperate zone which we are to traverse. After taking some note of them in their present condition and relations, we may inquire into their pedigree; and, from a consideration of what and where the component trees have been in days of old, derive some probable explanation of peculiarities which otherwise seem inexplicable and strange.

In speaking of our forests in their present condition, I mean not exactly as they are to-day, but as they were before civilized man had materially interfered with them. In the district we inhabit such interference is so recent that we have little difficulty in conceiving the conditions which here prevailed, a few generations ago, when the "forest primeval" — described in the first lines of a familiar poem — covered essentially the whole country, from the Gulf of St. Lawrence and Canada to Florida and Texas, from the Atlantic to beyond the Mississippi. This, our Atlantic forest, is one of the largest and almost the richest of the temperate forests of the world. That is, it comprises a greater diversity of species than any other, except one.

In crossing the country from the Atlantic westward, we leave this forest behind us when we pass the western borders of those organized States which lie along the right bank of the Mississippi. We exchange it for prairies and open plains, wooded only along the watercourses, — plains which grow more and more bare and less green as we proceed westward, with only some scattering Cottonwoods (*i. e.* Poplars) on the immediate banks of the traversing rivers, which are themselves far between.

[1] A lecture delivered before the Harvard University Natural History Society, April 18, 1878. (American Journal of Science and Arts, 3 ser., xvi. 85, 183.)

In the Rocky Mountains we come again to forest, but only in narrow lines or patches; and if you travel by the Pacific Railroad you hardly come to any: the eastern and the interior-desert plains meet along the comparatively low level of the divide which here is so opportune for the railway; but both north and south of this line the mountains themselves are fairly wooded. Beyond, through all the wide interior basin, and also north and south of it, the numerous mountain chains seem to be as bare as the alkaline plains they traverse, mostly north and south; and the plains bear nothing taller than sage-brush. But those who reach and climb these mountains find that their ravines and higher recesses nourish no small amount of timber, though the trees themselves are mostly small and always low.

When the western rim of this great basin is reached there is an abrupt change of scene. This rim is formed of the Sierra Nevada. Even its eastern slopes are forest-clad in great measure, while the western bear in some respects the noblest and most remarkable forest of the world, — remarkable even for the number of species of evergreen trees occupying a comparatively narrow area, but especially for their wonderful development in size and altitude. Whatever may be claimed for individual Eucalyptus-trees in certain sheltered ravines of the southern part of Australia, it is probable that there is no forest to be compared for grandeur with that which stretches, essentially unbroken, — though often narrowed, and nowhere very wide, — from the southern part of the Sierra Nevada in lat. 36° to Puget Sound beyond lat. 49°, and not a little farther.

Descending into the long valley of California, the forest changes, dwindles, and mainly disappears. In the Pacific coast ranges it resumes its sway, with altered features, some of them not less magnificent and of greater beauty. The Redwoods of the coast, for instance, are little less gigantic than the Big Trees of the Sierra Nevada, and far handsomer, and a thousand times more numerous. And several species, which are merely or mainly shrubs in the drier Sierra, become lordly trees in the moister air of the northerly coast

ranges. Through most of California these two Pacific forests are separate; in the northern part of that State they join, and form one rich woodland belt, skirting the Pacific, backed by the Cascade Mountains, and extending through British Columbia into our Alaskan territory.

So we have two forest regions in North America, — an Atlantic and a Pacific. They may take these names, for they are dependent upon the oceans which they respectively border. Also we have an intermediate isolated region or isolated lines of forest, flanked on both sides by bare and arid plains, — plains which on the eastern side may partly be called prairies, on the western, deserts.

This mid-region mountain forest is intersected by a transverse belt of arid and alkaline plateau, or eastward of grassy plain — a hundred miles wide from north to south, — through which passes the Union Pacific Railroad. This divides the Rocky Mountain forest into a southern and a northern portion. The southern is completely isolated. The northern, in a cooler and less arid region, is larger, broader, more diffused. Trending westward, on and beyond the northern boundary of the United States, it approaches, and here and there unites with, the Pacific forest. Eastward, in northern British territory, it makes a narrow junction with northwestward prolongations of the broad Atlantic forest.

So much for these forests as a whole, their position, their limits. Before we glance at their distinguishing features and component trees, I should here answer the question, why they occupy the positions they do; — why so curtailed and separated at the south, so much more diffused at the north, but still so strongly divided into eastern and western. Yet I must not consume time with the rudiments of physical geography and meteorology. It goes without saying that trees are nourished by moisture. They starve with dryness and they starve with cold. A tree is a sensitive thing. With its great spread of foliage, its vast amount of surface which it cannot diminish or change, except by losing that whereby it lives, it is completely and helplessly exposed to every atmospheric change; or at least its resources for adaptation are

very limited; and it cannot flee for shelter. But trees are social, and their gregarious habits give a certain mutual support. A tree by itself is doomed, where a forest, once established, is comparatively secure.

Trees vary as widely as do other plants in their constitution; but none can withstand a certain amount of cold and other exposure, nor make head against a certain shortness of summer. Our high northern regions are therefore treeless; and so are the summits of high mountains in lower latitudes. As we ascend them we walk at first under Spruces and Fir-trees or Birches; at 6000 feet on the White Mountains of New Hampshire, at 11,000 or 12,000 feet on the Colorado Rocky Mountains, we walk through or upon them; sometimes upon dwarfed and depressed individuals of the same species that made the canopy below. These depressed trees retain their hold on life only in virtue of being covered all winter by snow. At still higher altitudes the species are wholly different; and for the most part these humble alpine plants of our temperate zone — which we cannot call trees, because they are only a foot or two or a span or two high — are the same as those of the arctic zone, of northern Labrador, and of Greenland. The arctic and the alpine regions are equally unwooded from cold.

As the opposite extreme, under opposite conditions, look to equatorial America, on the Atlantic side, for the wildest and most luxuriant forest-tract in the world, where winter is unknown, and a shower of rain falls almost every afternoon. The size of the Amazon and Orinoco — brimming throughout the year — testifies to the abundance of rain and its equable distribution.

The other side of the Andes, mostly farther south, shows the absolute contrast, in the want of rain, and absence of forest; happily it is a narrow tract. The same is true of great tracts either side of the equatorial regions, the only district where great deserts reach the ocean.

It is also true of great continental interiors out of the equatorial belt, except where cloud-compelling mountain-chains coerce a certain deposition of moisture from air which

could give none to the heated plains below. So the broad interior of our country is forestless from dryness in our latitude, as the high northern zone is forestless from cold.

Regions with distributed rain are naturally forest-clad. Regions with scanty rain, and at one season, are forestless or sparsely wooded, except they have some favoring compensations. Rainless regions are desert.

The Atlantic United States in the zone of variable weather and distributed rains, and the Gulf of Mexico as a caldron for brewing rain, and no continental expanse between that great caldron and the Pacific, crossed by a prevalent southwest wind in summer, is greatly favored for summer as well as winter rain.

And so this forest region of ours, with an annual rainfall of fifty inches on the lower Mississippi, fifty-two inches in all the country east of it bordering the Gulf of Mexico, forty-five to forty-one in all the proper Atlantic district from east Florida to Maine and the whole region drained by the Ohio, — diminished only to thirty-four inches on the whole upper Mississippi and Great Lake region, — with this amount of rain, fairly distributed over the year, and the greater part not in the winter, our forest is well accounted for.

The narrow district occupied by the Pacific forest has a much more unequal rainfall, — more unequal in its different parts, most unequal in the different seasons of the year, very different in the same place in different years.

From the Gulf of Mexico to the Gulf of St. Lawrence, the amount of rain decreases moderately and rather regularly from south to north ; but, as less is needed in a cold climate, there is enough to nourish forest throughout. On the Pacific coast, from the Gulf of California to Puget Sound, the southerly third has almost no rain at all ; the middle portion has less than our Atlantic least ; the northern third has about our Atlantic average.

Then, New England has about the same amount of rainfall in winter and in summer ; Florida and Alabama about one half more in the three summer than in the three winter months, — a fairly equable distribution. But on the Pacific

coast there is no summer rain at all, except in the northern portion, and there little. And the winter rain, of forty-four inches on the northern border, diminishes to less than one half before reaching the Bay of San Francisco; dwindles to twelve, ten, and eight inches on the southern coast, and to four inches before we reach the United States boundary below San Diego.

Taking the whole year together, and confining ourselves to the coast, the average rainfall for the year, from Puget Sound to the border of California, is from eighty inches at the north to seventy at the south, *i. e.*, seventy on the northern edge of California; thence it diminishes rapidly to thirty-six, twenty (about San Francisco), twelve, and at San Diego to eight inches.

The two rainiest regions of the United States are the Pacific coast north of latitude forty-five, and the northeastern coast and borders of the Gulf of Mexico. But when one is rainy the other is comparatively rainless. For while this Pacific rainy region has only from twelve to two inches of its rain in the summer months, Florida, out of its forty to sixty, has twenty to twenty-six in summer, and only six to ten in the winter months.

Again, the diminution of rainfall, as we proceed inland from the Atlantic and Gulf shores, is gradual; the expanse that is or was forest-clad is very broad, and we wonder only that it did not extend farther west than it does.

On the other side of the continent, at the north, the district so favored with winter rain is but a narrow strip, between the ocean and the Cascade Mountains. East of the latter, the amount abruptly declines, — for the year, from eighty inches to sixteen; for the winter months, from forty-four and forty to eight and four inches; for the summer months, from twelve and four to two and one.

So we can understand why the Cascade Mountains abruptly separate dense and tall forest on the west from treelessness on the east. We may conjecture, also, why this north Pacific forest is so magnificent in its development.

Equally, in the rapid decrease of rainfall southward, in its

corresponding restriction to one season, in the continuation of the Cascade Mountains as the Sierra Nevada, cutting off access of rain to the interior, in the unbroken stretch of coast ranges near the sea, and the consequent small and precarious rainfall in the great interior valley of California, we see reasons why the Californian forest is mainly attenuated southward into two lines, — into two files of a narrow but lordly procession, advancing southward along the coast ranges, and along the western flank of the Sierra Nevada, leaving the long valley between comparatively bare of trees.

By the limited and precarious rainfall of California we may account for the limitations of its forest. But how shall we account for the fact that this district of comparatively little rain produces the largest trees in the world — not only produces, alone of all the world, those two peculiar Big Trees which excite our special wonder, — their extraordinary growth might be some idiosyncrasy of a race, — but also produces Pines and Fir-trees whose brethren we know, and whose capabilities we can estimate, upon a scale only less gigantic? Evidently there is something here wonderfully favorable to the development of trees, especially of coniferous trees; and it is not easy to determine what it can be.

Nor, indeed, does the rainfall of the coast of Oregon, great as it is, fully account for the extraordinary development of its forest; for the rain is nearly all in winter, very little in summer. Yet here is more timber to the acre than in any other part of North America, or perhaps in any other part of the world. The trees are never so enormous in girth as some of the Californian, but are of equal height, at least on the average, three hundred feet being common, and they stand almost within arms' length of each other.

The explanation of all this may mainly be found in the great climatic differences between the Pacific and the Atlantic sides of the continent; and the explanation of these differences is found in the difference in the winds and the great ocean currents.

The winds are from the ocean to the land all the year round, from northwesterly in summer, southwesterly in win-

ter. And the great Pacific Gulf-stream sweeps toward and along the coast, instead of bearing away from it, as on our Atlantic side.

The winters are mild and short, and are to a great extent a season of growth, instead of suspension of growth as with us. So there is a far longer season available to tree vegetation than with us, during all of which trees may either grow or accumulate the materials for growth. On our side of the continent and in this latitude, trees use the whole autumn in getting ready for a six-months winter, which is completely lost time.

Finally, as concerns the west coast, the lack of summer rain is made up by the moisture-laden ocean winds, which regularly every summer afternoon wrap the coast-ranges of mountains, which these forests affect, with mist and fog. The Redwood, one of the two California Big Trees, — the handsomest and far the most abundant and useful, — is restricted to these coast ranges, bathed with soft showers fresh from the ocean all winter, and with fogs and moist ocean air all summer. It is nowhere found beyond the reach of these fogs. South of Monterey, where this summer condensation lessens, and winter rains become precarious, the Redwoods disappear, and the general forest becomes restricted to favorable stations on mountain sides and summits. . . . The whole coast is bordered by a line of mountains, which condense the moisture of the sea-breezes upon their cool slopes and summits. These winds, continuing eastward, descend dry into the valleys, and warming as they descend, take up moisture instead of dropping any. These valleys, when broad, are sparsely wooded or woodless, except at the north, where summer rain is not very rare.

Beyond stretches the Sierra Nevada, all rainless in summer, except local hail-storms and snowfalls on its higher crests and peaks. Yet its flanks are forest-clad; and, between the levels of 3000 and 9000 feet, they bear an ample growth of the largest coniferous trees known. In favored spots of this forest, and only there, are found those groves of the giant Sequoia, near kin of the Redwood of the coast ranges, whose trunks are from fifty to ninety feet in circumference, and whose height is

from two hundred to three hundred and twenty-five feet. And in reaching these wondrous trees you ride through miles of Sugar Pines, Yellow Pines, Spruces, and Firs, of such magnificence in girth and height, that the Big Trees, when reached — astonishing as they are — seem not out of keeping with their surroundings.

I cannot pretend to account for the extreme magnificence of this Sierra forest. Its rainfall is in winter, and of unknown but large amount. Doubtless most of it is in snow, of which fifty or sixty feet fall in some winters, and — different from the coast and from Oregon, where it falls as rain, and at a temperature which does not suspend vegetable action — here the winter must be complete cessation. But with such great snowfall the supply of moisture to the soil should be abundant and lasting.

Then the Sierra — much loftier than the coast ranges, rising from 7000 or 8000 to 11,000 and 14,000 feet — is refreshed in summer by the winds from the Pacific, from which it takes the last drops of available moisture; and mountains of such altitude, to which moisture from whatever source or direction must necessarily be attracted, are always expected to support forests, — at least when not cut off from sea-winds by interposed chains of equal altitude. Trees such mountains will have. The only and the real wonder is, that the Sierra Nevada should rear such immense trees!

Moreover, we shall see that this forest is rich and superb only in one line; that, beyond one favorite tribe, it is meagre enough. Such for situation, and extent, and surrounding conditions, are the two forests — the Atlantic and Pacific — which are to be compared.

In order to come to this comparison, I must refrain from all account of the intervening forest of the Rocky Mountains — only saying that it is comparatively poor in the size of its trees and the number of species; that few of its species are peculiar, and those mostly in the southern part, and of the Mexican plateau type; that they are common to the mountain chains which lie between, stretched north and south *en echelon*, all through that arid or desert region of Utah and Nevada, of

which the larger part belongs to the great basin between the Rocky Mountains and the Sierra Nevada; that most of the Rocky Mountain trees are identical in species with those of the Pacific forest, except far north, where a few of our eastern ones are intermingled. I may add that the Rocky Mountains proper get from twelve to twenty inches of rain in the year, mostly in winter snow, some in summer showers.

But the interior mountains get little, and the plains or valleys between them less; the Sierra arresting nearly all the moisture coming from the Pacific, the Rocky Mountains all coming from the Atlantic side.

Forests being my subject, I must not tarry on the woodless plain — on an average 500 miles wide — which lies between what forest there is in the Rocky Mountains and the western border of our eastern wooded region. Why this great sloping plain should be woodless — except where some Cottonwoods and their like mark the course of the traversing rivers — is, on the whole, evident enough. Great interior plains in temperate latitudes are always woodless, even when not very arid. This of ours is not arid to the degree that the corresponding regions west of the Rocky Mountains are. The moisture from the Pacific which those could otherwise share is — as we have seen — arrested on or near the western border by the coast-ranges and again by the Sierra Nevada; and so the interior (except for the mountains) is all but desert.

On the eastern side of the continent, the moisture supplied by the Atlantic and the Gulf of Mexico meets no such obstruction. So the diminution of rainfall is gradual instead of abrupt. But this moisture is spread over a vast surface, and it is naturally bestowed, first and most on the seaboard district, and least on the remote interior. From the lower Mississippi eastward and northward, including the Ohio River basin, and so to the coast, and up to Nova Scotia, there is an average of forty-seven inches of rain in the year. This diminishes rather steadily westward, especially northwestward, and the western border of the ultra-Mississippian plain gets less than twenty inches.

Indeed, from the great prevalence of westerly and southerly

winds, what precipitation of moisture there is on our western plains is not from Atlantic sources, nor much from the Gulf. The rain-chart plainly shows that the water raised from the heated Gulf is mainly carried northward and eastward. It is this which has given us the Atlantic forest-region ; and it is the limitation of this which bounds that forest at the west. The line on the rain-chart indicating twenty-four inches of annual rain is not far from the line of the western limit of trees, except far north, beyond the Great Lakes, where, in the coolness of high latitudes, as in the coolness of mountains, a less amount of rainfall suffices for forest-growth.

We see, then, why our great plains grow bare as we proceed from the Mississippi westward ; though we wonder why this should take place so soon and so abruptly as it does. But, as already stated, the general course of the wind-bearing rains from the Gulf and beyond is such as to water well the Mississippi Valley and all eastward, but not the district west of it.

It does not altogether follow that, because rain or its equivalent is needed for forest, therefore wherever there is rain enough, forest must needs cover the ground. At least there are some curious exceptions to such a general rule, — exceptions both ways. In the Sierra Nevada we are confronted with a stately forest along with a scanty rainfall, with rain only in the three winter months. All summer long, under those lofty trees, if you stir up the soil you may be choked with dust. On the other hand, the prairies of Iowa and Illinois, which form deep bays or great islands in our own forest region, are spread under skies which drop more rain than probably ever falls on the slopes of the Sierra Nevada, and give it at all seasons. Under the lesser and brief rains we have the loftiest trees we know; under the more copious and well-dispersed rain, we have prairies, without forest at all.

There is little more to say about the first part of this paradox ; and I have not much to say about the other. The cause or origin of our prairies — of the unwooded districts this side of the Mississippi and Missouri — has been much discussed, and a whole hour would be needed to give a fair account of

the different views taken upon this knotty question. The only settled thing about it is, that the prairies are not directly due to a deficiency of rain. That the rain-charts settle, as Professor Whitney well insists.

The prairies which indent or are inclosed in our Atlantic forest region, and the plains beyond this region, are different things. But as the one borders — and in Iowa and Nebraska passes into — the other, it may be supposed that common causes have influenced both together, perhaps more than Professor Whitney allows.

He thinks that the extreme fineness and depth of the usual prairie soil will account for the absence of trees; and Mr. Lesquereux equally explains it by the nature of the soil, in a different way. These and other excellent observers scout the idea that immemorial burnings, in autumn and spring, have had any effect. Professor Shaler, from his observations in the border land of Kentucky, thinks that they have, — that there are indications there of comparatively recent conversion of Oak-openings into prairie, and now — since the burnings are over — of the reconversion of prairie into woodland.

I am disposed, on general considerations, to think that the line of demarcation between our woods and our plains is not where it was drawn by Nature. Here, when no physical barrier is interposed between the ground that receives rain enough for forest and that which receives too little, there must be a debatable border, where comparatively slight causes will turn the scale either way. Difference in soil and difference in exposure will here tell decisively. And along this border, annual burnings — for the purpose of increasing and improving Buffalo-feed — practised for hundreds of years by our nomade predecessors, may have had a very marked effect. I suspect that the irregular border line may have in this way been rendered more irregular, and have been carried farther eastward wherever nature of soil or circumstances of exposure predisposed to it.

It does not follow that trees would reoccupy the land when the operation that destroyed them, or kept them down, ceased. The established turf or other occupation of the soil, and the

sweeping winds, might prevent that. The difficulty of re-foresting bleak New England coasts, which were originally well wooded, is well known. It is equally but probably not more difficult to establish forest on an Iowa prairie, with proper selection of trees.

The difference in the composition of the Atlantic and Pacific forests is not less marked than that of the climate and geographical configuration to which the two are respectively adapted.

With some very notable exceptions, the forests of the whole northern hemisphere in the temperate zone (those that we are concerned with) are mainly made up of the same or similar kinds. Not of the same species; for rarely do identical trees occur in any two or more widely separated regions. But all round the world in our zone, the woods contain Pines and Firs and Larches, Cypresses and Junipers, Oaks and Birches, Willows and Poplars, Maples and Ashes, and the like. Yet with all these family likenesses throughout, each region has some peculiar features, some trees by which the country may at once be distinguished.

Beginning by a comparison of our Pacific with our Atlantic forest, I need not take the time to enumerate the trees of the latter, as we all may be supposed to know them, and many of the genera will have to be mentioned in drawing the contrast to which I invite your attention. In this you will be impressed most of all, I think, with the fact that the greater part of our familiar trees are "conspicuous by their absence" from the Pacific forest.

For example, it has no Magnolias, no Tulip-tree, no Papaw, no Linden or Basswood, and is very poor in Maples; no Locust-tree — neither Flowering Locust nor Honey Locust — nor any Leguminous tree; no Cherry large enough for a timber-tree, like our wild Black Cherry; no Gum-trees (Nyssa nor Liquidambar), nor Sorrel-tree, nor Kalmia; no Persimmon, or Bumelia; not a Holly; only one Ash that may be called a timber-tree; no Catalpa, or Sassafras; not a single Elm, nor Hackberry; not a Mulberry, nor Planer-tree, nor Maclura; not a Hickory, nor a Beech, nor a true Chestnut,

nor a Hornbeam; barely one Birch-tree, and that only far north, where the differences are less striking. But as to Coniferous trees, the only missing type is our Bald Cypress, the so-called Cypress of our southern swamps, and that deficiency is made up by other things. But as to ordinary trees, if you ask what takes the place in Oregon and California of all these missing kinds, which are familiar on our side of the continent, I must answer, nothing, or nearly nothing. There is the Madroña (Arbutus) instead of our Kalmia (both really trees in some places); and there is the California Laurel instead of our southern Red Bay tree. Nor in any of the genera common to the two does the Pacific forest equal the Atlantic in species. It has not half as many Maples, nor Ashes, nor Poplars, nor Walnuts, nor Birches, and those it has are of smaller size and inferior quality: it has not half as many Oaks; and these and the Ashes are of so inferior economical value, that (as we are told) a passable wagon-wheel cannot be made of California wood, nor a really good one in Oregon.

This poverty of the western forest in species and types may be exhibited graphically, in a way which cannot fail to strike the eye more impressively than when we say that, whereas the Atlantic forest is composed of sixty-six genera and one hundred and fifty-five species, the Pacific forest has only thirty-one genera and seventy-eight species.[1] In the appended diagrams, the short side of the rectangle is proportional to the number of genera, the long side to the number of species.

Now the geographical areas of the two forests are not very different. From the Gulf of Mexico to the Gulf of St. Lawrence about twenty degrees of latitude intervene. From the southern end of California to the peninsula of Alaska there

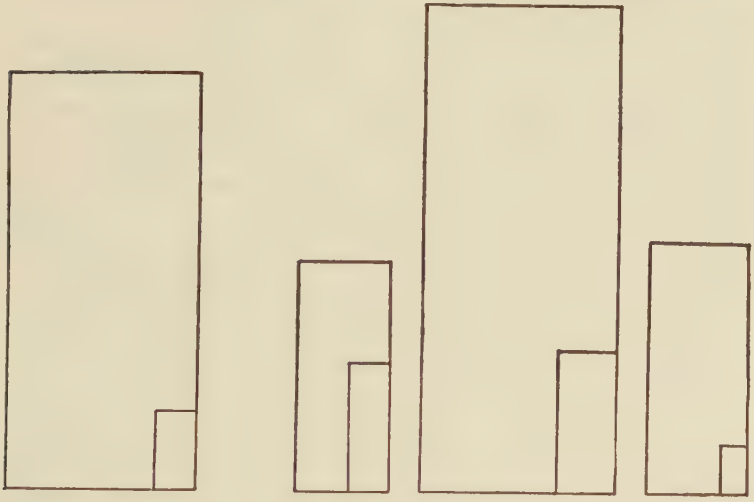
[1] We take in only timber trees, or such as attain in the most favorable localities to a size which gives them a clear title to the arboreous rank. The subtropical southern extremity and Keys of Florida are excluded. So also are one or two trees of the Arizonian region which may touch the evanescent southern borders of the Californian forest. In counting the coniferous genera, Pinus, Larix, Picea, Abies, and Tsuga are admitted to this rank, but Cupressus and Chamæcyparis are taken as one genus.

are twenty-eight degrees, and the forest on the coast runs some degrees north of this; the length may therefore make up for the comparative narrowness of the Pacific forest region. How can so meagre a forest make so imposing a show? Surely not by the greater number and size of its individuals, so far as deciduous (or more correctly non-coniferous) trees are concerned; for on the whole they are inferior to their eastern brethren in size if not in number of individuals. The reason is, that a large proportion of the genera and species are coniferous trees; and these, being evergreen (except the Larches), of aspiring port and eminently gregarious habit, usually dominate where they occur. While the east has almost three times as many genera and four times as many species of non-coniferous trees as the west, it has slightly fewer genera and almost one half fewer species of coniferous trees than the west. That is, the Atlantic coniferous forest is represented by eleven genera and twenty-five species; the Pacific by twelve genera and forty-four species. This relative preponderance may also be expressed by the diagrams, in which the smaller inclosed rectangles, drawn on the same scale, represent the coniferous portions of these forests.

Indeed, the Pacific forest is made up of conifers, with non-coniferous trees as occasional undergrowth or as scattered individuals, and conspicuous only in valleys or in the sparse tree-growth of plains, on which the Oaks at most reproduce the features of the "Oak-openings" here and there bordering the Mississippi prairie region. Perhaps the most striking contrast between the west and the east, along the latitude usually traversed, is that between the spiry evergreens which the traveler leaves when he quits California, and the familiar woods of various-hued round-headed trees which give him the feeling of home when he reaches the Mississippi. The Atlantic forest is particularly rich in these, and is not meagre in coniferous trees. All the glory of the Pacific forest is in its coniferous trees; its desperate poverty in other trees appears in the annexed diagram.

These diagrams could be made more instructive, and the relative richness of the forests round the world in our latitude

could be most simply exhibited, by the addition of two or three similar ones. Two would serve, one for Europe, the other for northeast Asia. A third would be the Himalay-Altaian region, geographically intermediate between the other two, as the Arizona-Rocky Mountain district is intermediate between our eastern and western. Both are here left out of view, partly for the same, partly for special reasons pertaining



1. Atlantic American Forest.
2. Pacific American Forest.
3. Japan-Mandchurian Forest.
4. European Forest.

to each, which I must not stop to explain. These four marked specimens will simply and clearly exhibit the general facts.

Keeping as nearly as possible to the same scale, we may count the indigenous forest trees of all Europe at thirty-three genera and eighty-five species; and those of the Japan-Mandchurian region, of very much smaller geographical area, at sixty-six genera and one hundred and sixty-eight species. I here include in it only Japan, eastern Mandchuria, and the adjacent borders of China. The known species of trees must be rather roughly determined; but the numbers here given are not exaggerated, and are much more likely to be sensibly

increased by further knowledge than are those of any of the other regions. Properly to estimate the surpassing richness of this Japan-Mandchurian forest, the comparative smallness of geographical area must come in as an important consideration.

To complete the view, let it be noted that the division of these forests into coniferous and non-coniferous is, for the

European, non-coniferous	26 genera,	68 species.
" coniferous	7 "	17 "
Total,	33 genera,	85 species.
Japan-Mandchurian, non-coniferous .	47 genera,	123 species.
" " coniferous . .	19 "	45 "
Total,	66 genera,	168 species.

In other words, a narrow region in eastern Asia contains twice as many genera and about twice as many species of indigenous trees as are possessed by all Europe; and as to coniferous trees, the former has more genera than the latter has species, and over twice and a half as many species.

The only question about the relation of these four forest-regions, as to their component species, which we can here pause to answer, is to what extent they contain trees of identical species. If we took the shrubs, there would be a small number, if the herbs a very considerable number, of species common to the two New World and to the two Old World areas respectively, at least to their northern portions, even after excluding arctic-alpine plants. The same may be said, in its degree, of the north European flora compared with the Atlantic North American, of the northeast Asiatic compared with the northern part of the Pacific North American, and also in a peculiar way (which I have formerly pointed out and shall have soon to mention) of the northeastern Asiatic flora in its relations to the Atlantic North American. But as to the forest trees there is very little community of species. Yet this is not absolutely wanting. The Red Cedar (*Juniperus Virginiana*) among coniferous trees, and *Populus tremuloides* among the deciduous, extend across the American

continent specifically unchanged, though hardly developed as forest trees on the Pacific side. There are probably, but not certainly, one or two instances on the northern verge of these two forests. There are as many in which eastern and western species are suggestively similar. The Hemlock Spruce of the northern Atlantic States and the Yew of Florida are extremely like corresponding trees of the Pacific forest; indeed the Yew-trees of all four regions may come to be regarded as forms of one polymorphous species. The White Birch of Europe and that of Canada and New England are in similar case; and so is the common Chestnut (in America confined to the Atlantic States), which on the other side of the world is also represented in Japan. A link in the other direction is seen in one Spruce-tree (called in Oregon Menzies Spruce) which inhabits northeast Asia, while a peculiar form of it represents the species in the Rocky Mountains.

But now other and more theoretical questions come to be asked, such as these: —

Why should our Pacific forest-region, which is rich and in some respects unique in coniferous, be so poor in deciduous trees?

Then the two Big Trees, Sequoias, as isolated in character as in location, — being found only in California, and having no near relatives anywhere, — how came California to have them?

Such relatives as the Sequoias have are also local, peculiar, and chiefly of one species to each genus. Only one of them is American, and that solely eastern, the Taxodium of our Atlantic States and the plateau of Mexico. The others are Japanese and Chinese.

Why should trees of six related genera, which will all thrive in Europe, be restricted naturally, one to the eastern side of the American continent, one genus to the western side and very locally, the rest to a small portion of the eastern border of Asia?

Why should coniferous trees most affect and preserve the greatest number of types in these parts of the world?

And why should the northeast Asian region have, in a

comparatively small area, not only most coniferous trees, but a notably larger number of trees altogether than any other part of the northern temperate zone? Why should its only and near rival be in the antipodes, namely, here in Atlantic North America? In other words, why should the Pacific and the European forests be so poor in comparison, and why the Pacific poorest of all in deciduous, yet rich in coniferous trees?

The first step toward an explanation of the superior richness in trees of these antipodal regions, is to note some striking similarities of the two, and especially the number of peculiar types which they divide between them. The ultimate conclusion may at length be ventured, that this richness is normal, and that what we really have to explain is the absence of so many forms from Europe on the one hand, from Oregon and California on the other. Let me recall to mind the list of kinds (*i. e.* genera) of trees which enrich our Atlantic forest but are wanting to that of the Pacific. Now almost all these recur, in more or less similar but not identical species, in Japan, north China, etc. Some of them are likewise European, but more are not so. Extending the comparison to shrubs and herbs, it more and more appears, that the forms and types which we count as peculiar to our Atlantic region, when we compare them, as we first naturally do, with Europe and with our West, have their close counterparts in Japan and north China; some in identical species (especially among the herbs), often in strikingly similar ones, not rarely as sole species of peculiar genera or in related generic types. I was a very young botanist when I began to notice this; and I have from time to time made lists of such instances. Evidences of this remarkable relationship have multiplied year after year, until what was long a wonder has come to be so common that I should now not be greatly surprised if a Sarracenia or a Dionæa, or their like, should turn up in eastern Asia. Very few of such isolated types remain without counterparts. It is as if Nature, when she had enough species of a genus to go round, dealt them fairly, one at least to each quarter of our zone; but when she had only

two of some peculiar kind, gave one to us and the other to Japan, Mandchuria, or the Himalayas; when she had only one, divided these between the two partners on the opposite sides of the table. The result, as to the trees, is seen in these four diagrams. As to number of species generally, it cannot be said that Europe and Pacific North America are at all in arrears. But as to trees, either the contrasted regions have been exceptionally favored, or these have been hardly dealt with. There is, as I have intimated, some reason to adopt the latter alternative.

We may take it for granted that the indigenous plants of any country, particularly the trees, have been selected by climate. Whatever other influences or circumstances have been brought to bear upon them, or the trees have brought to bear on each other, no tree could hold its place as a member of any forest or flora which is not adapted to endure even the extremes of the climate of the region or station. But the character of the climate will not explain the remarkable paucity of the trees which compose the indigenous European forest. That is proved by experiment, sufficiently prolonged in certain cases to justify the inference. Probably there is no tree of the northern temperate zone which will not flourish in some part of Europe. Great Britain alone can grow double or treble the number of trees that the Atlantic States can. In all the latter we can grow hardly one tree of the Pacific coast. England supports all of them, and all our Atlantic trees also, and likewise the Japanese and north Siberian species, which do thrive here remarkably in some part of the Atlantic coast, especially the cooler temperate ones. The poverty of the European sylva is attributable to the absence of our Atlantic American types, to its having no Magnolia, Liriodendron, Asimina, Negundo, no Æsculus, none of that rich assemblage of Leguminous trees represented by Locusts, Honey-Locusts, Gymnocladus, and Cladrastis (even its Cercis, which is hardly European, is like the Californian one mainly a shrub); no Nyssa, nor Liquidambar; no *Ericaceæ* rising to a tree; no Bumelia, Catalpa, Sassafras, Osage Orange, Hickory, or Walnut; and as to Conifers, no Hem-

lock Spruce, Arbor-Vitæ, Taxodium, or Torreya. As compared with northeastern Asia, Europe wants most of these same types, also the Ailantus, Gingko, and a goodly number of coniferous genera. I cannot point to any types tending to make up the deficiency, that is, to any not either in east North America or in northeast Asia, or in both. Cedrus, the true Cedar, which comes near to it, is only north African and Asian. I need not say that Europe has no Sequoia, and shares no special type with California.

Now the capital fact is, that many and perhaps almost all of these genera of trees were well represented in Europe throughout the later Tertiary times. It had not only the same generic types, but in some cases even the same species, or what must pass as such, in the lack of recognizable distinctions between fossil remains and living analogues. Probably the European Miocene forest was about as rich and various as is ours of the present day, and very like it. The Glacial period came and passed, and these types have not survived there, nor returned. Hence the comparative poverty of the existing European sylva, or, at least, the probable explanation of the absence of those kinds of trees which make the characteristic difference.

Why did these trees perish out of Europe but survive in America and Asia? Before we inquire how Europe lost them, it may be well to ask, how it got them. How came these American trees to be in Europe? And among the rest, how came Europe to have Sequoias, now represented only by our two Big Trees of California? It actually possessed two species and more; one so closely answering to the Redwood of the coast ranges, and another so very like the *Sequoia gigantea* of the Sierra Nevada, that, if such fossil twigs with leaves and cones had been exhumed in California instead of in Europe, it would confidently be affirmed that we had resurrected the veritable ancestors of our two giant trees. Indeed, so it may probably be. "Cœlum non animam mutant," etc., may be applicable even to such wide wanderings and such vast intervals of time. If the specific essence has not changed, and even if it has suffered some change, genealogical connection is to be inferred in all such cases.

That is, in these days it is taken for granted that individuals of the same species, or with a certain likeness throughout, had a single birthplace, and are descended from the same stock, no matter how widely separated they may have been either in space or time, or both. The contrary supposition may be made, and was seriously entertained by some not very long ago. It is even supposable that plants and animals originated where they now are, or where their remains are found. But this is not science; in other words it is not conformable to what we now know, and is an assertion that scientific explanation is not to be sought.

Furthermore, when species of the same genus are not found almost everywhere, they are usually grouped in one region, as are the Hickories in the Atlantic States, the Asters and Goldenrods in North America and prevailingly on the Atlantic side, the Heaths in western Europe and Africa. From this we are led to the inference that all species closely related to each other have had a common birthplace and origin. So that, when we find individuals of a species or of a group widely out of the range of their fellows, we wonder how they got there. When we find the same species all round the hemisphere, we ask how this dispersion came to pass.

Now, a very considerable number of species of herbs and shrubs, and a few trees, of the temperate zone are found all round the northern hemisphere; many others are found part way round, — some in Europe and eastern Asia; some in Europe and our Atlantic States; many, as I have said, in the Atlantic States and eastern Asia; fewer (which is curious) common to the Pacific States and eastern Asia, nearer though these countries be.

We may set it down as useless to try to account for this distribution by causes now in operation and opportunities now afforded, *i. e.*, for distribution across oceans by winds and currents, and birds. These means play their part in dispersion from place to place, by step after step, but not from continent to continent, except for few things and in a subordinate way.

Fortunately we are not obliged to have recourse to overstrained suppositions of what might possibly have occurred

now and then, in the lapse of time, by the chance conveyance of seeds across oceans, or even from one mountain to another. The plants of the top of the White Mountains and of Labrador are mainly the same; but we need not suppose that it is so because birds have carried seeds from the one to the other.

I take it that the true explanation of the whole problem comes from a just general view, and not through piecemeal suppositions of chances. And I am clear that it is to be found by looking to the north, to the state of things at the arctic zone, — first, as it now is, and then as it has been.

North of our forest-regions comes the zone unwooded from cold, the zone of arctic vegetation. In this, as a rule, the species are the same round the world; as exceptions, some are restricted to a part of the circle.

The polar projection of the earth down to the northern tropic, as here exhibited, shows to the eye — as our maps do not — how all the lands come together into one region, and how natural it may be for the same species, under homogeneous conditions, to spread over it. When we know, moreover, that sea and land have varied greatly since these species existed, we may well believe that any ocean-gaps, now in the way of equable distribution, may have been bridged over. There is now only one considerable gap.

What would happen if a cold period was to come on from the north, and was to carry very slowly the present arctic climate, or something like it, down far into the temperate zone? Why, just what has happened in the Glacial period, when the refrigeration somehow pushed all these plants before it down to southern Europe, to middle Asia, to the middle and southern part of the United States; and, at length receding, left some parts of them stranded on the Pyrenees, the Alps, the Apennines, the Caucasus, on our White and Rocky Mountains, or wherever they could escape the increasing warmth as well by ascending mountains as by receding northward at lower levels. Those that kept together at a low level, and made good their retreat, form the main body of present arctic vegetation. Those that took to the mountains had their line of retreat cut off, and hold their positions on the moun-

tain-tops under cover of the frigid climate due to elevation. The conditions of these on different continents or different mountains are similar, but not wholly alike. Some species proved better adapted to one, some to another, part of the world; where less adapted, or less adaptable, they have perished; where better adapted, they continue, — with or without some change; and hence the diversification of alpine plants, as well as the general likeness through all the northern hemisphere.

All this exactly applies to the temperate-zone vegetation, and to the trees that we are concerned with. The clew was seized when the fossil botany of the high arctic regions came to light; when it was demonstrated that in the times next preceding the Glacial period — in the latest Tertiary — from Spitzbergen and Iceland to Greenland and Kamtschatka, a climate like that we now enjoy prevailed, and forests like those of New England and Virginia, and of California, clothed the land. We infer the climate from the trees; and the trees give sure indications of the climate.

I had divined and published the explanation long before I knew of the fossil plants. These, since made known, render the inference sure, and give us a clear idea of just what the climate was. At the time we speak of, Greenland, Spitzbergen, and our arctic sea-shore had the climate of Pennsylvania and Virginia now. It would take too much time to enumerate the sorts of trees that have been identified by their leaves and fruits in the arctic later Tertiary deposits.

I can only say, at large, that the same species have been found all round the world; that the richest and most extensive finds are in Greenland; that they comprise most of the sorts which I have spoken of as American trees which once lived in Europe, — Magnolias, Sassafras, Hickories, Gum-trees, our identical Southern Cypress (for all we can see of difference), and especially Sequoias, not only the two which obviously answer to the two Big Trees now peculiar to California, but several others; that they equally comprise trees now peculiar to Japan and China, three kinds of Gingko-trees, for instance, one of them not evidently distinguishable

from the Japanese species which alone survives; that we have evidence, not merely of Pines and Maples, Poplars, Birches, Lindens, and whatever else characterize the temperate-zone forests of our era, but also of particular species of these, so like those of our own time and country, that we may fairly reckon them as the ancestors of several of ours. Long genealogies always deal more or less in conjecture; but we appear to be within the limits of scientific inference when we announce that our existing temperate trees came from the north, and within the bounds of nigh probability when we claim not a few of them as the originals of present species. Remains of the same plants have been found fossil in our temperate region, as well as in Europe.

Here, then, we have reached a fair answer to the question how the same or similar species of our trees came to be so dispersed over such widely separated continents. The lands all diverge from a polar centre, and their proximate portions — however different from their present configuration and extent, and however changed at different times — were once the home of those trees, where they flourished in a temperate climate. The cold period which followed, and which doubtless came on by very slow degrees during ages of time, must have long before its culmination brought down to our latitude, with the similar climate, the forest they possess now, or rather the ancestors of it. During this long (and we may believe first) occupancy of Europe and the United States, were deposited in pools and shallow waters the cast leaves, fruits, and occasionally the branches, which are imbedded in what are called Miocene Tertiary or later deposits, most abundant in Europe, from which the American character of the vegetation of the period is inferred. Geologists give the same name to these beds, in Greenland and southern Europe, because they contain the remains of identical or very similar species of plants; and they used to regard them as of the same age on account of this identity. But in fact this identity is good evidence that they cannot be synchronous. The beds in the lower latitudes must be later, and were forming when Greenland probably had very nearly the climate which it has now.

Wherefore the high, and not the low, latitudes must be assumed as the birthplace of our present flora;[1] and the present arctic vegetation is best regarded as a derivative of the temperate. This flora, which when circumpolar was as nearly homogeneous round the high latitudes as the arctic vegetation is now, when slowly translated into lower latitudes, would preserve its homogeneousness enough to account for the actual distribution of the same and similar species round the world, and for the original endowment of Europe with what we now call American types. It would also vary or be selected from by the increasing differentiation of climate in the divergent continents, and on their different sides, in a way which might well account for the present diversification. From an early period, the system of the winds, the great ocean currents (however they may have oscillated north and south), and the general proportions and features of the continents in our latitude (at least of the American continent) were much the same as now, so that species of plants, ever so little adapted or predisposed to cold winters and hot summers, would abide and be developed on the eastern side of continents, therefore in the Atlantic States and in Japan and Mandchuria; those with preference for milder winters would incline to the western sides; those disposed to tolerate dryness would tend to interiors, or to regions lacking summer rain. So that, if the same thousand species were thrust promiscuously into these several districts, and carried slowly onward in the way supposed, they would inevitably be sifted in such a manner that the survival of the fittest for each district might explain the present diversity.

Besides, there are re-siftings to take into the account. The Glacial period or refrigeration from the north, which at its inception forced the temperate flora into our latitude, at its culmination must have carried much or most of it quite beyond. To what extent displaced, and how far superseded by

[1] This takes for granted, after Nordenskjöld, that there was no preceding Glacial period, as neither palæontology nor the study of arctic sedimentary strata afford any evidence of it. Or if there was any, it was too remote in time to concern the present question.

the vegetation which in our day borders the ice, or by ice itself, it is difficult to form more than general conjectures — so different and conflicting are the views of geologists upon the Glacial period. But upon any, or almost any, of these views, it is safe to conclude that temperate vegetation, such as preceded the refrigeration and has now again succeeded it, was either thrust out of northern Europe and the northern Atlantic States, or was reduced to precarious existence and diminished forms. It also appears that, on our own continent at least, a milder climate than the present, and a considerable submergence of land, transiently supervened at the north, to which the vegetation must have sensibly responded by a northward movement, from which it afterward receded.

All these vicissitudes must have left their impress upon the actual vegetation, and particularly upon the trees. They furnish probable reason for the loss of American types sustained by Europe.

I conceive that three things have conspired to this loss. First, Europe, hardly extending south of latitude 40°, is all within the limits generally assigned to severe glacial action. Second, its mountains trend east and west, from the Pyrenees to the Carpathians and the Caucasus beyond, near its southern border; and they had glaciers of their own, which must have begun their operations, and poured down the northward flanks, while the plains were still covered with forest on the retreat from the great ice-wave coming from the north. Attacked both on front and rear, much of the forest must have perished then and there. Third, across the line of retreat of those which may have flanked the mountain-ranges, or were stationed south of them, stretched the Mediterranean, an impassable barrier. Some hardy trees may have eked out their existence on the northern shore of the Mediterranean and the Atlantic coast. But we doubt not, Taxodium and Sequoias, Magnolias and Liquidambars, and even Hickories and the like, were among the missing. Escape by the east, and rehabilitation from that quarter until a very late period, was apparently prevented by the prolongation of the Mediterranean to the Caspian, and thence to the Siberian ocean. If we

accept the supposition of Nordenskjöld, that anterior to the Glacial period, Europe was "bounded on the south by an ocean extending from the Atlantic over the present deserts of Sahara and Central Asia to the Pacific," all chance of these American types having escaped from or reëntered Europe from the south and east, is excluded. Europe may thus be conceived to have been for a time somewhat in the condition in which Greenland is now, and indeed to have been connected with Greenland in this or in earlier times. Such a junction, cutting off access of the Gulf Stream to the Polar sea, would, as some think, other things remaining as they are, almost of itself give glaciation to Europe. Greenland may be referred to, by way of comparison, as a country which having undergone extreme glaciation, bears the marks of it in the extreme poverty of its flora, and in the absence of the plants to which its southern portion, extending six degrees below the arctic circle, might be entitled. It ought to have trees, and might support them. But since destruction by glaciation, no way has been open for their return. Europe fared much better, but suffered in its degree in a similar way.

Turning for a moment to the American continent for a contrast, we find the land unbroken and open down to the tropic, and the mountains running north and south. The trees, when touched on the north by the on-coming refrigeration, had only to move their southern border southward, along an open way, as far as the exigency required; and there was no impediment to their due return. Then the more southern latitude of the United States gave great advantage over Europe. On the Atlantic border, proper glaciation was felt only in the northern part, down to about latitude 40°. In the interior of the country, owing doubtless to greater dryness and summer heat, the limit receded greatly northward in the Mississippi Valley, and gave only local glaciers to the Rocky Mountains; and no volcanic outbreaks or violent changes of any kind have here occurred since the types of our present vegetation came to the land. So our lines have been cast in pleasant places, and the goodly heritage of forest trees is one of the consequences.

The still greater richness of northeast Asia in arboreal vegetation may find explanation in the prevalence of particularly favorable conditions, both ante-glacial and recent. The trees of the Miocene circumpolar forest appear to have found there a secure home; and the Japanese islands, to which most of these trees belong, must be remarkably adapted to them. The situation of these islands — analogous to that of Great Britain, but with the advantage of lower latitude and greater sunshine, — their ample extent north and south, their diversified configuration, their proximity to the great Pacific gulf-stream, by which a vast body of warm water sweeps along their accentuated shores, and the comparatively equable diffusion of rain throughout the year, all probably conspire to the preservation and development of an originally ample inheritance.

The case of the Pacific forest is remarkable and paradoxical. It is, as we know, the sole refuge of the most characteristic and widespread type of Miocene *Coniferæ*, the Sequoias; it is rich in coniferous types beyond any country except Japan; in its gold-bearing gravels are indications that it possessed, seemingly down to the very beginning of the Glacial period, Magnolias and Beeches, a true Chestnut, Liquidambar, Elms, and other trees now wholly wanting to that side of the continent, though common both to Japan and to Atlantic North America.[1] Any attempted explanation of this extreme paucity of the usually major constituents of forests, along with a great development of the minor, or coniferous, element, would take us quite too far, and would bring us to mere conjectures.

Much may be attributed to late glaciation;[2] something to the tremendous outpours of lava which, immediately before

[1] See, especially, "Report on the Fossil Plants of the auriferous gravel deposits of the Sierra Nevada," by L. Lesquereux; "Mem. Mus. Comp. Zoölogy," vi. No. 2. — Determinations of fossil leaves, etc., such as these, may be relied on to this extent by the general botanist, however wary of specific and many generic identifications. These must be mainly left to the expert in fossil botany.

[2] Sir Joseph Hooker, in an important lecture delivered to the Royal Institution of Great Britain, April 12, insists much on this.

the period of refrigeration, deeply covered a very large part of the forest-area; much to the narrowness of the forest-belt, to the want of summer rain, and to the most unequal and precarious distribution of that of winter.

Upon all these topics questions present themselves which we are not prepared to discuss. I have done all that I could hope to do in one lecture if I have distinctly shown that the races of trees, like the races of men, have come down to us through a prehistoric (or pre-natural historic) period; and that the explanation of the present condition is to be sought in the past, and traced in vestiges and remains and survivals; that for the vegetable kingdom also there is a veritable Archæology.

THE PERTINACITY AND PREDOMINANCE OF WEEDS.[1]

A WEED is defined by the dictionaries to be "Any useless or troublesome plant." "Every plant which grows in a field other than that of which the seed has been (intentionally) sown by the husbandman is a weed," says the "Penny Cyclopædia," as cited in Worcester's Dictionary. The "Treasury of Botany" defines it as "Any plant which obtrusively occupies cultivated or dressed ground, to the exclusion or injury of some particular crop intended to be grown. Thus, even the most useful plants may become weeds if they appear out of their proper place. The term is sometimes applied to any insignificant-looking or unprofitable plants which grow profusely in a state of nature; also to any noxious or useless plant." We may for present purposes consider weeds to be plants which tend to take prevalent possession of soil used for man's purposes, irrespective of his will; and, in accordance with usage, we may restrict the term to herbs. This excludes predominant indigenous plants occupying ground in a state of nature. Such become weeds when they conspicuously intrude into cultivated fields, meadows, pastures, or the ground around dwellings. Many are unattractive, but not a few are ornamental; many are injurious, but some are truly useful. White Clover is an instance of the latter. Bur Clover (*Medicago denticulata*) is in California very valuable as food for cattle and sheep, and very injurious by the damage which the burs cause to wool. In the United States, and perhaps in most parts of the world, a large majority of the weeds are introduced plants, brought into the country directly or indirectly by man. Some such as Dandelion, Yarrow, and probably the common Plantain and the common Purslane, are importations as weeds, although the species naturally occupy some part of the country.

[1] American Journal of Science and Arts, 3 ser., xviii. 161. (1879.)

Why weeds are so pertinaceous and aggressive, is too large and loose a question: for any herb whatever when successfully aggressive becomes a weed; and the reasons of predominance may be almost as diverse as the weeds themselves. But we may inquire, whether weeds have any common characteristic which may give them advantage, and why the greater part of the weeds of the United States, and probably of similar temperate countries, should be foreigners.

As to the second question, this is strikingly the case throughout the Atlantic side of temperate North America, in which the weeds have mainly come from Europe; but it is not so, or hardly so, west of the Mississippi in the region of prairies and plains. So that the answer we are accustomed to give must be to a great extent the true one, namely, that, as the district here in which weeds from the Old World prevail was naturally forest-clad, there were few of its native herbs which, if they could bear the exposure at all, were capable of competition on cleared land with emigrants from the Old World. It may be said that these same European weeds, here prepotent, had survived and adapted themselves to the change from forest to cleared land in Europe, and therefore our forest-bred herbs might have done the same thing here. But in the first place the change must have been far more sudden here than in Europe; and in the next place, we suppose that most of the herbs in question never were indigenous to the originally forest-covered regions of the Old World; but rather, as western and northern Europe became agricultural and pastoral, these plants came with the husbandmen and the flocks, or followed them, from the woodless or sparsely wooded regions farther east where they originated. This, however, will not hold for some of them, such as Dandelion, Yarrow, and Ox-eye Daisy. It may be said that our weeds might have come to a considerable extent from the bordering more open districts on the west and south. But there was little opportunity until recently, as the settlement of the country began on the eastern border; yet a certain number of our weeds appear to have been thus derived: for instance, *Mollugo verticillata*, *Erigeron Canadense*,

Xanthium, *Ambrosia artemisiæfolia*, *Verbena hastata*, *V. urticifolia*, etc., *Veronica peregrina*, *Solanum Carolinense*, various species of Amarantus and Euphorbia, *Panicum capillare*, etc. Of late, and in consequence of increased communication with the Mississippi region and beyond — especially by railroads — other plants are coming into the eastern States as weeds, step by step, by somewhat rapid strides; such as *Dysodia chrysanthemoides*, *Matricaria discoidea*, and *Artemisia biennis*. Fifty years ago *Rudbeckia hirta*, which flourished from the Alleghanies westward, was unknown farther east. Now since twenty years, it is an abundant and conspicuous weed in grass-fields throughout the eastern States, having been accidentally desimated with Red Clover seed from the western States.

There are also native American weeds, doubtless indigenous to the region, such as *Asclepias Cornuti*, *Antennaria margaritacea*, and *A. plantaginifolia*, and in enriched soils *Phytolacca decandra*, which have apparently become strongly aggressive under changed conditions. These are some of the instances which may show that predominance is not in consequence of change of country and introduction to new soil.

In many cases it is easy to explain why a plant, once introduced, should take a strong and persistent hold and spread rapidly. In others we discern nothing in the plant itself which should give it advantage. *Lespedeza striata* is a small and insignificant annual, with no obvious provision for dissemination. It is a native of China and Japan. In some unexplained way it reached Alabama and Georgia, and was first noticed about thirty-five years ago; it has spread rapidly since, especially over old fields and along roadsides, and it is now very abundant up to Virginia and Tennessee, throughout the middle and upper districts, reaching even to the summits of the mountains of moderate elevation. In the absence of better food it is greedily eaten by cattle and sheep. The voiding by them of undigested seeds must be the means of dissemination; but one cannot well understand why it should spread so widely and rapidly, and take such complete possession of the ground. It is one of the few weeds which are accounted a blessing.

Professor Claypole, of Antioch College, Ohio, has recently contributed to the "Third Report of the Montreal Horticultural Society" (1877–8) an interesting essay, "On the Migration of Plants from Europe to America, with an Attempt to explain Certain Phenomena connected therewith." The phenomena which he would explain are the abundant migration of numerous weeds from Europe to the shore of North America, while others fail to come, and the general failure of North American weeds to invade Europe. We have offered a fairly good explanation of the first. And Professor Claypole goes far toward explaining the second when he notes that seed is (or formerly was) mainly brought from the Old World to the New, and the same may be said of cattle and other emigration; that the cooler and shorter summer of the north of Europe renders the ripening of some seed precarious, etc. He does not mention the fact that American plants by chance reaching Europe have to compete with a vegetable world in comparatively stable equilibrium of its species, while European weeds coming — or which formerly came — to the United States found the course of nature disturbed by man and new-made fields for which they could compete with advantage. But this ingenious hypothesis is that weeds have a peculiarly "plastic nature, one capable of being moulded by and to the new surroundings," by which the plant "ere long adapts itself, if the change is not too great or sudden, to its new situation, takes out a new lease of life, and continues in the strictest sense a weed; that the plants of the European flora possess more of this plasticity, are less unyielding in their constitution, can adapt themselves more readily to new surroundings," and that it is "the lack of this plasticity in the American flora which incapacitates it from securing a foothold and obtaining a living in the different conditions of the New World;" that although "in the Miocene era the European and American floras were very much alike," yet "since that era the European flora has been vastly altered, while the American flora still retains a Miocene aspect, and is therefore the elder of the two; that this long persistence of type in the American flora may have induced, by habit, a rigidity or

indisposition to change;" that "the European is thus better able to adapt itself to the strange climate and conditions — that is to emigrate — than the American: and thus, being more plastic or adaptable, it succeeds in the New World, while the less adaptable American flora fails in the Old World."

So far as we know, the greater plasticity of European as compared with American plants is purely hypothetical. "More plastic" would mean of greater variability, which, if true, might be determined by observation. Because Europe once had more species or types in common with North America than it now has, it does not seem to follow that the former has "a younger plant-life," or that its existing plants are more recent than those of the American flora. And as already intimated, so refined an hypothesis is hardly necessary for the probable explanation of the predominance of Old World weeds in the Atlantic United States.

Mr. Henslow, in his remarkable memoir, "On the Self-Fertilization of Plants," derives from different but equally theoretical premises an opposite conclusion, — namely, that weeds or intrusive and dominant plants in general, and of great emigrating capabilities, have "a longer ancestral life-history than their less aggressive relatives." He also maintains that weeds, and plants best fitted for domination in the manner of weeds, possess a common characteristic to which this dominance may be attributed, namely, that they are in general self-fertilized plants. A rapid generalizer might find confirmation of this in the converse, which is obviously true, that plants with blossoms very specially adapted for cross-fertilization by particular insects, and therefore dependent on such special aid, are comparatively local and unaggressive; yet some of these are widely distributed. It will also be understood that self-fertilization may give advantage to an intruding plant at the outset by enabling an exceptionally well-fitted individual to initiate a favored race. And self-fertilization, with its sureness, would always be most advantageous unless cross-fertilization brings some compensatory advantage greater on the whole than that of immediate sureness to fertilize.

But the test of the theory is, whether weeds and emigrating herbs in general are more self-fertilizing or less subject to cross-fertilization than the majority of related plants, and whether many or any of them are actually self-fertilized through a succession of generations. It seemed to us that, in a limited way, the weeds which Europe has given to North America might answer this question. To keep within bounds and to have a case with all the data unquestionable, we will collate the weeds of European parentage which evince a dominating character in the United States east of the Mississippi, referring for the purpose to the "Manual of Botany of the Northern United States" and Chapman's "Flora of the Southern States." The latter, however, adds not a single weed from Europe of any predominance. We include only those which have taken a strong hold and become prominent either by their general diffusion over the area or by taking marked possession of certain districts. For examples of the latter take *Echium vulgare* in Virginia, *Ranunculus bulbosus* and *Leontodon autumnale* in eastern New England, and *Genista tinctoria*, which covers certain tracts in the eastern part of Massachusetts, although nearly unknown elsewhere. We must include several species which as weeds came from Europe, although they are probably, some of them undoubtedly, indigenous to some part of the United States.

The following are the herbaceous plants naturalized from Europe and of an aggressive character in the Atlantic United States. Herbs of recent introduction, and those of however ancient naturalization which have not either spread widely or increased greatly over a considerable district, are omitted.

The eighteen species in italic type, nearly half of them Grasses, are probably indigenous to some portions of North America. In some cases the introduced and the indigenous plants have come into contact.

Ranunculus bulbosus.
Ranunculus acris.
Nasturtium officinale.
Sisymbium officinale.
Brassica Sinipistrum.
Raphanus Raphanistrum.
Capsella Bursa-pastoris.
Reseda Luteola.
Saponaria officinalis.
Silene inflata.
Lychnis Githago.
Stellaria media.
Portulaca oleracea.
Malva rotundifolia.

Genista tinctoria.
Trifolium arvense.
Trifolium agrarium.
Trifolium repens.
Daucus Carota.
Pastinaca sativa.
Conium maculatum.
Tussilago Farfara.
Inula Helenium
Gnaphalium uliginosum.
Anthemis Cotula.
Achillœa Millefolium.
Tanacetum vulgare.
Leucanthemum vulgare.
Cirsium arvense.
Cirsium lanceolatum.
Lappa officinalis.
Cichorium Intybus.
Leontodon autumnale.
Taraxacum Dens-leonis.
Plantago major.
Plantago lanceolata.
Anagallis arvensis.
Verbascum Thapsus.
Verbascum Blattaria.
Linaria vulgaris.
Mentha viridis.
Mentha piperita.
Calamintha Nepeta.
Calamintha Clinopodium.
Nepeta Cataria.
Nepeta Glechoma.
Marrubium vulgare.
Galeopsis Tetrahit.
Leonurus Cardiaca.
Lamium amplexicaule.
Echium vulgare.
Symphytum officinale.
Echinospermum Lappula.
Cynoglossum officinale.
Solanum nigrum.
Chenopodium album.
Chenopodium hybridum.
Chenopodium Botrys.
Polygonum aviculare.
Polygonum Convolvulus.
Rumex crispus.
Rumex sanguineus.
Rumex Acetosella.
Allium vineale.
Alopecurus pratensis.
Phleum pratense.
Agrostis vulgaris.
Agrostis alba.
Dactylis glomerata.
Poa annua.
Poa compressa.
Poa pratensis.
Poa trivialis.
Eragrostis pœoides.
Festuca ovina.
Festuca pratensis.
Bromus secalinus.
Lolium perenne.
Triticum repens.
Triticum caninum.
Anthoxanthum odoratum.
Panicum glabrum.
Panicum sanguinale.
Panicum Crus-galli.
Setaria glauca.
Setaria virdis.

The plants of this list, regarded as weeds, are of very various character; and several of them, such as White Clover and most of the Grasses, where most dominant, do not fall under the ordinary definition of weeds at all, but under that of plants useful to the farmer. Some, like Purslane, are only garden weeds; some belong to pastures and meadows; others affect roadsides. The fewness of European corn-weeds is remarkable. Ches and Corn-cockle (*Lychnis Githago*) are the only ones on the list. Corn Poppy, Bluebottle and Knapweed (*Centaurea Cyanus* and *C. nigra*) and Larkspur are conspicuously wanting; but the last two are not wholly unknown in some parts of the country.

But the only question before us is, whether these plants introduced from Europe are or are not self-fertilized, or more habitually so than others, so that this may be accounted an

element of their predominance. Apparently this question must be answered in the negative. The question is not whether they are self-fertilizable. The great majority of plants are so, even of those specially adapted for intercrossing. The plants of this list appear to belong to the *juste milieu*. Only one (*Rumex Acetosella*) is completely dicœcious; a few are incompletely dicœcious or polygamous; the two species of Plantago are dichogamous to the extent of necessary dioicism or monoicism; a large number of the corolline species are either proterandrous or proterogynous, including two or three anemophilous species; and all the Grasses (which form the last quarter of the list) are anemophilous and more or less dichogamous, and therefore not rarely cross-fertilized. Of those which are not anemophilous we notice none which are not habitually visited by insects (except perhaps *Gnaphalium uliginosum*), and which therefore are almost as likely to be cross-fertilized as close-fertilized; while in not a few (such as the *Compositæ* generally and most of the other *Gamopetalæ*) the arrangements which favor intercrossing are explicit. There is no cleistogamous and therefore necessarily self-fertilized plant in the list, except *Lamiuum amplexicaule*, which also cross-fertilizes freely.

In California the prevalent weeds are largely different from those of the Atlantic States, and, as would be expected, are mostly of indigenous species or immigrants from South America; yet the common weeds of the Old World, especially of southern Europe, are coming in. The well-established and aggressive ones, such as *Brassica nigra*, *Silene Gallica*, *Erodium cicutarium*, *Malva borealis*, *Medicago denticulata*, *Marrubium vulgare*, and *Avena sterilis*, were perhaps introduced by way of western South America. They are mostly plants capable of self-fertilization, but also with adaptations (of dichogamy and otherwise) which must secure occasional crossing.

We cannot avoid the conclusion that self-fertilization is neither the cause nor a perceptible cause of the prepotency of the European plants which are weeds in North America.

A cursory examination brings us to a similar conclusion as respects the indigenous weeds of the Atlantic States, those

herbs which under new conditions, have propagated most abundantly and rapidly, and competed most successfully in the strife for the possession of fields that have taken the place of forest. The most aggressive of these in the Northern States are *Epilobium spicatum* in the newest clearings, which is dichogamous (proterandrous) to a degree which practically forbids self-fertilization; and in older fields, *Asclepias Cornuti*, which is specially adapted for cross-fertilization by flying insects; *Antennaria plantaginifolia* and *A. margaritacea*, which are diœcious; and next to these perhaps the two wild Strawberries, then *Erigeron annuum* and *E. strigosum*, with certain Asters and Goldenrods, all insect-visited and dichogamous, and *Verbena hastata*, *V. urticifolia*, etc., the frequent natural hybridization of which testifies to habitual intercrossing.

Those who suppose that only conspicuous or odorous flowers are visited by flying insects should see how bees throng the small, greenish, and to us odorless blossoms of Ampelopsis or Virginia Creeper and of its Japanese relative.

THE FLORA OF NORTH AMERICA.[1]

In the remarks which I have to offer to this Section, you will understand the word "Flora" to be written with a capital initial. I am to speak of the attempts made in my own day, and still making, to provide our botanists with a compendious systematic account of the phænogamous vegetation of the whole country which the American Association calls its own.

I shall make no effort to avoid the personal turn which my narrative is likely to take. In fact, it will be seen that I have partly a personal object in drawing up this statement.

Only two Floras of North America have ever been published as completed works, that of Michaux and that of Pursh. A third was begun (by Dr. Torrey, assisted by a young man who is no longer young), by the publication in the summer of 1838 of a first fasciculus; the first volume of 700 pages was issued two years afterward; and 500 pages of the second volume appeared in 1841 and in the early part of 1843. The time for continuing it in the original form has long ago passed by. Its completion in the form in which I have undertaken it anew is precarious. Precarious in the original sense of the word, for it is certainly to be prayed for: precarious, too, in the current sense of the word as being uncertain; yet not so, according to an accepted definition, namely: "uncertain, because depending upon the will of another;" for it is not our will but our power that is in question; and it is only by the combined powers and efforts of all of us interested in Botany that the desired end can possibly be attained.

[1] A paper read to the Botanists at the meeting of the American Association for the Advancement of Science, at Montreal, August 25, 1882. American Journal of Science and Arts, 3 ser., xxiv. 321.

It were well to consider for a moment how and why it is that a task which has twice been — it would seem — easily accomplished has now become so difficult.

The earliest North American Flora, that of the elder Michaux, appeared in the year 1803. It was based entirely upon Michaux's own collections and observations, does not contain any plants which he had not himself gathered or seen, is not, therefore, an exhaustive summary of the botany of the country as then known, and so was the more readily prepared. Michaux came to this country in 1785, returned to France in 1796, left it again in Baudin's expedition to Australia in 1800, and died of fever in Madagascar in 1802. The Flora purports to be edited by his son, F. A. Michaux, who signed the classical Latin preface. The finish of the specific characters, and especially the capital detailed characters of the new genera, reveal the hand of a master; and tradition has it that these were drawn up by Louis Claude Richard, who was probably the ablest botanist of his time. This tradition is confirmed by the fact that Richard's herbarium (bequeathed to his son, and now belonging to Count Franqueville) contains an almost complete set of the plants described, and I found that the specimens of Michaux supplied to Willdenow's herbarium at Berlin were ticketed and sent by Richard. Not only the younger Richard but Kunth also habitually cited the new genera of the work as of Richard, and some others have followed this example. Singularly enough, however, there is no reference whatever to Richard in any part of the Flora, nor in the elaborate preface. The most venerable botanist now living told me that there was a tradition at Paris that Richard performed a similar work for Persoon's "Synopsis Plantarum," and that he declined all mention of his name in the Synopsis and in the Flora, because the two works — contrary to the French school — were arranged upon the Linnæan Artificial System. He had his way, and the tradition may be preserved in history; but his name cannot be cited for the genera Elytraria, Micranthemum, Elodea, Stipulicida, Dichromena, Oryzopsis, Erianthus, and the like. For, by the record these are of Michaux, "Flora Boreali-Americana," and not of Richard.

Michaux's explorations extended from Hudson's Bay, which he reached by way of the Saguenay, to Florida, as far, at least as St. Augustine and Pensacola; he was the first botanical explorer of the higher Alleghany Mountains, and, crossing these mountains in Tennessee, he reached the Mississippi in Illinois, and was as far south as Natchez. His original itinerary, which I once consulted, is preserved by the American Philosophical Society, at Philadelphia, to which it was presented by his son. It ought to be printed. That little journal shows that it was not Michaux's fault that the first Flora of North America was restricted to the district east of the Mississippi River. He had a scheme for crossing the continent to the Pacific. He warmly solicited the government at Washington to undertake such an exploration, and offered to accompany it as naturalist. This may have been the germ or the fertilizing idea of the expedition of Lewis and Clark, which was sent out a few years afterward by Jefferson, to whom, if I rightly remember, Michaux addressed his enterprising proposal.

Leaving out the Cryptogams of lower rank than the Ferns, we find that the Flora of Michaux, published at the beginning of this century, say eighty years ago, contains 1530 species, in 528 genera. No very formidable number; as to species (speaking without a count) little over half as many as are described in my "Manual of the Botany of the Northern States," which covers less than half of Michaux's area.

Eleven years afterward, namely, in the year 1814 (the preface is dated December, 1813), appeared the second Flora of North America, namely, the "Flora Americæ Septentrionalis," by Frederick Pursh. This was not confined to the author's own collections, but aimed at completeness, or to give "a systematic arrangement and description of the plants of North America, containing, besides what have been described by preceding authors, many new and rare species, collected during twelve years' travels and residence in that country."

It appears that Pursh was born at Tobolsk, in Siberia, of what parentage we do not know. He himself tells us, in his preface, that he was educated in Dresden, and that he came

to this country — to Baltimore and Philadelphia — at the close of the last century, when he must have been only twenty-five years old. He was able to make the acquaintance not only of Muhlenberg, who survived until 1815, and of William Bartram, who died in 1823, but also of the veteran, Humphrey Marshall, who died in 1805. His early and principal patron was Dr. Benjamin Smith Barton, who supplied the means for most of the travels which he was able to undertake, and who, as Pursh states, "for some time previous had been collecting materials for an American Flora." Pursh's personal explorations were not extensive. From 1802 till 1805 he was in charge of the gardens of William Hamilton, near Philadelphia. In the spring of the latter year, as he says, he "set out for the mountains and western territories of the southern States, beginning at Maryland and extending to the Carolinas (in which tract the interesting high mountains of Virginia and Carolina took my particular attention), returning late in the autumn through the lower countries along the sea-coast to Philadelphia." But, in tracing his steps by his collections[1] and by other indications, it appears that he did not reach the western borders of Virginia nor cross its southern boundary into the mountains of North Carolina. The Peaks of Otter and Salt-pond Mountain (now Mountain Lake) were the highest elevations which he attained. Pursh's preface continues: "The following season, 1806, I went in like manner over the northern States, beginning with the mountains of Pennsylvania and extending to those of New Hampshire (in which tract I traversed the extensive and highly interesting country of the Lesser and Great Lakes), and returning as before by the sea-coast." The diary of this expedition, found among Dr. Barton's papers and collection in possession of the American Philosophical Society, has recently been printed by the late Mr. Thomas Potts James. It shows that the journey was not as extended or as thorough as would be supposed; that it was from Philadelphia directly north to the Pokono Mountains, thence to Onondaga, and to Oswego, — the only point on the Great Lakes reached, — thence back to Utica,

[1] In herb. Barton and herb. Lambert.

down the Mohawk Valley to Saratoga, and north to the upper part of Lake Champlain and to the lesser Green Mountains in the vicinity of Rutland, but not beyond. Discouraged by the lateness of the season, and disheartened — as he had all along been — by the failure and insufficiency of remittances from his patron, Pursh turned back from Rutland on the 22d of September, reached New York on the 1st of October, and Philadelphia on the 5th. The next year (1807) Pursh took charge of the Botanic Garden which Dr. Hosack had formed at New York and afterward sold to the State, which soon made it over to Columbia College.[1] In 1810, he made a voyage to the West Indies for the recovery of his health. Returning in the autumn of 1811, he landed at Wiscasset, in Maine, "had an opportunity of visiting Professor Peck of Cambridge College, near Boston," and of seeing the alpine plants which Peck had collected on the White Mountains.[2] At the end of the latter year or early in 1812 he went to England with his collections and notes; and at the close of 1813, under the auspices of Lambert, he produced his Flora, consulting, the while, the herbaria of Clayton, Pallas, Plukenet, Catesby, Morison, Sherard, Walter, and that of Banks. Evidently such consultations and the whole study must have

[1] Expecting, no doubt, that it would be kept up. But the Elgin Botanic Garden was soon discontinued. It occupied the block of ground now covered by the buildings of the College, and the surrounding tract — now so valuable — from which the college derives an ample revenue. *Noblesse oblige*, and it may be expected that the College, so enriched, will, before long, provide itself with a botanical professorship, and see to the careful preservation and maintenance of the precious Torrey Herbarium, which it possesses along with other subsidiary herbaria.

[2] It is at Wiscasset, therefore, that Pursh's "*Plantago cucullata*, Lam. . . . in wet rocky situations, Canada and Province of Maine," is to be sought. Mr. Pringle has recently found the related *P. Cornuti* (which may be the plant meant), in Lower Canada, not far from the other side of Maine.

It must have been in Professor Peck's herbarium (no longer extant), that Pursh saw what he took to be *Alchemilla alpina*, which he marks "*v. s.*" and refers to from memory only, probably mistakenly. For it has not since been detected either in Vermont or New Hampshire, or anywhere in North America ; and Pursh's Journal makes it certain that he did not reach any alpine region in the Green Mountains.

been rapid. The despatch is wonderful. One can hardly understand the ground of the statement made by Lambert to my former colleague, Dr. Torrey, that he was obliged to shut Pursh up in his house in order to keep him at his work.

I know not how Pursh was occupied for the next four years, nor when he came to Canada. But he died here at Montreal, in 1820, at the early age of forty-six. More is probably known of him here. If I rightly remember, his grave has been identified, and a stone placed upon it inscribed to his memory.[1] A tradition has come down to us — and it is partly confirmed by a statement which Lambert used to make, in reference to the vast quantity of beer he had to furnish during the preparation of the Flora — that, in his latter days, our predecessor was given to drink, and that his days were thereby shortened.

In Pursh's Flora we begin to have plants from the Great Plains, the Rocky Mountains, and the Pacific coast, although the collections were very scanty. The most important one which fell into Pursh's hands was that of about 150 specimens, gathered by Lewis and Clark on their homeward journey from the mouth of the Columbia River. A larger collection, more leisurely made on the outward journey, was lost. Menzies in Vancouver's voyage had botanized on the Pacific coast, both in California and much farther north. Some of his plants were seen by Pursh in the Banksian herbarium, and taken up. I may here say that in the winter of 1838–39 I had the pleasure of making the acquaintance of the venerable Menzies, then about ninety-five years old.

In the Supplement, Pursh was able to include a considerable

[1] In the Canadian Naturalist, Principal Dawson gives a brief account of the transference of the remains of Pursh from a grave-yard below Montreal, in which they were interred, to the beautiful Mount Royal Cemetery, where they rest in a lot purchased for the purpose and under a neat and durable granite monument, provided by the naturalists of Montreal and their friends. A small company of botanists, led by Dr. Dawson, visited the spot shortly after the reading of this paper. We learned that Pursh had botanized largely in Canada, in view of a Canadian Flora, and that his collections were consumed by a fire at Quebec shortly before his death, to his extreme discouragement.

number of species, collected by Bradbury on the upper Missouri, in what was then called Upper Louisiana, — much to the discontent of Nuttall, who was in that region at the same time, and who, indeed, partly and imperfectly anticipated Pursh in certain cases, through the publication by the Frasers of a catalogue of some of the plants collected by Nuttall.

To come now to the extent of Pursh's Flora, published nearly sixty-nine years ago. It contains 740 genera of Phænogamous and Filicoid plants, and 3076 species, — just about double the number of species contained in Michaux's Flora of eleven years before.

I must omit all mention of more restricted works, even such as Nuttall's "Genera of North American Plants," which came only four years after Pursh's Flora; also the "Flora Boreali-Americana" of Sir William Hooker, which began in 1829, but was restricted to British America. I cannot say how early it was that my revered master, Dr. Torrey, conceived the idea of the Flora which he at length undertook. But he once told me that he had invited Nuttall to join him in the production of such a work, and that Nuttall declined. This must have been as early as the year 1832, that is, half a century ago. My correspondence with Dr. Torrey began in the summer of 1830, when I was a young medical student, and three or four years afterward I joined him at New York and became, for a short time, his assistant, for all the rest of his life, his botanical colleague. He was very much occupied with his duties as professor, chiefly of chemistry; he had not yet abandoned the idea of completing his "Flora of the Northern and Middle States," the first volume of which was finished in 1824, while yet free from all professional cares. Although working in the direction of the larger undertaking, the "Flora of North America" did not assume definite shape before the year 1835. I believe that some of the first actually-prepared manuscript for it was written by myself in that or the following year. I was then and for a long time expecting to accompany the South Pacific Exploring Expedition, as originally organized under the command of Commodore Ap Catesby Jones, but which was subject to long delay and many vicissi-

tudes; during which, having plentiful leisure, I tried my 'prentice hand upon some of the earlier natural orders. Before the expedition, as modified, was ready to sail, under the command of Captain Wilkes, I had accepted Dr. Torrey's proposal that I should be his associate in the work upon which I had made a small beginning as a volunteer. Two parts, or half, of the first volume (360 pages), of this Flora, were printed and issued in July and October, 1838.

It was thought at first, in all simplicity, that the whole task could be done at something like this rate. But, apart from other considerations, it soon became clear that there had been no proper identification of the foundation-species of the earlier botanists, from Linnæus downward; and that our Flora could not go on satisfactorily without this. Dr. Torrey had, indeed, some years before, made a hasty visit to Hooker at Glasgow, to London, and to Paris; but the taking of a few notes upon some particular plants in the herbaria of Hooker, Lambert, and Michaux, and the acquisition, from Hooker, of a good set of the Arctic plants of the British explorers, was about all that had been done. I proposed to attempt something more; so, taking advantage of a favorable opportunity, I sailed for Liverpool in November, 1838, and devoted a good part of the ensuing year to the examination of the principal herbaria, which I need not here specify, in Scotland (where the important one of Sir William Hooker still remained), England, France, Switzerland, and Germany, namely those which contained the specimens upon which most of the then-published North American species had been directly or indirectly founded, especially those of Linnæus and Gronovius, of Walter, of Aiton's "Hortus Kewensis," Michaux, Wildenow, Pursh, and the later ones of De Candolle and Hooker.

After my return the work made good progress; the remaining half of the first volume was brought out in the spring of the year 1840, and by the spring of 1843 the five hundred pages of the second volume, mostly occupied by the vast order *Compositæ*, had been issued. But meanwhile I had in my turn to assume professorial duties and incident engagements, — with the result that, although the study of North Ameri-

can plants was at no time pretermitted, either by Dr. Torrey while he lived, or by myself, we were unable to continue the publication during my associate's lifetime; and it was only recently, in the spring of 1878, that I succeeded in bringing out, in a changed form, another instalment of the work, completing the *Gamopetalæ*.

In the interval I had made two year-long visits to Europe for botanical investigation, the first partly relating to the botany of the South Pacific, the second wholly in view of the North American flora. And since this last publication still another visit — the fourth and we may suppose the last — of the same character and the same duration, has been successfully accomplished.

The serious question, in which we are all concerned, arises, whether this work can be carried through to completion, and the older parts (wholly out of print and out of date), reëlaborated, — I will not say by my hands, but in my time, or soon enough to render the whole a reasonably full and homogeneous representation of the North American flora, as known in this latter part of the nineteenth century. And it brings us to consider why the undertaking to which so much time has been devoted should be so slow of accomplishment.

If this slowness is a constant wonder and disappointment to most people interested in the matter, I can only add that it is hardly less so to myself. It is a constant surprise — if one may so say — that the work does not get on faster.

Of course the undertaking has become more and more formidable with the enlargement of geographical boundaries and of the number of species discovered. As to the increase in the number of species to be treated, we have by no means yet reached the end. The area, that of our continent down to the Mexican line, we trust is definitely fixed, at least for our day. And since we cannot be rid of the peninsula and keys of Florida, which entails upon us a considerable number of tropical species, mostly belonging to the West Indies — the southern boundary is now as natural a one as we can have.

The area which Pursh's Flora covered was, we may say, the United States east of the Mississippi, with Canada to Labra-

dor, to which was added a couple of hundred of species known to him outside these limits northwestward.

Torrey and Gray's Flora took the initiative in annexing Texas, ten years before its political incorporation into the Union; although the only plants we then possessed from it were certain portions of Drummond's collections. California was also annexed at the same time, on account of Douglas's collections, and those of Nuttall, who had just returned from his visit to the western coast, which he reached by a tedious journey across the continent over ground in good part new to the botanist. Douglas had already made remarkably full collections along a more northern line. The British arctic explorers, both by sea and land, had well developed the botany of the boreal regions, and Sir William Hooker was bringing out the results in his Flora of British America. Of course our knowledge of the whole interior and western region was small indeed, compared with the present; and the botany of a vast region from the western part of Texas to the Californian coast was absolutely unknown, and so remained until after the publication of the Flora was suspended.

As to the number of species which Torrey and Gray had to deal with, I can only say that a rapid count gives us for the first volume about 2200 *Polypetalæ;* that there are one hundred and nine species in the small orders which in the second volume precede the *Compositæ;* and that there are of the *Compositæ* 1054. So one may fairly conclude that if the work had been pushed on to completion, say in the year 1850, the 3076 species of Pursh's Flora in the year 1814 might have been just about doubled. Probably more rather than less; for if we reckon from the number of the *Compositæ*, and on the estimate that they constitute one-eighth of the phænogamous plants of North America, instead of 6150, there would have been 8430 species known in the year specified.

It most concerns us to know the number of species which, after the lapse of thirty years more — years in which exploration has been active, and has left no considerable part of our great area wholly unvisited — the now revived Flora has to deal with. We can make an estimate which cannot be far

wrong. In the year 1878, my colleague, Mr. Watson, finished and published his "Bibliographical Index to the Polypetalæ of North America," covering, that is, the same ground as the first volume of Torrey and Gray's Flora, completed in 1840. In it the 2200 species of the latter date are increased to 3038. The "*Gamopetalæ* after *Compositæ*" in the "Synoptical Flora," brought out in the same year, contains 1656 species. The two together must make up half of our phænogamous botany, that is, adding the increase of the last four years, about 5000 species. And so Mr. Watson adopts the estimate of 10,000 species of our known Phænogams and Ferns. My impression is that the species of *Compositæ* have increased at a rate which, unless they exceed the eight part of our Phænogams, will warrant a still higher estimate. The number of introduced species of various orders, which will have to be enumerated and most of them described, is, unhappily, fast increasing;[1] and new indigenous species are almost daily coming to us from some part or other of our wide territory. So that the 10,000 species of this estimate may before long rise to eleven or twelve thousand. Only the experienced botanist can form a just idea of what is involved in the accurate discrimination and proper coördination of 10,000 to 12,000 species, and in the putting of the results into the language and form which may make our knowledge available to learners or to succeeding botanists.

Moreover, there is of late an *embarras des richesses* which is becoming serious as respects labor and time. The continued and ever increasing influx of material to Cambridge, beneficial as it ever is, is accountable for this retardation of progress in a greater degree than almost any one would suppose. The herbarium, upon whose materials this work is mainly done, and which has been, like the Temple, full forty and six years in building, has received the contributions of two generations of botanists, and the Torrey herbarium goes back one generation farther. Still the number of American

[1] I say "unhappily," for they adulterate the natural character of our flora, and raise difficult questions as to how much of introduction and settlement should give to these denizens the rights of adopted citizens.

specimens annually coming to it is greater than in most former years. Apart from the mere selection and care of these, consider how in other ways it affects the rate of progress of the Flora. The incoming of additional specimens may at a glance settle doubt as to the validity of a species; but new specimens are as apt to raise questions as to settle them; more commonly they raise the question as to the limitation and right definition of the species concerned, not rarely, also, that of their validity. When one has only single specimens of related species, the case may seem clear and the definition easy. The acquisition of a few more, from a different region or grown under different conditions, almost always calls for some reconsideration, not rarely for reconstruction. People generally suppose that species, and even genera, are like coin from the mint, or bank notes from the printing press, each with its fixed marks and signature, which he that runs may read, or the practised eye infallibly determine. But in fact species are judgments — judgments of variable value, and often very fallible judgments, as we botanists well know. And genera are more obviously judgments, and more and more liable to be affected by new discoveries. Judgments formed to-day — perhaps with full confidence, perhaps with misgiving — may to-morrow, with the discovery of new materials or the detection of some before unobserved point of structure, have to be weighed and decided anew. You see how all this bears upon the question of time and labor in the preparation of the Flora of a great country. If even in Old Europe the work has to be done over and over, how much more so in America, where new plants are almost daily coming to hand. It is true that these fall into their ranks, or are adjustable into their proper or probable places, but not without painstaking and tedious examination.

Of our Flora, it may indeed be said, that "If it were done, when 't is done, then 't were well it were done quickly." But I may have made it clear that, in the actual state of the case, it is likely to be done slowly. At least you will understand why thus far it has been done slowly. As to the future, if it depended wholly upon me, the completion would obvi-

ously be hopeless. I need not say that our dependence, for the actual elaboration, must largely be upon associates, upon the few who have the training and the vast patience, and the access to herbaria and libraries, requisite for this kind of work, but above all upon my associate in the herbarium at Cambridge, to whom, being present with us, I will not further allude.

Of course we rely, very much indeed, upon the continued coöperation of all the cultivators of botany in the country; and it is gratifying to know that their number is increasing, new ones not less zealous than the old, and better equipped, are taking the place of those that have passed away, and some of them extending their exploration over the remotest parts of the land, and into districts where there is most to be discovered. All can help on the work, and all are doing so, by communication of specimens and of observations. Those within the range of the published Manuals and Floras get on — or should get on — with only occasional help from us. They should send us notes and specimens to any amount; but they should not ask us to stop to examine and name their plants, except in special cases, which we are always ready enough to take up. Those who collect in regions as yet destitute of such advantages may claim more aid, and we take great pains to render it: partly on our own account, that we may assort their contributions into their proper places, partly for the encouragement of such correspondents, who otherwise would not know what they have obtained, and who naturally like to know when they have made interesting discoveries.

But the scattered and piecemeal study of plants is neither very satisfactory nor safe. And it involves great loss of time, besides interrupting that continuity and concentration of attention which the proper study of any group of plants demands. As respects the orders of plants which are yet to be elaborated for the Flora, and as to plants which require critical study or minute examination, necessarily consuming much time, it is better to defer their complete determination until the groups to which they severally belong are regularly taken in hand.

The coöperation of all our botanical associates is solicited in this regard, as a matter of common interest and advantage. For we are all equally concerned in forwarding the progress of the Flora of North America; and we may confidently expect from our botanical associates their sympathy, their forbearance, and their continued aid.

GENDER OF NAMES OF VARIETIES.[1]

Among other subordinate questions in Natural-history nomenclature, it has been asked whether names of varieties, like those of species, should conform in gender to the genus, or whether they may not as well conform to the word *varietas*, and so always be feminine.

Linnæus introduced the current practice of numbering varieties by the letters of the Greek alphabet α, β, γ, etc. But to some varieties, evidently to the more important, he gave names. These names, when adjectives, were always (so far as we know) made to agree in gender with the generic name, *e. g.: Viburnun Opulus*, β *roseum. Asparagus officinalis*, α *maritimus*, β *altilis. Mesembryanthemum ringers*, α *canium*, β *felinum.*

In our days named varieties play a more and more important part; and all botanists, as a rule, appear to have followed the Linnean model, with now and then a divergence which is readily explained, and which may be said to be accidental, such as *Ripogonum album*, var. *leptostachya*, Benth.

This is as one writes "form a *albiflora*" or "var. *albiflora*," a white-flowered form or variety. But that this is not the pattern nor the true construction of varietal names appears at once on reference to ordinary cases. Thus, for example, in "*Nasturium amphibium*, α *indivisum*, DC. Syst.," it is not an individual variety of the species that is meant, but a name which stands in the same grammatical relation to Nasturtium that amphibium does, and to write *N. amphibium*, α *indivisa*, is obviously wrong. We should say that it makes no difference whether the word variety, or its abbreviation var. is expressed or understood. When the conditions of the case seem to call for it, we should write *N. amphibium*, var. α *indivisum*, just as, if it were ever needful, we might write

[1] American Journal of Science and Arts, 3 ser., xxvii. 396. (1884.)

"*Nasturtium*, spec. *amphibium*," and just as L. C. Richard (a good model), in Michaux's Flora writes, *Viburnum dentatum*, var. α *glabellum*, β *semi-tomentosum*. *Rhus Toxicodendron*, var. α *vulgare*, β *quercifolium*.

The editor of the "Gardener's Chronicle" (March 22, p. 373), having put this kind of question to M. Alphonse De Candolle (whom we should consider the highest living authority upon nomenclatural matters), understands him to reply that "the insertion of the abbreviation *var.* for *varietas*, which is feminine, demands a feminine termination; but if the word *var.* be omitted, then the rule would be for the variety to follow the specific name;" — meaning probably the generic name, for in one of the examples given, *Thymus Serpyllum*, β *montanus*, it does not follow the specific.

From this point of view, namely: that where the nature of the group (in this case variety) is expressed, the adjective name should be feminine, but where only understood, it might be masculine or neuter — we must commend the editor's closing remark: —

"Perhaps the simplest and most easily recollected rule would be to make the varietal name feminine in all cases, whether the *var.* of *varietas*, were expressed, or understood. This at least would be intelligible, and would conduce to uniformity of practice."

It would also be logical, and the logic also would require all specific names to be feminine; for the word understood, *species*, is feminine.

Now we do not suppose that M. De Candolle would tolerate a double set of genders for the names of varieties. His doctrine is that the "var." should be discarded and the Greek letters only employed, not only for numbering the varieties, but for designating the fact that the name they are prefixed to is a variety.

It is not difficult to perceive why it has come to pass that "English writers generally use the abbreviation var.," and that some continental botanical writers follow the practice. One reason is, that it enables us to cite an author's variety by its name without having to concern ourselves with its Greek number, whether it is β or γ or δ, which otherwise we should

have to attend to. Another is, that our sense of good form revolts at beginning sentences and paragraps without capitals. In our books, varieties usually stand in independent paragraphs. Even in Latin we do not like to begin a paragraph —

"*a indivisum* foliis omnibus integerrimis serratisve, non aut vix basi auriculatis."

In English we can still less abide it. So we prefix "Var.," and either number our varieties with Greek letters or, preferentially, leave them out.

But, we did not suppose that by the employment of the word "var." we had interfered with the relation of the name of the variety to that of its genus. Var. *indivisum*, in this case, we should construe the phrase: "Varietas cujus nomen est *indivisum*. 'Var. *indivisum*' stands on the same ground as 'species amphibium.'" The latter rank we rarely need to express, because we always prefix the generic name or its initial. The former may often come in a shape which renders the designating prefix "var." necessary, or at least most convenient.

We may indeed, quite correctly write, var. *albiflora*, a white flowered variety, var. *longifolia*, a long-leaved variety; but that is not according to the Linnean pattern nor to the regular practice, nor to the strict analogy of the varietal name with the specific.

Moreover, if the gender of the word which designates the grade of the name is to govern the gender of the name, at least when expressed, as by *var.*, then all subspecies must be made feminine. Now this term subspecies is coming largely into use. And it has to be expressed in every case, in this wise: *Ranunculus aquatilis*, *L.* Subsp. *heterophyllus*. Subsp. *hederaceus*, etc.

If the proposition which we deprecate is adopted, these names would have to be written *heterophylla* and *hederacea* by an author who ranked them as subspecies but *heterophyllus* and *hederaceus* by one who took them as varieties and simply numbered them by Greek letters. Obviously the propositions in the "Gardener's Chronicle" has not been thoroughly worked out.

CHARACTERISTICS OF THE NORTH AMERICAN FLORA.[1]

When the British Association, with much painstaking, honors and gratifies the cultivators of science on this side of the ocean by meeting on American soil, it is but seemly that a corresponding member for the third of a century should endeavor to manifest his interest in the occasion and to render some service, if he can, to his fellow-naturalists in Section D. I would attempt to do so by pointing out, in a general way, some of the characteristic features of the vegetation of the country which they have come to visit, — a country of "magnificent distances," but of which some vistas may be had by those who can use the facilities which are offered for enjoying them. Even to those who cannot command the time for distant excursions, and to some who may know little or nothing of botany, the sketch which I offer may not be altogether uninteresting. But I naturally address myself to the botanists of the Association, to those who, having crossed the wide Atlantic, are now invited to proceed westward over an almost equal breadth of land; some, indeed, have already journeyed to the Pacific coast, and have returned; and not a few, it is hoped, may accept the invitation to Philadelphia, where a warm welcome awaits them — warmth of hospitality, rather than of summer temperature, let us hope; but Philadelphia is proverbial for both. There opportunities may be afforded for a passing acquaintance with the botany of the Atlantic border of the United States, in company with the botanists of the American Association, who are expected to muster in full force.

What may be asked of me, then, is to portray certain out-

[1] An Address to the Botanists of the British Association for the Advancement of Science at Montreal; read to the Biological Section, August 29, 1884. (American Journal of Science and Arts, 3 ser., xxviii. 323.)

lines of the vegetation of the United States and the Canadian Dominion, as contrasted with that of Europe; perhaps also to touch upon the causes or anterior conditions to which much of the actual differences between the two floras may be ascribed. For, indeed, however interesting or curious the facts of the case may be in themselves, they become far more instructive when we attain to some clear conception of the dependent relation of the present vegetation to a preceding state of things, out of which it has come.

As to the Atlantic border on which we stand, probably the first impression made upon the botanist or other observer coming from Great Britain to New England or Canadian shores, will be the similarity of what he here finds with what he left behind. Among the trees the White Birch and the Chestnut will be identified, if not as exactly the same, yet with only slight differences — differences which may be said to be no more essential or profound than those in accent and intonation between the British speech and that of the "Americans." The differences between the Beeches and Larches of the two countries are a little more accentuated; and still more those of the Hornbeams, Elms, and the nearest resembling Oaks. And so of several other trees. Only as you proceed westward and southward will the differences overpower the similarities, which still are met with.

In the fields and along open roadsides the likeness seems to be greater. But much of this likeness is the unconscious work of man, rather than of Nature, the reason of which is not far to seek. This was a region of forest, upon which the aborigines, although they here and there opened patches of land for cultivation, had made no permanent encroachment. Not very much of the herbaceous or other low undergrowth of this forest could bear exposure to the fervid summer's sun; and the change was too abrupt for adaptive modification. The plains and prairies of the great Mississippi Valley were then too remote for their vegetation to compete for the vacancy which was made here when forest was changed to grain-fields and then to meadow and pasture. And so the vacancy came to be filled in a notable measure by agrestial plants from Eu-

rope, the seeds of which came in seed-grain, in the coats and fleece and in the imported fodder of cattle and sheep, and in the various but not always apparent ways in which agricultural and commercial people unwittingly convey the plants and animals of one country to another. So, while an agricultural people displaced the aborigines which the forests sheltered and nourished, the herbs, purposely or accidentally brought with them, took possession of the clearings, and prevailed more or less over the native and rightful heirs to the soil, — not enough to supplant them, indeed, but enough to impart a certain adventitious Old World aspect to the fields and other open grounds, as well as to the precincts of habitations. In spring-time you would have seen the fields of this district yellow with European Buttercups and Dandelions, then whitened with the Ox-eye Daisy, and at midsummer brightened by the cerulean blue of Chicory. I can hardly name any native herbs which in the fields and at the season can vie with these intruders in floral show. The common Barberry of the Old World is an early denizen of New England. The tall Mullein, of a wholly alien race, shoots up in every pasture and new clearing, accompanied by the common Thistle, while another imported Thistle, called in the United States "the Canada Thistle," has become a veritable nuisance, at which much legislation has been leveled in vain.

According to tradition the wayside Plantain was called by the American Indian "White-Man's foot," from its springing up wherever that foot had been planted. But there is some reason for suspecting that the Indian's ancestors brought it to this continent. Moreover there is another reason for surmising that this long-accepted tradition is fictitious. For there was already in the country a native Plantain, so like *Plantago major* that the botanists have only of late distinguished it. (I acknowledge my share in the oversight.) Possibly, although the botanists were at fault, the aborigines may have known the difference. The cows are said to know it. For a brother botanist of long experience tells me that, where the two grow together, cows freely feed upon the undoubtedly native species, and leave the naturalized one untouched.

It has been maintained that the ruderal and agrestial Old World plants and weeds of cultivation displace the indigenous ones of newly settled countries in virtue of a strength which they have developed through survival in the struggle of ages, under the severe competition incident to their former migrations. And it does seem that most of the pertinacious weeds of the Old World which have been given to us may not be indigenous even to Europe, at least to western Europe, but belong to campestrine or unwooded regions farther east; and that, following the movements of pastoral and agricultural people, they may have played somewhat the same part in the once forest-clad western Europe that they have been playing here. But it is unnecessary to build much upon the possibly fallacious idea of increased strength gained by competition. Opportunity may count for more than exceptional vigor; and the cases in which foreign plants have shown such superiority are mainly those in which a forest-destroying people have brought upon newly-bared soil the seeds of an open-ground vegetation.

The one marked exception that I know of, the case of recent and abundant influx of this class of Old World plants into a naturally treeless region, supports the same conclusion. Our associate, Mr. John Ball, has recently called attention to it. The pampas of southeastern South America beyond the Rio Colorado, lying between the same parallels of latitude in the south as Montreal and Philadelphia in the north, and with climate and probably soils fit to sustain a varied vegetation, and even a fair proportion of forest, are not only treeless, but excessively poor in their herbaceous flora. The district has had no trees since its comparatively recent elevation from the sea. As Mr. Darwin long ago intimated: "Trees are absent not because they cannot grow and thrive, but because the only country from which they could have been derived — tropical and sub-tropical South America — could not supply species to suit the soil and climate." And as to the herbaceous and frutescent species, to continue the extract from Mr. Ball's instructive paper recently published in the Linnæan Society's Journal, "in a district raised from the sea during the latest

geological period, and bounded on the west by a great mountain range mainly clothed with an alpine flora requiring the protection of snow in winter, and on the north by a warm-temperate region whose flora is mainly of modified sub-tropical origin — the only plants that could occupy the newly-formed region were the comparatively few which, though developed under very different conditions, were sufficiently tolerant of change to adapt themselves to the new environment. The flora is poor, not because the land cannot support a richer one, but because the only regions from which a large population could be derived are inhabited by races unfit for emigration."

Singularly enough, this deficiency of herbaceous plants is being supplied from Europe, and the incomers are spreading with great rapidity; for lack of other forest material even Apple-trees are running wild and forming extensive groves. Men and cattle are, as usual, the agents of dissemination. But colonizing plants are filling, in this instance, a vacancy which was left by nature, while ours was made by man. We may agree with Mr. Ball in the opinion that the rapidity with which the intrusive plants have spread in this part of South America "is to be accounted for, less by any special fitness of the immigrant species, than by the fact that the ground is to a great extent unoccupied."

The principle applies here also; and in general, that it is opportunity rather than specially acquired vigor that has given Old World weeds an advantage may be inferred from the behavior of our weeds indigenous to the country, the plants of the unwooded districts — prairies or savannas west and south, — which, now that the way is open, are coming in one by one into these eastern parts, extending their area continually, and holding their ground quite as pertinaciously as the immigrant denizens. Almost every year gives new examples of the immigration of campestrine western plants into the eastern States. They are well up to the spirit of the age; they travel by railway. The seeds are transported, some in the coats of cattle and sheep on the way to market, others in the food which supports them on the journey, and many in a way which you might not suspect, until you

consider that these great roads run east and west, that the prevalent winds are from the west, that a freight-train left unguarded was not long ago blown on for more than one hundred miles before it could be stopped, not altogether on down grades, and that the bared and mostly unkempt borders of these railways form capital seed-beds and nursery-ground for such plants.

Returning now from this side issue, let me advert to another and, I judge, a very pleasant experience which the botanist and the cultivator may have on first visiting the American shores. At almost every step he comes upon old acquaintances, upon shrubs and trees and flowering herbs, mostly peculiar to this country, but with which he is familiar in the grounds and gardens of his home. Great Britain is especially hospitable to American trees and shrubs. There those both of the eastern and western sides of our continent flourish side by side. Here they almost wholly refuse such association. But the most familiar and longest-established representatives of our flora (certain western annuals excepted) were drawn from the Atlantic coast. Among them are the Virginia Creeper or Ampelopsis, almost as commonly grown in Europe as here, and which, I think, displays its autumnal crimson as brightly there as along the borders of its native woods where you will everywhere meet with it; the Red and Sugar Maples, which give the notable autumnal glow to our northern woods, but rarely make much show in Europe, perhaps for lack of sharp contrast between summer and autumn; the ornamental Ericaceous shrubs, Kalmias, Azaleas, Rhododendrons, and the like, specially called American plants in England, although all the Rhododendrons of the finer sort are half Asiatic, the hardy American species having been crossed and recrossed with more elegant but tender Indian species.

As to flowering herbs, somewhat of the delight with which an American first gathers wild Primroses and Cowslips and Foxgloves and Daisies in Europe, may be enjoyed by the European botanist when he comes upon our Trilliums and Sanguinaria, Cypripediums and Dodecatheon, our species of Phlox, Coreopsis, etc., so familiar in his gardens; or when,

crossing the continent, he comes upon large tracts of ground yellow with Eschscholtzia or blue with Nemophilas. But with a sentimental difference; in that Primroses, Daisies, and Heaths, like nightingales and larks, are inwrought into our common literature and poetry, whereas our native flowers and birds, if not altogether unsung, have attained at the most to only local celebrity.

Turning now from similarities, and from that which interchange has made familiar, to that which is different or peculiar, I suppose that an observant botanist upon a survey of the Atlantic border of North America (which naturally first and mainly attracts our attention) would be impressed by the comparative wealth of this flora in trees and shrubs. Not so much so in the Canadian Dominion, at least in its eastern part; but even here the difference will be striking enough on comparing Canada with Great Britain.

The *Coniferæ*, native to the British Islands, are one Pine, one Juniper, and a Yew; those of Canada proper are four or five Pines, four Firs, a Larch, an Arbor-Vitæ, three Junipers, and a Yew, fourteen or fifteen to three. Of Amentaceous trees and shrubs, Great Britain counts one Oak (in two marked forms), a Beech, a Hazel, a Hornbeam, two Birches, an Alder, a Myrica, eighteen Willows, and two Poplars, — twenty-eight species in nine genera, and under four natural orders. In Canada there are at least eight Oaks, a Chestnut, a Beech, two Hazels, two Hornbeams of distinct genera, six Birches, two Alders, about fourteen Willows and five Poplars, also a Plane tree, two Walnuts and four Hickories; say forty-eight species, in thirteen genera, and belonging to seven natural orders. The comparison may not be altogether fair; for the British flora is exceptionally poor, even for islands so situated. But if we extend it to Scandinavia, so as to have a continental and an equivalent area, the native *Coniferæ* would be augmented only by one Fir, the *Amentaceæ* by several more Willows, a Poplar, and one or two more Birches; no additional orders nor genera.

If we take in the Atlantic United States east of the Mississippi, and compare this area with Europe, we should find

the species and the types increasing as we proceed southward, but about the same numerical proportion would hold.

But, more interesting than this numerical preponderance — which is practically confined to the trees and shrubs — will be the extra-European types, which, intermixed with familiar Old-World forms, give peculiar features to the North American flora, — features discernible in Canada, but more and more prominent as we proceed southward. Still confining our survey to the Atlantic district, that is, without crossing the Mississippi, the following are among the notable points:

1. Leguminous Trees of peculiar types. Europe alongside its leguminous shrubs or under-shrubs, mostly of the Genistean tribe, which is wanting in all North America, but has no Leguminous tree of more pretence than the Cercis and Laburnum. Our Atlantic forest is distinguished by a Cercis of its own, three species of Locust, two of them fine trees, and two Honey Locusts, the beautiful Cladrastis, and the stately Gymnocladus. Only the Cercis has any European relationship. For relatives of the others we must look to the China-Japanese region.

2. The great development of the *Ericaceæ* (taking the order in its widest sense), along with the absence of the Ericeous tribe, that is, of the Heaths themselves. We possess on this side of the Mississippi thirty genera, and not far from ninety species. All Europe has only seventeen genera and barely fifty species. We have most of the actual European species, excepting their Rhododendrons and their Heaths, — and even the latter are represented by some scattered patches of Calluna, of which it may be still doubtful whether they are thence introductions or sparse and scanty survivals; and besides we have a wealth of peculiar genera and species. Among them the most notable in an ornamental point of view are the Rhododendrons, Azaleas, Kalmias, Andromedas, and Clethras; in botanical interest, the endemic *Monotropeæ*, of which there is only one species in Europe, but seven genera in North America, all but one absolutely peculiar; and in edible as well as botanical interest, the unexampled development and diversification of the genus Vaccinium (along with

the allied American type, Gaylussacia) will attract attention. It is interesting to note the rapid falling away of *Ericaceæ* westward in the valley of the Mississippi as the forest thins out.

3. The wealth of this flora in *Compositæ* is a most obvious feature; one especially prominent at this season of the year, when the open grounds are becoming golden with Solidago, and the earlier of the autumnal Asters are beginning to blossom. The *Compositæ* form the largest order of Phænogamous plants in all temperate floras of the northern hemisphere, are well up to the average in Europe, but are nowhere so numerous as in North America, where they form an eighth part of the whole. But the contrast between the *Compositæ* of Europe and Atlantic North America is striking. Europe runs to Thistles, to *Inuloideæ*, to *Anthemideæ*, and to *Cichoriaceæ*. It has very few Asters, and only two Solidagoes, no Sunflowers, and hardly anything of that tribe. Our Atlantic flora surpasses all the world in Asters and Solidagoes, as also in Sunflowers and their various allies, is rich in *Eupatoriaceæ*, of which Europe has extremely few, and is well supplied with *Vernoniaceæ* and *Helenioideæ*, of which she has none; but is scanty in all the groups that predominate in Europe. I may remark that if our larger and most troublesome genera, such as Solidago and Aster, were treated in our systematic works even in the way that Nyman has treated Hieracium in Europe, the species of these two genera (now numbering seventy-eight and one hundred and twenty-four respectively) would be at least doubled.

4. Perhaps the most interesting contrast between the flora of Europe and that of the eastern border of North America is in the number of generic and even ordinal types here met with which are wholly absent from Europe. Possibly we may distinguish these into two sets of differing history. One will represent a tropical element, more or less transformed, which has probably acquired or been able to hold its position so far north in virtue of our high summer temperature. (In this whole survey the peninsula of Florida is left out of view, regarding its botany as essentially Bahaman and Cuban, with a

certain admixture of northern elements.) To the first type I refer such trees and shrubs as Asimina, sole representative of the *Anonaceæ* out of the tropics, and reaching even to lat. 42°; Chrysobalanus, representing a tropical suborder; Pinckneya, representing as far north as Georgia the Cinchoneous tribe; the Baccharis of our coast, reaching even to New England; Cyrilla and Cliftonia, the former actually West Indian; Bumelia, representing the tropical order *Sapotaceæ;* Bignonia and Tecoma of the *Bignoniaceæ;* Forestiera in *Oleaceæ;* Persea of the *Laurineæ;* and finally the *Cactaceæ.* Among the herbaceous plants of this set I will allude only to some of peculiar orders. Among them I reckon Sarracenia, of which the only extra-North American representative is tropical-American, the *Melastomaceæ*, represented by Rhexia; Passiflora (our species being herbaceous), a few representatives of *Loasaceæ* and *Turneraceæ*, also of *Hydrophyllaceæ;* our two genera of *Burmanniaceæ;* three genera of *Hæmodoraceæ;* Tillandsia in *Bromeliaceæ;* two genera of *Pontederiaceæ;* two of *Commelynaceæ;* the outlying Mayaca and Xyris, and three genera of *Eriocaulonaceæ.* I do not forget that one of our species of Eriocaulon occurs on the west coast of Ireland and in Skye, wonderfully out of place, though on this side of the Atlantic it reaches Newfoundland. It may be a survival in the Old World; but it is more probably of chance introduction.

The other set of extra-European types, characteristic of the Atlantic North American flora, is very notable. According to a view which I have much and for a long while insisted on, it may be said to represent a certain portion of the once rather uniform flora of the arctic and less boreal zone, from the late Tertiary down to the incoming of the Glacial period, and which, brought down to our lower latitudes by the gradual refrigeration, has been preserved here in eastern North America and in the corresponding parts of Asia, but was lost to Europe. I need not recapitulate the evidence upon which this now generally accepted doctrine was founded; and to enumerate the plants which testify in its favor would amount to an enumeration of the greater part of the genera or sub-

ordinate groups of plants which distinguish our Atlantic flora from that of Europe. The evidence, in brief, is that the plants in question, or their moderately differentiated representatives, still coexist in the flora of eastern North America and that of the Chino-Japanese region, the climates and conditions of which are very similar; and that the fossilized representatives of many of them have been brought to light in the late tertiary deposits of the arctic zone wherever explored. In mentioning some of the plants of this category I include the Magnolias, although there are no nearly identical species, but there is a seemingly identical Liriodendron in China, and the Schizandras and Illiciums are divided between the two floras; and I put into the list Menispermum, of which the only other species is eastern Siberian, and is hardly distinguishable from ours. When you call to mind the series of wholly extra-European types which are identically or approximately represented in the eastern North American and in the eastern Asiatic temperate floras, such as Trautvetteria and Hydrastis in *Ranunculaceæ;* Caulophyllum, Diphylleia, Jeffersonia and Podophyllum in *Berberideæ;* Brasenia and Nelumbium in *Nymphæaceæ;* Stylophorum in *Papaveraceæ;* Stuartia and Gordonia in *Ternstræmiaceæ;* the equivalent species of Xanthoxylum, the equivalent and identical species of Vitis, and of the poisonous species of Rhus (one, if not both, of which you may meet with in every botanical excursion, and which it will be safer not to handle); the Horse-chestnuts, here called Buckeyes; the Negundo, a peculiar offshoot of the Maple tribe; when you consider that almost every one of the peculiar Leguminous tree mentioned as characteristic of our flora is represented by a species in China or Mandchuria or Japan, and so of some herbaceous *Leguminosæ;* when you remember that the peculiar small order of which Calycanthus is the principal type has its other representative in the same region; that the species of Philadelphus, of Hydrangea, of Itea, Astilbe, Hamamelis, Diervilla, Triosteum, Mitchella which carpets the ground under evergreen woods, Chiogenes, creeping over the shaded bogs; Epigæa, choicest woodland flower of early spring; Elliottia; Shortia

(the curious history of which I need not rehearse); Styrax of cognate species; Nyssa, the Asiatic representatives of which affect a warmer region; Gelsemium, which under the name of Jessamine is the vernal pride of the southern Atlantic States; Pyrularia and Buckleya, peculiar Santalaceous shrubs; Sassafras and Benzoins of the Laurel family; Planera and Maclura; Pachysandra of the Box tribe; the great development of the *Juglandaceæ* (of which the sole representative in Europe probably was brought by man into southeastern Europe in pre-historic times); our Hemlock Spruces, Arbor-Vitæ, Chamæcyparis, Taxodium, and Torreya, with their east Asian counterparts, the *Roxburghiaceæ*, represented by Croomia, — and I might much further extend and particularize the enumeration, — you will have enough to make it clear that the peculiarities of the one flora are the peculiarities of the other, and that the two are in striking contrast with the flora of Europe.

This contrast is susceptible of explanation. I have ventured to regard the two antipodal floras thus compared as the favored heirs of the ante-glacial high-northern flora, or rather as the heirs who have retained most of their inheritance. For, inasmuch as the present arctic flora is essentially the same round the world, and the Tertiary fossil plants entombed in the strata beneath are also largely identical in all the longitudes, we may well infer that the ancestors of the present northern temperate plants were as widely distributed throughout their northern home. In their enforced migration southward, geographical configuration and climatic differences would begin to operate. Perhaps the way into Europe was less open than into the lower latitudes of America and eastern Asia, although there is reason to think that Greenland was joined to Scandinavia. However that be, we know that Europe was fairly well furnished with many of the vegetable types that are now absent, possibly with most of them. Those that have been recognized are mainly trees and shrubs, which somehow take most readily to fossilization, but the herbaceous vegetation probably accompanied the arboreal. At any rate, Europe then possessed Torreyas, and Gingkos, Taxodium and

Glyptostrobus, Libocedrus, Pines of our five-leaved type, as well as the analogues of other American forms, several species of Juglans answering to the American forms, and the now peculiarly American genus Carya, Oaks of the American types, Myricas of the two American types, one or two Planer-trees, species of Populus answering to our Cottonwoods and our Balsam-poplar, a Sassafras and the analogues of our Persea and Benzoin, a Catalpa, Magnolias, and a Liriodendron, Maples answering to ours, and also a Negundo, and such peculiarly American Leguminous genera as the Locust, Honey Locust, and Gymnocladus. To understand how Europe came to lose these elements of her flora, and Atlantic North America to retain them, we must recall the poverty of Europe in native forest trees, to which I have already alluded. A few years ago, in an article on this subject, I drew up a sketch of the relative richness of Europe, Atlantic North America, Pacific North America, and the eastern side of temperate Asia in genera and species of forest trees.[1] In that sketch, as I am now convinced, the European forest elements were somewhat underrated. I allowed only thirty-three genera and eighty-five species, while to our Atlantic American forest were assigned sixty-six genera and one hundred and fifty-five species. I find from Nyman's Conspectus that there are trees on the southern and eastern borders of Europe which I had omitted; that there are good species which I had reckoned as synonyms, and some that may rise to arboreal height which I had counted as shrubs. But on the other hand and for the present purpose it may be rejoined that the list contained several trees, of as many genera, which were probably carried from Asia into Europe by the hand of man. On Nyman's authority I may put into this category *Cercis Siliquastrum*, *Ceratonia Siliqua*, *Diospyros Lotus*, *Styrax officinalis*, the Olive, and even the Walnut, the Chestnut, and the Cypress. However this may be, it seems clear that the native forest flora of Europe is exceptionally poor, and that it has lost many species and types which once belonged to it. We must suppose that the herbaceous flora has suffered in the same

[1] American Journal of Science and Arts, 3 ser., xvi. 85.

way. I have endeavored to show how this has naturally come about. I cannot state it more concisely than in the terms which I used six years ago.

"I conceive that three things have conspired to this loss of American, or as we might say, of normal types sustained by Europe. First, Europe, extending but little south of lat. 40°, is all within the limits of severe glacial action. Second, its mountains trend east and west, from the Pyrenees to the Carpathians and the Caucasus beyond: they had glaciers of their own, which must have begun their work and poured down the northward flanks while the plains were still covered with forest on the retreat from the great ice forces coming from the north. Attacked both on front and rear, much of the forest must have perished then and there.

"Third, across the line of retreat of whatever trees may have flanked the mountain ranges, or were stationed south of them, stretched the Mediterranean, an impassable barrier. . . . Escape by the east, and rehabilitation from that quarter until a very late period, was apparently prevented by the prolongation of the Mediterranean to the Caspian, and probably thence to the Siberian Ocean. If we accept the supposition of Nordenskjöld that, anterior to the Glacial period, Europe was 'bounded on the south by an ocean extending from the Atlantic over the present deserts of Sahara and Central Asia to the Pacific,' all chance of these American types having escaped from and reëntered Europe from the south and east seems excluded. Europe may thus be conceived to have been for a time somewhat in the condition in which Greenland is now. . . . Greenland may be referred to as a country which, having undergone extreme glaciation, bears the marks of it in the extreme poverty of its flora, and in the absence of the plants to which its southern portion, extending six degrees below the arctic circle, might be entitled. It ought to have trees and it might support them. But since their destruction by glaciation no way has been open for their return. Europe fared much better, but has suffered in its degree in a similar way."

Turning to this country for a contrast, we find the conti-

nent on the eastern side unbroken and open from the arctic circle to the tropic, and the mountains running north and south. The vegetation when pressed on the north by oncoming refrigeration had only to move its southern border southward to enjoy its normal climate over a favorable region of great extent; and, upon the recession of glaciation to the present limit, or in the oscillations which intervened, there was no physical impediment to the adjustment. Then, too, the more southern latitude of this country gave great advantage over Europe. The line of terminal moraines, which marks the limit of glaciation, rarely passes the parallel of 40° or 39°. Nor have any violent changes occurred here, as they have on the Pacific side of the continent, within the period under question. So, while Europe was suffering hardship, the lines of our Atlantic American flora were cast in pleasant places, and the goodly heritage remains essentially unimpaired.

The transverse direction and the massiveness of the mountains of Europe, while they have in part determined the comparative poverty of its forest-vegetation, have preserved there a rich and widely distributed alpine flora. That of Atlantic North America is insignificant. It consists of a few arctic plants, left scattered upon narrow and scattered mountain-tops, or in cool ravines of moderate elevation; the maximum altitude is only about 6000 feet in lat. 44°, on the White Mountains of New Hampshire, where no winter snow outlasts mid-summer. The best alpine stations are within easy reach of Montreal. But as almost every species is common to Europe, and the mountains are not magnificent, they offer no great attraction to a European botanist.

Farther south, the Appalachian Mountains are higher, between lat. 36° and 34° rising considerably above 6000 feet; they have botanical attractions of their own, but they have no alpine plants. A few sub-alpine species linger on the cool shores of Lake Superior, at a comparatively low level. Perhaps as many are found nearly at the level of the sea on Anticosti, in the Gulf of St. Lawrence, abnormally cooled by the Labrador current.

The chain of the great fresh-water lakes, which are discharged by the brimming St. Lawrence, seems to have little effect upon our botany, beyond the bringing down of a few northwestern species. But you may note with interest that they harbor sundry maritime species, mementoes of the former saltness of these interior seas. *Cakile Americana*, much like the European Sea Rocket, *Hudsonia tomentosa* (a peculiar Cistaceous genus imitating a Heath), *Lathyrus maritimus*, and *Ammophila arenaria*, are the principal. Salicornia, Glaux, *Scirpus maritimus*, *Ranunculus Cymbalaria*, and some others, may be associated with them. But these are widely diffused over the saline soil which characterizes the plains beyond our wooded region.

I have thought that some general considerations like these might have more interest for the biological section at large than any particular indications of our most interesting plants, and of how and where the botanist might find them. Those who in these busy days can find time to herborize will be in the excellent hands of the Canadian botanists. At Philadelphia their brethren of the United States will be assembled to meet their visitors, and the Philadelphians will escort them to their classic ground, the Pine Barrens of New Jersey. To have an idea of this peculiar phytogeographical district, you may suppose a long wedge of the Carolina coast to be thrust up northward quite to New York harbor, bringing into a comparatively cool climate many of the interesting low-country plants of the south, which, at this season, you would not care to seek in their sultry proper homes. Years ago, when Pursh and Leconte and Torrey used to visit it, and in my own younger days, it was wholly primitive and upsoiled. Now, when the shore is lined with huge summer hotels, the Pitch Pines carried off for firewood, the bogs converted into Cranberry-grounds, and much of the light sandy or gravelly soil planted with wine-yards or converted into Melon and Sweet-potato patches, I fear it may have lost some of its botanical attractions. But large tracts are still nearly in a state of nature. *Drosera filiformis*, so unlike any European species, and the beautiful Sabbatias, the yellow Fringed Orchis,

Lachnanthes and Lophiola, the larger Xyrises and Eriocaulons, the curious grass Amphicarpum with cleistogamous flowers at the root, the showy species of Chrysopsis, and many others, must still abound. And every botanist will wish to collect *Schizæa pusilla*, rarest, most local, and among the smallest of Ferns.

If only the season would allow it, there is a more southern station of special interest, — Wilmington, on the coast of North Carolina. Carnivorous plants have, of late years, excited the greatest interest, both popular and scientific; and here, of all places, carnivorous plants seem to have their most varied development. For this is the only and the very local home of Dionæa; here grow almost all the North American species of Drosera; here or near by are most of the species of Sarracenia, of the bladder-bearing Utricularias, — one of which the president of our Section has detected in fish-catching, — and also the largest species of Pinguicula.

But at this season a more enjoyable excursion may be made to the southern portion of the Alleghany or Appalachian Mountains, which separate the waters of the Atlantic side from those of the Mississippi. These mountains are now easily reached from Philadelphia. In Pennsylvania, where they consist of parallel ridges without peaks or crests, and are of no great height, they are less interesting botanically than in Virginia; but it is in North Carolina and the adjacent borders of Tennessee that they rise to their highest altitude and take on more picturesque forms. On their sides the Atlantic forest, especially its deciduous-leaved portion, is still to be seen to greatest advantage, nearly in pristine condition, and composed of a greater variety of genera and species than in any other temperate region, excepting Japan. And in their shade are the greatest variety and abundance of shrubs, and a good share of the most peculiar herbaceous genera. This is the special home of our Rhododendrons, Azaleas, and Kalmias; at least here they flourish in greatest number and in most luxuriant growth. *Rhododendron maximum* (which is found in a scattered way even as far north as the vicinity of Montreal) and *Kalmia latifolia* (both called

Laurels) even become forest trees in some places; more commonly they are shrubs, forming dense thickets on steep mountain-sides, through which the traveler can make his way only by following old bear-paths, or by keeping strictly on the dividing crests of the leading ridges.

Only on the summits do we find *Rhododendron Catawbiense*, parent of so many handsome forms in English grounds, and on the higher wooded slopes the yellow and the flame-colored *Azalea calendulacea;* on the lower, the pink *A. nudiflora* and more showy *A. arborescens*, along with the common and widespread *A. viscosa*. The latter part of June is the proper time to explore this region, and, if only one portion can be visited, Roan Mountain should be preferred.

On these mountain-tops we meet with a curious anomaly in geographical distribution. With rarest exceptions, plants which are common to this country and to Europe extend well northward. But on these summits from southern Virginia to Carolina, yet nowhere else, we find — undoubtedly indigenous and undoubtedly identical with the European species — the Lily-of-the-Valley!

I have given so much of my time to the botany of the Atlantic border that I can barely touch upon that of the western regions.

Between the wooded country of the Atlantic side of the continent and that of the Pacific side lies a vast extent of plains which are essentially woodless, except where they are traversed by mountain-chains. The prairies of the Atlantic States bordering the Mississippi and of the Winnipeg country shade off into the drier and gradually more saline plains, which, with an even and gradual rise, attain an elevation of 5000 feet or more where they abut against the Rocky Mountains. Until these are reached (over a space from the Alleghanies westward of about twenty degrees of longitude) the plains are unbroken. To a moderate distance beyond the Mississippi the country must have been in the main naturally wooded. There is rainfall enough for forest on these actual prairies. Trees grow fairly well when planted; they are coming up spontaneously

under present opportunities ; and there is reason for thinking that all the prairies east of the Mississippi, and of the Missouri up to Minnesota, have been either greatly extended or were even made treeless under Indian occupation and annual burnings. These prairies are flowery with a good number of characteristic plants, many of them evidently derived from the plains farther west. At this season, the predominant vegetation is of *Compositæ*, especially of Asters and Solidagoes, and of Sunflowers, Silphiums, and other Helianthoid *Compositæ*.

The drier and barer plains beyond, clothed with the short Buffalo-grasses, probably never bore trees in their present state, except as now some Cottonwoods (*i. e.* Poplars) on the margins of the long rivers which traverse them in their course from the Rocky Mountains to the Mississippi. Westward, the plains grow more and more saline ; and Wormwoods and *Chenopodiaceæ* of various sorts form the dominant vegetation, some of them *sui generis* or at least peculiar to the country, others identical or congeneric with those of the steppes of central Asia. Along with this common campestrine vegetation there is a large infusion of peculiar American types, which I suppose came from the southward, and to which I will again refer.

Then come the Rocky Mountains, traversing the whole continent from north to south ; their flanks wooded, but not richly so, — chiefly with Pines and Firs of very few species, and with a single ubiquitous Poplar, their higher crests bearing a well-developed alpine flora. This is the arctic flora prolonged southward upon the mountains of sufficient elevation, with a certain admixture in the lower latitudes of types pertaining to the lower vicinity.

There are almost 200 alpine Phænogamous species now known on the Rocky Mountains ; fully three-quarters of which are arctic, including Alaskan and Greenlandian ; and about half of them are known in Europe. Several others are north Asian but not European. Even in that northern portion of the Rocky Mountains which the Association is invited to visit, several alpine species novel to European botany may be

met with; and farther south the peculiar forms increase. On the other hand, it is interesting to note how many Old-World species extend their range southward even to lat. 36° or 35°.

I have not seen the Rocky Mountains in the Dominion; but I apprehend that the aspect and character of the forest is Canadian, is mainly coniferous, and composed of very few species. Oaks and other cupuliferous trees, which give character to the Atlantic forest, are entirely wanting, until the southern confines of the region are reached in Colorado and New Mexico, and there they are few and small. In these southern parts there is a lesser amount of forest, but a much greater diversity of genera and species; of which the most notable are the Pines of the Mexican-plateau type.

The Rocky Mountains and the coast ranges on the Pacific side so nearly approach in British America that their forests merge, and the eastern types are gradually replaced by the more peculiar western. But in the United States a broad, arid and treeless, and even truly desert region is interposed. This has its greatest breadth and is best known where it is traversed by the Central Pacific Railroad. It is an immense plain between the Rocky Mountains and the Sierra Nevada, largely a basin with no outlet to the sea, covered with Sage-brush (*i. e.* peculiar species of Artemisia) and other subsaline vegetation, all of grayish hue; traversed, mostly north and south, by chains of mountains, which seem to be more bare than the plains, but which hold in their recesses a considerable amount of forest and of other vegetation, mostly of Rocky Mountains types.

Desolate and desert as this region appears, it is far from uninteresting to the botanist; but I must not stop to show how. Yet even the ardent botanist feels a sense of relief and exultation when, as he reaches the Sierra Nevada, he passes abruptly into perhaps the noblest coniferous forest in the world, — a forest which stretches along this range and its northern continuation, and along the less elevated ranges which border the Pacific coast, from the southern part of California to Alaska.

So much has been said about this forest, about the two

gigantic trees which have made it famous, and its Pines and Firs which are hardly less wonderful, and which in Oregon and British Columbia, descending into the plains, yield far more timber to the acre than can be found anywhere else, — I have myself discoursed upon the subject so largely on former occasions, that I may cut short all discourse upon the Pacific-coast flora and the questions it brings up.

I note only these points. Although this flora is richer than that of the Atlantic in *Coniferæ* (having almost twice as many species), richer indeed than any other except that of eastern Asia, it is very meagre in deciduous trees. It has a fair number of Oaks, indeed, and it has a Flowering Dogwood, even more showy than that which brightens our eastern woodlands in spring. But, altogether it possesses only one-quarter of the number of species of deciduous trees that the Atlantic forest has; it is even much poorer than Europe in this respect. It is destitute not only of the characteristic trees of the Atlantic side, such as Liriodendron, Magnolia, Asimina, Nyssa, Catalpa, Sassafras, Carya, and the arboreous *Leguminosæ* (Cercis excepted), but it also wants most of the genera which are common throughout all the other northern-temperate floras, having no Lindens, Elms, Mulberries, Celtis, Beech, Chestnut, Hornbeam, and few and small Ashes and Maples. The shrubbery and herbaceous vegetation, although rich and varied, is largely peculiar, especially at the south. At the north we find a fair number of species identical with the eastern; but it is interesting to remark that this region, interposed between the north-east Asiatic and the north-east American and with coast approximate to the former, has few of those peculiar genera which, as I have insisted, witness to a most remarkable connection between two floras so widely sundered geographically. Some of these types, indeed, occur in the intermediate region, rendering the general absence the more noteworthy. And certain peculiar types are represented in single identical species on the coasts of Oregon and Japan, etc., (such as Lysichiton, Fatsia, Glehnia); yet there is less community between these floras than might be expected from their geographical proximity at the north. Of course the high-northern flora is not here in view.

Now if, as I have maintained, the eastern side of North America and the eastern side of northern Asia are the favored heirs of the old boreal flora, and if I have plausibly explained how Europe lost so much of its portion of a common inheritance, it only remains to consider how the western side of North America lost so much more. For that the missing types once existed there, as well as in Europe, has already been indicated in the few fossil explorations that have been made. They have brought to light Magnolias, Elms, Beeches, Chestnuts, a Liquidambar, etc. And living witnesses remain in the two Sequoias of California, whose ancestors, along with Taxodium, which is similarly preserved on the Atlantic side, appear to have formed no small part of the Miocene flora of the arctic regions.

Several causes may have conspired in the destruction; — climatic differences between the two sides of the continent, such as must early have been established (and we know that a difference no greater than the present would be effective); geographical configuration, probably confining the migration to and fro to a long and narrow tract, little wider, perhaps, than that to which it is now restricted; the tremendous outpouring of lava and volcanic ashes just anterior to the Glacial period, by which a large part of the region was thickly covered; and, at length, competition from the Mexican-plateau vegetation, — a vegetation beyond the reach of general glacial movement from the north, and climatically well adapted to the southwestern portion of the United States.

It is now becoming obvious that the Mexican-plateau vegetation is the proximate source of most of the peculiar elements of the Californian flora, as also of the southern Rocky Mountain-region and of the Great Basin between; and that these plants from the south have competed with those from the north on the eastward plains and prairies. It is from this source that are derived not only our *Cacteæ* but our *Mimoseæ*, our Daleas and Petalostemons, our numerous and varied *Onagraceæ*, our *Loasaceæ*, a large part of our *Compositæ*, especially the *Eupatoriaceæ*, *Helianthoideæ*, *Helenioideæ*, and *Mutisiaceæ*, which are so characteristic of the country, the

Asclepiadeæ, the very numerous *Polemoniaceæ*, *Hydrophyllaceæ*, *Eriogoneæ*, and the like.

I had formerly recognized this element in our North American flora; but I have only recently come to apprehend its full significance. With increasing knowledge we may in a good measure discriminate between the descendants of the ancient northern flora and those which come from the highlands of the southwest.

BIOGRAPHICAL SKETCHES.

BROWN AND HUMBOLDT.[1]

Beyond the immediate pale of science, and the circle of its most devoted cultivators, the association of the names of Humboldt and Brown may seem new and strange; — the one a name familiar to the whole civilized world; the other, hardly known to a large portion of his educated countrymen. Yet these names stand together, in the highest place, upon the rolls of almost every Academy of Science in the world; and the common judgment of those competent to pronounce it will undoubtedly be, that although these vacant places upon these honorable rolls may be occupied, they will not be filled, in this, perhaps not in several generations.

Upon the death of Robert Brown, which occurred on the 10th of June last, in his eighty-fifth year, it was remarked that, next to Humboldt, his name adorned the list of a greater number of scientific societies than that of any other naturalist or philosopher. It was Humboldt himself who, many years ago, saluted Brown with the appellation of "Botanicarum facile Princeps"; and the universal consent of botanists recognized and confirmed the title. However the meed of merit in science should be divided between the most profound, and the most active and prolific minds, — between those who divine and those who elaborate, — it will probably be conceded by all that no one since Linnæus has brought such rare sagacity to bear upon the structure, and especially upon the ordinal characters and natural affinities of plants, as did Robert

[1] Proceedings of the American Academy of Arts and Science, iv. 229. (1859.)

Brown. True, he was fortunate in his time and in his opportunities. Men of great genius, happily, often are, or appear to be, through their power of turning opportunities to good account. The whole herbaria of Sir Joseph Banks and the great collections which he himself made around the coast of Australia, in Flinder's expedition, and which he was able to investigate upon the spot in the four years devoted to this exploration, opportunely placed in Brown's able hands as it were the vegetation of a new world, as rich as it was peculiar, — just at the time, too, when the immortal work of Jussieu had begun to be appreciated, and the European and other ordinary forms of vegetation had begun to be understood in their natural relations. The new, various, and singular types which render the botany of New Holland so unlike all other, Mr. Brown had to compare among themselves, — to unravel their intricacies with scarcely a clue to guide him, except that which his own genius enabled him to construct in the process of the research, — and to bring them harmoniously into the general system of botanical natural alliance as then understood, and as he was himself enabled to ascertain and display it. It was the wonderful sagacity and insight which he evinced in these investigations, which, soon after his return from Australia, revealed the master mind in botanical science, and erelong gave him the position of almost unchallenged eminence, which he retained, as if without effort, for more than half a century.

The common observer must wonder at this general recognition, during an era of great names and unequaled activity, of a claim so rarely, and as it were so reluctantly, asserted. For brief and comparatively few — alas! how much fewer than they should have been! — are Mr. Brown's publications. Much the largest of them is the "Prodromus of the Flora of New Holland," issued fifty years ago, which begins upon the one hundred and forty-fifth page, and which stopped short at the end of the first volume. The others are special papers, mostly of small bulk, devoted to the consideration of a particular plant, or a particular group or small collection of plants. But their simple titles seldom foreshow the full import of their contents. Brown delighted to rise from a special case to

high and wide generalizations; and was apt to draw most important and always irresistible conclusions from small, selected data, or particular points of structure, which to ordinary apprehension would appear wholly inadequate to the purpose. He had unequaled skill in finding decisive instances. So all his discoveries, so simply and quietly announced, and all his notes and observations sedulously reduced to the briefest expression, are fertile far beyond the reader's expectation. Cautious to excess, never suggesting a theory until he had thoroughly weighed all the available objections to it, and never propounding a view which he did not know how to prove, perhaps no naturalist ever taught so much in writing so little, or made so few statements that had to be recalled, or even recast; and of no one can there be a stronger regret that he did not publish more.

With this character of mind, and while carefully sounding his way along the deep places of a science the philosophy and ground of which were forming, day by day, under his own and a few contemporary hands, Brown could not have been a voluminous writer. He could never have undertaken a "Systema Regni Vegetabilis," content to do his best at the moment, and take upon trust what he had not the means or the time to verify, — like his contemporary, De Candolle, who may worthily be compared with Brown for genius, and contrasted with him for the enthusiastic devotion which constantly impelled him to publication, and to lifelong, unselected herculean labor, over all the field, for the general good.

Nor could Brown ever be brought to undertake a "Genera Plantarum," like that of Jussieu; although his favorable and leisurely position, his vast knowledge, his keen discrimination, and his most compact mode of expression, especially indicated him for the task. Evidently, his influence upon the progress of botany might have been greater, or at least more immediate and more conspicuous. Yet, rightly to estimate that influence now, we have only to compare the "Genera Plantarum" of Endlicher with that of Jussieu, — separated as they are by the half-century which coincided with Brown's career, — and mark how largely the points of difference between the

two, so far as they represent inquiry, and genuine advancement in the knowledge of floral structure, actually originated with him. Still, after making due allowance for a mind as scrupulous and cautious as it was clear and profound, also for an unusually retiring disposition, which even in authorship seems to have rendered him as sedulous to avoid publicity as most writers are to gain it, it must be acknowledged that his retentiveness was excessive ; and that his guarded published statements sometimes appear as if intended — like the anagrams of the older mathematicians and philosophers — rather to record his knowledge than to reveal it. But this was probably only in appearance, and rather to be attributed to his sensitive regard for entire accuracy, and his extreme dislike of all parade of knowledge, — to the same peculiarity which everywhere led him to condense announcements of great consequence into short paragraphs or foot-notes, and to insert the most important facts in parentheses, which he who runs over the page may read, indeed, but which only the most learned and the most reflecting will be apt to comprehend. In candor it must be said that his long career has left some room for the complaint that he did not feel bound to exert fully and continuously all his matchless gifts in behalf of the science of which he was the most authoritative expositor.

But if thus in some sense unjust to himself and to his high calling, Brown could never be charged with the slightest injustice to any fellow-laborer. He was scrupulously careful, even solicitous, of the rights and claims of others ; and in tracing the history of any discovery in which he had himself borne a part, he was sure to award to each one concerned his full due. If not always communicative, he was kind and considerate to all. To adopt the words of one of his intimate associates, " those who knew him as a man will bear unanimous testimony to the unvarying simplicity, truthfulness, and benevolence of his character," as well as to " the singular uprightness of his judgment."

The remaining and the most illustrious name of all — and one in its wide renown strongly in contrast with the last — has only just now been inscribed on our obituary list.

The telegraph of last week brought to us the painful intelligence that the patriarch of science, the universal Humboldt, died at Berlin on the 6th of May. Born in 1769, a year more prolific in great men than any equal period of all preceding time,[1] Humboldt had, before the end of the eighteenth century, exhibited qualities of the very highest order, and obtained a place of acknowledged celebrity in Europe. This, however, was the mere prelude to his career, for with the close of that century he commenced, with Bonpland, his wonderful exploration of Spanish America, which continued during five years. This journey must be considered in all future time as, substantially, the scientific discovery of Spanish America; and whether we measure its results by the amount of knowledge through the wide fields of astronomy, geography, geology, mineralogy, meteorology, zoölogy, botany, and political economy, or the personal qualities by which this knowledge was collected and reduced to its place in the records of science, we cannot hesitate to rank the expedition amongst the most important and successful ever executed by man.

On his return to Europe, in 1805, Humboldt was employed several years in reducing his immense collection of materials to form for publication. From that time to his death, a period of almost half a century, he resided (except for a short time, in which he made his journey to northern Asia) in Europe, mostly in France and Germany. The last twelve or fifteen years of this great man were principally employed in the production of his "Cosmos," — the crowning labor of his long life, the harvest of his mature wisdom, — a work that could not have been produced by any other man, simply because no other man possessed the treasures, or the key to the treasures, of the various knowledge contained in it.

From his return to Europe to his death, he possessed, indisputably, the first place among philosophers, for the vast ex-

[1] Napoleon, Wellington, Mehemet Ali, Soult, Lannes, Ney, Castlereagh, Chateaubriand, Cuvier, and Humboldt. The name of Metternich is sometimes added to this list, probably incorrectly. That of Canning does not belong here, nor that of Mackintosh, nor of Sir Walter Scott.

tent of his acquirements. Without doubt, at all times during the present century there have been men much greater than Humboldt in each special department of science, but no one to compare with him in the number of subjects in which he had but few superiors, — no one who could, like him, bring all the sciences into one field of view, and compare them as one whole, through their relations and dependences. It was probably this extent of knowledge that led him to generalization rather than particular discovery; to trace connections and relations, rather than to search for new and minute facts or particular laws; to produce the "Cosmos," rather than discover the atomic theory or the cellular formation of organic structures. Many other men have been masters of several specialties; Humboldt alone brought the whole range of the physical and natural sciences into one specialty.

We cannot close this brief notice of the character and career of our illustrious associate without one moment's allusion to his amiable moral nature, his love of justice, and his superiority to all merely personal ends. So strong was his desire to give the influence of his high scientific position to the cause of civilization and the progress of knowledge, by assisting all applicants for his opinion and advice upon scientific subjects, that he permitted a correspondence to be extorted from him which in his last days became a load too great to be borne, and compelled a cry for relief that had hardly subsided when the news of his death reached us.

Such is the faint outline of a man whose name is indelibly written with those who have been most eminent in this wonderful age of scientific activity. The Academy claims the privilege, in common with the learned societies with which he was associated throughout the civilized world, to express its sorrow for his death, and to offer its tribute of honor to his memory.

AUGUSTIN-PYRAMUS DE CANDOLLE.

De Candolle was born at Geneva on the fourth day of February, 1778; he commenced his distinguished career as a botanist in Paris in the later days of the French republic; he continued it at Montpellier until 1816, when he returned to his native Geneva, where he died in September, 1841, — on the fifth day of that month, according to the opening paragraph of his son's preface to this volume,[1] — on the twenty-fifth according to the note by the same excellent authority at the close of the Memoir, p. 489. We cannot account for the discrepancy; but the former is without doubt the true date.

The twenty-one years which have elapsed since his death have thinned the ranks of those who knew De Candolle, either personally or by correspondence. The "Théorie Elémentaire," the "Organographie," and the "Physiologie Végétale" have played their part, and have long ago passed out of general use. Yet, thanks to their influence, but more especially to the "Prodromus," the name of De Candolle is still perhaps the most prominent one with the cultivators of the science in general the world over, — is associated, not indeed with the profoundest depths, but with a larger amount of botany than any other name except that of Linnæus. These are the personal memoirs of an industrious, highly useful, prosperous, and honored life. Begun at middle age, perhaps mainly for the writer's own satisfaction, or that of his family, and continued at considerable intervals down to his last year, and evidently with a growing expectation of future publication, — they have appeared none too soon to secure the most interested but rapidly narrowing circle of readers. The outer circle, however, is as

[1] *Mémoires et Souvenirs* de Augustin-Pyramus De Candolle. *Ecrit par lui-même.* Geneva and Paris, 1862. (American Journal of Science and Arts, 2 ser., xxxv. 1. 1863.)

wide as ever, embracing all the lovers of botany in our day, to none of whom can the name of De Candolle be indifferent. The memoirs portray not so much the botanist as the man. Indeed, the perusal was rather disappointing to us in the former regard. We expected to get fresh glimpses of his mind at work upon the problems of the time, and to watch the rise and development of the ideas which brought him fame. That could be had, however, only from letters, diaries, or other contemporary records: these are only reminiscences. On this account, too, and perhaps because the record was made with only a dim and distant view to publication, the narrative somehow has not all the vivacity and sprightliness, nor the ready flow of language, nor the affluence of anecdote, which those who personally knew the writer would have expected. There are, however, many favorable specimens of De Candolle's powers of delineation, and some amusing anecdotes or interesting recollections of distinguished savans and others.

The family of De Candolle (to retain the style of orthography which is kept up at Geneva, in which the De is written as a substantial part of the name) is an old and noble one in the Provence ; and a branch of it, reaching Naples in the thirteenth century in the suite of the Anjou princes, flourished there, under a name gradually changed from Candola to Caldora, down to the middle of the sixteenth century. Augustin-Pyramus De Candolle derived one of his baptismal names from his ancestor, Pyramus de Candolle, who, becoming protestant, fled from Provence to Geneva in the year 1591, following an uncle who had already been established there for thirty or forty years. Augustin was the name of his father, in his earlier days a Genevan banker, a member of the state council, military syndic, and, about the time of the outbreak of the French Revolution, Premier Syndic of the little republic. Displaced by an earlier *coup d'état* just as he was about to enter upon the duties of this office, he had retired into the country just in time to escape the worst perils of the woful imitation at Geneva of the reign of terror, in July, 1794, although he was condemned to death for contumacy, and his property in the city for a time sequestrated. The rest of his

life was peaceful and long: he attained the age of eighty-four years, and died in 1820.

Augustin-Pyramus, the writer of this autobiography, appears to have been remarkable in his boyhood rather for quickness of learning than for scholarship. His early tastes were for belles-lettres and poetry. Specimens of his poetical productions, both of his youth and of maturer years, are appended to the volume. Of their merit we cannot pretend to judge. At the age of sixteen he happened to attend a few lectures of a short course on botany, given by Vaucher, — who, living to a venerable age, survived his distinguished pupil. Here he learned the names of the parts of the flower, but nothing whatever of classification, having gone into the country for the summer before that portion of the course was reached. But his curiosity was awakened; and in his leisure hours he began to collect, observe, and even to describe the plants he met with in his rambles, at first without any botanical book whatever to guide him, and without any idea beyond that of amusement or relaxation. The next winter, returning to Geneva and to his college studies, he came to know Saussure, then in his last years, and half paralytic. The veteran physicist, while he endeavored to attract the young man to scientific pursuits, discouraged his predilection for botany. That he regarded as quite unworthy of serious attention. Another summer passed upon the side of the Jura, however, and the perusal of Duhamel's "Physique des Arbres," of the "Researches upon Leaves" of Pastor Bonnet (a friend of his father), also of Hale's "Vegetable Statics," which he painfully translated from the English, and finally the acquisition of the "Linné de l'Europe" of Gilibert — in which the Linnæan artificial classification even then annoyed him by its incongruity with the natural relationships which he already recognized: these had by this time fixed his fate before he was at all aware of it; and perhaps had even determined in some sort his characteristics as a botanist.

An unexpected opportunity to pass the ensuing winter in Paris opened the way. This occurred through an invitation from Dolomieu, who, while young De Candolle was herboriz-

ing in the Jura, had been mineralogizing in the Alps, attended by two of De Candolle's school-mates, Picot and Pictet. In the autumn of 1796 the three young men proceeded to Paris, under the auspices of Dolomieu, who secured for De Candolle a lodging immediately over his own apartments, and presented him to Desfontaines and Deleuze at the Jardin des Plantes. No botanical lectures were given at that season of the year; but De Candolle attended the principal scientific courses then in progress; among them those of Fourcroy and Vauquelin upon Chemistry, of Portal and Cuvier upon anatomy, and of Hauy upon mineralogy. It was at this early period that his acquaintance and life-long intimacy with the excellent Delessert family commenced. By a rather ingenious device he contrived to make the acquaintance of Lamarck, but he gained little thereby in the way of botany, Lamarck being just then wholly occupied with the discussion of chemical theories. When De Candolle returned to Geneva in the spring of 1797, Lamarck sent by his hands a volume to Senebier, and so he came to know his amiable countryman, who, in ascertaining the capital fact that plants decompose carbonic acid, may be said to have laid the foundation of modern vegetable physiology. The first genus which De Candolle established (in 1799) was Senebiera.

From his narrative it would appear that, during this summer of 1797, the ambitious young botanist of two years' standing, and only eighteen years old, had not only conceived the idea of writing an elementary work, but actually traced the plan and written some chapters of it! He even states that from this period date the first observations and the conceptions — confused indeed, but correct — of the part which the abortion and the union of organs play in floral structure, — namely, the ideas which principally distinguish the "Théorie Elémentaire," published fifteen years later. How far these ideas were developed, however, we have no means of ascertaining. One would like to see an extract from this early manuscript, in confirmation.

The following winter he began to study law at Geneva. But with the little state now annexed to the great French

Republic, the prospects were not encouraging. A career must be sought elsewhere. De Candolle determined to study medicine, at the same time prosecuting his botanical studies, so as to have a double chance, by falling back upon the former in case the latter should fail to support him.

In this view, he returned to Paris in the spring of 1798, just in time to see his patron Dolomieu set out for Egypt, as one of the savans of that famous expedition, and to decline a pressing invitation to accompany him. Taking a lodging in the Rue Copeau, to be near the Jardin des Plantes, he attended the hospitals and medical lectures, which he disliked, but recompensed himself at the Garden of Plants with the courses of Lacépède, Lamarck, Cuvier, and Hauy, omitting the botanical lectures as not to his mind, but sedulously examining the plants of the Garden. He renewed his acquaintance with Lamarck, at whose request he wrote a few articles (under the letter P) for the "Dictionaire Encyclopedique." Lamarck himself by this time had quite abandoned botany.

It was to Desfontaines that De Candolle was indebted for an immediate opportunity of beginning his botanical career. It came about thus: L'Heritier, who appears to have been wealthy, had engaged Redouté, the celebrated flower-painter, to prepare drawings of all the fleshy plants in cultivation, it being impossible to preserve them well in the herbarium. The artist undertaking to publish these drawings, applied to Desfontaines for a botanist to furnish the descriptive letter-press. The kind Desfontaines recommended De Candolle, and moreover offered to direct him in the work. He freely opened to the young botanist his herbarium and library, and allowed him to study by his side; indeed Desfontaines was his botanical master and fatherly friend. The botanical library of L'Heritier, then much the largest at Paris, was naturally at his service, until the death by assassination, soon afterwards, of its singular owner. De Candolle, thus connecting his name and studies with the work of the unrivaled flower-painter, acquired thereby, as he remarks, more reputation than he deserved, and more instruction than he expected.

In the course of this same summer, of 1798, an invitation

from Alexander Brongniart, the mineralogist (whom De Candolle had slightly known, through Dolomieu, on his first visit to Paris), connected him with a small party of naturalists who made an excursion to Fontainebleau. Besides Dejean, the entomologist, then very young, Cuvier and Dumeril were of the party. In the autumn of the same year he visited Normandy, with less celebrated companions, and formed his first acquaintance with marine vegetation. The next year he made a visit to Holland, to consult the gardens and conservatories of that country, the richest in the "plantes grasses," which then occupied his attention. One result of this journey was that he induced his friend Benjamin Delessert to purchase Burmann's herbarium, and thus to lay the foundation of the important collections and library at the Hotel Delessert which have been so useful to naturalists, and so liberally devoted to their service. During the winter of the following year De Candolle elaborated the "Astragalogia," his first independent work of any considerable consequence, and which was published two years later; in this he found opportunity to dedicate to his friend Delessert the Leguminous genus Lessertia.

About this time, namely, at the beginning of the century, he became acquainted with Mirbel, who had come up to Paris from the south of France, where he had been a pupil of Ramond. Instead of translating De Candolle's remarks, we may as well give them in the original.

"Il [Mirbel] savait alors peu de botanique, mais il annonçait de l'esprit et des talents. Je me liai avec lui. Il venait souvent déjeuner chez moi. Nous causions botanique; j'avais deux ou trois ans d'avance sur lui, et j'étais naturellement communicatif; je lui fis parts de plusieurs idées, nouvelles pour lui, et dont quelques-unes l'étaient pour le science. Elles parurent l'interresser, car j'en retrouvai une grande partie dans les éléments de physiologie qu'il publia peu d'années après; telles sont la distinction des feuilles séminales et primordiales, l'importance de l'étude des nervures principales des feuilles, etc. Appelé à rendre un compte succinct de cette ouvrage dans le 'Bulletin philomathique,' je me divertis à ne citer que les idées que j'avais suggérées à l'auteur: je n'en revindiquai aucune,

et ne sais pas même s'il s'est aperçu de cette petite malice. Je dois dire que je ne prétendis point, même alors, que se fût un plagiat volontaire, mais il arrive souvent dans les sciences qu'on s'appropie, sans s'en douter, ce qu'on a entendu dire.

"Cette circonstance éveilla ma propre attention sur la justice rigoureuse que j'ai désiré rendre a tous : la force de ma mémoire, et surtout le soin que j'ai eu très-jeune de noter les faits et les idées nouvelles que j'entendais dans la conversation, m'ont mis à même de pouvoir, bien des années après une conversation, citer exactement celui de qui j'avais appris un fait ou une opinion quelconque. Cette habitude de justice m'a fait beaucoup d'amis, et j'ai eu souvent des remerciements de gens cités par moi, qui eux-mêmes avaient oublié ce qu'ils m'avaient dit." (pp. 91, 92.)

To De Candolle's credit it must be said, not only that his career was remarkably free from controversies about priority and reclamations, but that his example and precepts, his scrupulous care to render due credit to every contributor, his respect for unpublished names communicated to his own or recorded in other herbaria, and the like, have been most influential in establishing both the law and the ethics which prevail in systematic botany (more fully, or from an earlier period than in the other departments of natural history), and which have secured such general coöperation and harmonious relations among its votaries.

In these early days De Candolle was a good deal occupied with vegetable physiology; the results are contained in his papers "on the pores in the bark of leaves," *i. e.* stomata; on the vegetation of the Mistletoe; and on his experiments relative to the influence of light on certain points, mainly those which exhibit strikingly the change in the position of their leaves at night, which has been called the sleep of plants. The account of these experiments, in which he caused certain plants to acknowledge an artificial night and day, when read before the Institute, gave him considerable eclat, — and probably also the compliment of being named one of the three candidates to fill the vacancy in the Academy of Sciences left by the death of L'Heritier. A mere compliment, for the contest, of course, was between Labillardière and Beauvois. In the

canvass De Candolle called upon Adanson, then very aged, and in his dotage more eccentric than ever.

If not chosen into the Institute, which indeed he could not pretend to expect, De Candolle was in that year made a member of that active association, — "la pépinière de l'Academie des Sciences," — the Société Philomathique, and was soon placed on the committee in charge of its Bulletin. This brought him into intimate connection with such colleagues as Brongniart (Alex.), Duméril, Cuvier, Biot, Lacroix, and Sylvestre.

"We met, at each other's lodgings, on Saturday evenings, after the session of the society, to read and to discuss the *morceaux* intended for the Bulletin, and when our labor was finished we took tea together and chatted familiarly. As one by one we exchanged the celibate for the married state, our wives were introduced; — then we no longer read our extracts, and at length we gave over making the Bulletin, but we kept up our Saturday evening reunions. It was in consequence of this that Cuvier continued long afterwards his Saturday evening receptions; but I return to the year 1800."

By De Candolle's account he was by about ten years the youngest member of this reunion. Yet he has the name of Biot and Duméril on his list, both of whom survived him for twenty years; and Biot was really not quite four years his senior, and Duméril only five.

As a member of this select circle of intimate friends and zealous savans, all then pressing on to the very highest distinction, we may well believe that the ambitious young botanist enjoyed, and improved to the full, such golden opportunities, that he learned something of every branch of natural history, and also — what was no less useful at Paris — "à connaître les hommes et les mobiles cachés de bien des choses."

De Candolle sketches the following portraits of three of his associates, Duméril, Cuvier, and Lacroix. And first of

"The excellent Duméril. He was the ideal of the frank character which we attribute to the Picards. He was a sincere and devoted friend, always ready to second and render any service to me and

mine. No cloud ever threw a shadow over our alliance, which became closer yet when, at a later period, the friendly connection of my wife with the widowed Madame Say determined the latter to marry Duméril. He was chief demonstrator in the anatomical department at the School of Medicine, but he became professor and member of the Academy of Sciences. Duméril was remarkable rather for the clearness of his ideas, and the variety and accuracy of his knowledge in natural history, than for theoretical principles. He was a practical man, whose elementary works had considerable success, but who, after having had a glimpse of some of the laws of organic symmetry, such as the analogy of the skull to vertebræ, seemed to have collapsed before their immensity. His principal services to science were in the way of teaching, and in the encouragement which he so well knew how to give to the young. The heart in this kind of influence is more essential than the head, and although Duméril's judgment was clear and quick, he was much more remarkable for his moral qualities.

"Cuvier, who was from the beginning the intimate friend of Duméril, was entirely different; and it would be difficult to find two people who were less analogous. Born at Montbéliard and brought up at Stuttgart, Cuvier had something of the gravity and even of the obstinacy of the German. Placed for some time in an inferior position, he was forced from his youth to make up for it by the dignity of his manner; but the world of savans, at least, will never forget his sojourn in Normandy, where he made those beautiful investigations on the molluscs which were the beginning of his fame. Called afterwards to the Jardin des Plantes as assistant to the aged Mertrud, he owed this position to the friendship of Geoffroy; but he soon surpassed his patron. In consequence of this position he was a member of the Institute from its foundation, and quickly acquired the reputation which results from great talent united to a skillful ambition. At the time when the office of secretary was annual he foresaw it would become perpetual, and arranged in such a manner as to fill one secretaryship almost continually, either himself or by others; so that he found himself in position to have it without contest when it became permanent and well paid. These first steps being taken, all places fell to him as of themselves, and we saw him successively Professor of the Ecoles Centrales, of the College de France, at the Jardin des Plantes, Inspector, then Councillor, then Chancellor of the University, Councillor of State, Baron, Peer of France, etc., etc. His talent, his aptitude for knowing and doing

everything, made him skillful in every function; he brought to it method, order, facility for administration, a knowledge of details and of the whole, a sincere love of justice, and a disinterestedness which caused him to be noticed and admired.

"Cuvier might justly be compared to Haller, whom he resembled as much as the difference of nation and time would allow. Both astonished by their extraordinary capacity for learning, knowing equally well natural and historical science, greedy of positive facts on all subjects, endowed with wonderful memory and a remarkable spirit of order, capable of great labor, and yet gifted with much facility. But at the side of these admirable qualities it might be observed that neither had an inventive genius; they observed facts well, but never thought to unite them by a theory that would divine or discover others. Their characters corresponded even outside of science: both loved power, and sacrificed precious time to the desire of political advancement; both loved reading to a passion, even at the hours destined ordinarily for meals and domestic intercourse; both were cold and haughty in conversation with those who inspired them with no interest, piquant and profound to those whom they thought worthy of it; finally both had a certain contempt for that class of ideas called liberal, and held to the aristocratic party. The great size of their heads gave them a certain physical resemblance. In one word, it would be difficult to find two celebrated men more exactly alike, and the lovers of metempsychosis might say, if the epochs would permit, that the soul of Haller had passed without change into the body of Cuvier.

"To me personally, Cuvier was wellnigh perfection. . . . Notwithstanding the great difference in our respective views of life and of politics, and even of science in some theoretical matters, our intimacy was never clouded, nor was it disturbed by his quarrel with Geoffroy, although he knew that my opinions inclined toward those of the latter.

"The geometrician Lacroix was a genuine specimen of the philosopher of the eighteenth century, a republican of the school of Condorcet, an enemy to the great and their hangers-on, uniting the gaiety of a child with the moroseness of a disappointed old man, — the ease, grace, and kindness of a warm-hearted gentleman with the gruffness of a grumbler. He was a thoroughly excellent man, but a stranger to the life of the world around him. The character of the misanthrope in Molière, which I supposed purely imaginary, I found completely realized when I knew Lacroix."

An episode of fifteen days, during which De Candolle, to his great surprise, had political functions to perform, — being appointed one of the three notables of the department of the Léman, in a representation of all the departments of the French Republic, which the First Consul called together, — gives us the first glimpse of Bonaparte in this narrative ; and De Candolle's account of the interviews with him, and with his minister of police, Fouché, is well worth preserving. With this transient exception, we have only the most incidental allusions to public affairs during the eventful years of the Consulate, the Empire, and the Restoration.

We pass by, also, the interesting account which De Candolle gives of the doings of Delessert and himself, in the establishment and administration of the Philanthropic Society, which grew out of the introduction by them of Count Rumford's economical soups, distributed to the poor. These honorable undertakings brought the two friends into relations with Rumford himself when he came to reside at Paris. Indeed Delessert, as we have had occasion to learn, became one of Count Rumford's executors. The admiration with which Rumford's writings and economical inventions had inspired the two young philanthropists was much diminished upon personal acquaintance.

" It was after his plans," writes De Candolle, "that we had constructed our furnaces, after his receipts that we made our soups, upon his advice that we were induced to substitute such assistance for gifts of money."

So when Rumford was expected at Paris, they congratulated themselves upon such an acquisition, went to meet him on his arrival, and brought him to dine with them.

" We found him a dry, methodical man, who spoke of benevolence as a discipline, and of the poor as we should not have dared to speak of vagabonds. It is necessary, said he, to punish those who give alms ; the poor must be forced to work, etc., etc. Great was our astonishment at hearing such maxims : however, we did our utmost to profit by his advice in practical matters. I had a good deal of intercourse with him, one among others odd enough. Mademoiselle Rath, a Genevese painter, and like ourselves enthusiastic about

Rumford, wished to paint his portrait to be engraved. M. Jay, her relation and my friend, then director of the 'Décade Philosophique,' wished to put it into his journal, and asked me for a notice of M. Rumford to accompany it. Knowing little of his former life, I asked M. Rumford himself for a few notes; he promised them, and appointed an interview at his house to give them to me. I went: what was my astonishment when he presented an article entirely complete and quite eulogistic. That was not all; he required me to copy it on the spot, not wishing to leave the manuscript in his writing in my hands. I thought the proceeding rather indelicate, and the distrust not very polite. I deferred however to the wishes of a man for whom I had always had until then the highest respect; I obeyed: I transmitted to the 'Décade' the written article, with small additions, and I have never mentioned until after the death of Rumford, not even until now, the secret of its origin, thinking that this trait would not raise him in estimation.

"M. Rumford settled in Paris, where he afterwards married Madame Lavoisier, the widow of the celebrated chemist. I saw something of both, and I never knew an odder union. M. Rumford was cold, imperturbable, obstinate, egotistical, prodigiously occupied with the material part of life, and in inventions in the smallest matters. He was engrossed with chimneys, lamps, coffee-pots, and windows made after a peculiar fashion; and he contradicted his wife twenty times a day about the management of her housekeeping. Madame Lavoisier-Rumford . . . was a woman of very decided character. A widow for twelve or fifteen years, she had been in the habit of having her own way, and did not like to be contradicted. Her mind was broad, her will strong, her character masculine. She was capable of lasting friendship, and I could always congratulate myself on her kindness to me. Her second marriage was soon disturbed by grotesque scenes. Separation was better for both than union. He got a pension, which he needed, but which death prevented his long enjoying. She obtained liberty and the title of Countess: both were satisfied. He could now arrange the house at Auteuil as he liked: she continued to receive a select circle at hers."

Of this racy and unflattering sketch we have only to remark that, however it may have been as to the pension, Rumford's pecuniary means, as shown by his endowments and legacies in this country, were more considerable than De Candolle supposed.

Apropos to reminiscences of distinguished savans, we look forward a year or two in the narrative, and select the following. And first, of a person who was well known to a past generation, and to some who still survive, at Philadelphia.

"Joseph Correa de Serra was then about fifty-five or sixty years old. He was of an ancient family in Portugal, which had produced several literary men. After studying at the University of Coimbra he was transferred to Rome, where he pursued theological studies for a dozen years at the College of the Sapienza, but which he left with a knowledge of many things beside theology. Returning to Portugal, he was made governor to the hereditary Prince, Secretary to the Academy of Sciences, etc., and became a very influential person, both on account of his talents and on account of the position of his pupil, who it was supposed would become king on attaining his majority, as his mother was only regent. Correa was made Minister; and his first act was to overthrow the Inquisition. But the Prince died just as he was coming of age, and Correa was left exposed to the hatred and jealousy of the priests. After a while he obtained permission to go to England, where he lived in the society of the savans of which Sir Joseph Banks' house was the centre. Afterwards he moved to Paris, where he also lived among savans and men of letters, and where he showed the most noble character when the seizure of Portugal by Bonaparte deprived him of all his resources. He possessed the singular faculty of knowing everything apparently without labor. It is only the people of the south who can thus combine great facility with profound idleness. The latter prevented his publishing anything beyond small dissertations, quite below his talents; but in conversation all his various knowledge and his ingenious views were charmingly exhibited. In these days Humboldt and Cuvier often came to my lodgings, where they occasionally met Correa. Although their celebrity was far above his, and justly so, on account of their published works, yet Correa always got the advantage over them; and it was by no means the least of the enjoyments of our sociable little dinners to see the sort of deference, and even fear, which Cuvier and Humboldt exhibited in the announcement of their opinions before Correa, who, with the grace and sly maliciousness of a cat, would at once expose their weak sides. Like them, he was familiar with all the historical and natural sciences, and he used his vast stores of knowledge with a severe logic and rare sagacity. He spent many hours in my herbarium; where

the subtle perspicacity which he brought to bear at a glance upon plants, often wholly new to him, taught me much of the art of observing, and especially of combining observations in botany. To such talents he joined a lofty soul and a heart devoted to friendship. It was a great grief to me when, at over sixty years of age, he quitted Europe to rejoin in Brazil the king who had persecuted him; but he forgot all his wrongs when his sovereign became unfortunate. Correa died when ambassador to the United States."

The following, of a somewhat later period, is abridged from De Candolle's account of the Société d'Arcueil: —

"Its founder was the excellent and illustrious Berthollet, who, then living in his country residence at Arcueil, . . . invited thither, once a month, a few young savans, by way of encouraging their efforts. His colleagues MM. de la Place and Chaptal, also senators and members of the Institute, were, so to say, vice-presidents of this little reunion. Humboldt also had a place, and the parterre was composed of Biot, Thénard, Gay-Lussac, Descotils, Malus, Amédée Berthollet, and myself. Later, Berard and François de la Roche were admitted. [And finally Arago, Poisson, and Dulong, adds the editor, who notes that the last volume of the "Mémoires d'Arcueil" was published in 1817.] The association was devoted to the physical and chemical sciences. I was admitted in view of the applications of vegetable physiology to chemistry; and I contributed some articles upon this subject to the 'Mémoires d'Arcueil,' namely, my Note on the cause of the direction of stems towards the light, my Memoir on the influence of absolute height upon vegetation, and upon the geographical or topographical distribution of plants, and, later, one upon double flowers, especially of the *Ranunculaceæ*. The first of these writings was a simple and clear solution [although an incorrect one, as it proves. — Eds.] of a problem which was deemed insoluble; the second reduced to just proportions the exaggerations of Humboldt upon the influence of elevation; the third was an essay connected with the observations of the degenerescence of organs, to which my 'Théorie Elémentaire' was devoted. . . .

"We commonly made our rendezvous at Thénard's, and went together to Arcueil, as happy with this run into the country as schoolboys out for a holiday. We walked about in this pleasant villa, and relished the society of our leaders. Nothing can fully describe the good-nature and simplicity of M. Berthollet and even of Madame. They were with us as parents with their children, and

we made ourselves at home in the house with perfect *abandon*. M. Berthollet was quite fat and very full-blooded. He feared heat so much that he wore clothes only out of respect to society, and at night he slept entirely uncovered upon his bed. 'What,' said we, 'even in winter?' 'Oh,' he answered, 'when it is very cold I spread my pocket-handkerchief over my feet.' This man, so high in social rank and scientific celebrity, bore contradiction unusually well, and loved above all things truth. When the first works of Berzelius upon definite proportions became known at Paris, I was very much taken with them, and although they were in direct opposition to the principles of statical chemistry he sustained, I did not fear to tell M. Berthollet the high opinion I had of them. Far from taking offense at this preference, he encouraged me to study the writings of Berzelius.

"M. de la Place was of quite a different character. He had the dryness of a geometrician and the haughtiness of a parvenu. Over and above these defects of manner, he was a man of honor and worth. . . . He often seconded me, although in truth he thought very little of natural history. In our meetings he often had little quarrels with M. Berthollet, and would think to silence him by saying. 'But you see, M. Berthollet, what I say to you is mathematics.' 'Eh, par Dieu, what I say to you is physics,' answered the other, 'and that is quite as good.' . . . Humboldt also came from time to time; but he added much of life and interest when he appeared. He affected to pass himself as the creator of the science of botanical geography, to which he has only added certain facts, and the exaggeration of a true theory so as to render it almost false. He never quite pardoned me for having, in the preface to my memoir on the geography of the plants of France, cited those who before him had occupied themselves with geographical botany, although in this exposition I had, in truth, much amplified his share.

"Among the other members of the society of whom I have not yet spoken, I would chiefly mention Thénard, who was then commencing a career which has since become very brilliant. His activity, his ardor, and his uprightness pleased me very much. . . . I could draw, in an anecdote, the contrast between the characters of Thénard and Descotils. . . . It was then very difficult to correspond with England, on account of the continental blockade. I happened to be the first to receive, by a letter from Dr. Marcet, the news of Davy's great discovery in decomposing the fixed alkalies. By a happy chance, it reached me on the morning of the day of our meet-

ing. I hastened to our usual rendezvous, and could not wait for the session to impart so important a discovery. I read my letter to the members present. Thénard was enthusiastic; he ran about the room like a madman, crying out, 'It is beautiful! it is admirable!' Then turning to me, and laying hold of his arm: 'Look here,' said he, 'I would give this arm to have made this discovery.' Descotils, tranquilly buried in an arm-chair, said also, but in quite another tone: 'It is very fine; but I would not give the end of my little finger to have made it.'"

We pass over all De Candolle's account of his life and domestic affairs during his residence at Paris, his particular investigations, his excursions, in Switzerland and elsewhere, — even the memorable one in the Jura with Biot and Bonpland. in which he led the party into a position of imminent danger, causing Bonpland to bemoan his hard fate in having to perish on such a mole-hill as the Jura, after having safely climbed Chimborazo (p. 154); — his engagement and marriage (the latter in April, 1802), with Mademoiselle Torras, of a Genevan family resident in Paris; of the foundation of his herbarium by the fortunate acquisition of that of L'Heritier; — of the first course of lectures which he gave, at the Collége de France, as a substitute for Cuvier, during the temporary absence of the latter, giving a course of vegetable physiology in place of one on general natural history; — how he prepared to take the degree of M. D. in order to qualify himself as a candidate for the chair of medical natural history at the School of Medicine, then vacant; but how Richard, who disliked him because he was a pupil of Desfontaines, as De Candolle says, instigated Jussieu to offer himself for this chair, upon which, of course, De Candolle withdrew, but nevertheless wrote and sustained as a thesis for the doctorate, his Essay on the Medical Properties of Plants, compared with their exterior forms and their natural classification. He bore his examination creditably, received his diploma, and, the same evening, a private mock inauguration, which, considering the parties engaged in it, must have been irresistibly comical.

"Duméril invited to his house my family, my comrades of the 'Bulletin Philomathique,' and even some of the Professors of the

Ecole de Medicine. This grave assembly amused themselves in giving me the reception, in full dress, from the 'Malade imaginaire.' It was a curious sight to see Cuvier, Lacroix, Biot, and other learned Academicians rehearsing the scene from Molière in the costumes of the Comédie Française. They had smothered me in an immense sugar-loaf paper cap ornamented all over with little lamps all alight. In the motion of bowing I constantly expected to be set on fire. But the acolyte who conducted me would then press a sponge well filled with water borne on the top of the cap, and the water ran down, not upon the lamps, but upon my head, — the audience laughing uproariously at my surprise."

Let us pass on to more serious matters, and rapidly sketch the outlines of the scientific career now fairly and promisingly opening. For the event which fixed De Candolle in his true field of labor was his arrangement (in 1802) with Lamarck — who had long since abandoned botany — to prepare a new edition of the "Flore Française." The arrangement was a favorable one to De Candolle, both financially and scientifically. The new edition was of course an entirely new work, one particularly adapted to De Candolle's genius, and which gave him at once a wide reputation. Indirectly this work gave origin to the botanical explorations of the provinces of France, under the auspices of the government, which engaged much of De Candolle's attention from the summer of 1806 until he ceased to be a French subject.

And now, the death of old Adanson left a vacancy in the botanical section of the Institute, which De Candolle might hope to fill. But parties and personal dislikes, as it appears, were not unknown nor uninfluential in the Paris of half a century ago. Indeed De Candolle (let us hope without sufficient grounds) roundly charges lamentable weakness to Lamarck, and less creditable motives to Fourcroy and even to Jussieu, in respect to the nomination and canvass; while of the Abbé Hauy he relates, to his credit, that, upon being approached with the suggestion that his conscience should prevent his voting for a protestant, he replied that he was very glad of an opportunity to show that he never mixed up religious opinions with scientific judgments. Palisot de Beauvois,

the rival candidate, was elected, in spite of the hearty support De Candolle received from his comrades of the "Bulletin Philomathique" and his eminent associates of the Société d'Arcueil, Berthollet, Chaptal, La Place, Cuvier, etc., — to say nothing of his scientific superiority over his rival, which De Candolle naturally regarded as very great. At that time, according to De Candolle, Beauvois had produced, "ni la 'Flore d'Oware,' ni le 'Prodrome de l'Ethéogamie,' ni en un mot aucun de ses ouvrages qui," etc. But in this De Candolle's memory was perhaps at fault; for, while this election took place in the autumn of 1806, the latter of these works of Beauvois, according to Pritzel, was published in 1805, and the first volume of the former in 1804.

Evidently the disappointment was keenly felt. Membership in the Institute secured not only an assured position but also a comfortable little annuity. This, and the prospective needs of an increasing family, disposed De Candolle to look elsewhere, and to accept, after some hesitation, the botanical chair at the University of Montpellier, which in 1807 became vacant by the death of Broussonet. Hardly was he established there when the death of Ventenat, in the autumn of 1808, made him again a candidate for a seat in the Institute; — again an unsuccessful one, but now chiefly because a considerable number of his particular friends in the Institute required a promise that if chosen he would reside at Paris, which he could not with propriety give. So they voted for Mirbel; — and De Candolle took root at Montpellier, where he flourished from 1808 to the year 1816.

That De Candolle, full of ambition and with a good opinion of his abilities, should have disliked to give up Paris is natural; but he himself afterwards records the opinion (which we share) that his removal from the metropolis was the best thing for him, as enabling him to accomplish more for botany. And as to the honors of the Institute, his disappointments were more than made up to him in the sequel by his election as one of the eight foreign associates of the Academy of Sciences.

At Montpellier, De Candolle was heartily welcomed by his

colleagues, by the official personages, and by the protestant society of the city, — in those days there was little social intercourse between catholics and protestants in the south of France, — and he gave himself with ardor and success to his new duties. He renovated the botanic garden, — the oldest in France, founded by Henry IV., — and secured additional funds for its support. He built up the botanical school, and developed peculiar talents as an instructor, — with results perhaps up to the average as respects the making of botanists; but Dunal, one of his earliest pupils, was about the only one at Montpellier who achieved a general reputation, and his fell much below expectations. He continued and extended his official botanical explorations of the provinces of France, making annual reports to the Minister of the Interior, and planning a very comprehensive work on the "Statique Végétale de la France," which, however, owing to political and other changes, was never written. He wrote and published the "Théorie Elémentaire" which made his reputation as a theoretical botanist, and well exemplifies the characteristics of his genius in this regard, — constructive rather than critical, — quick and ingenious in seizing analogies and in framing hypotheses, rather than sagacious in testing their validity, — content with an hypothesis which neatly connects observed facts, but not so solicitous to prove it actually true, nor urgent to follow it out to ultimate conclusions, — a lucid expositor, and a happy diviner within a certain reach, rather than a profound investigator, — in short, a generalizer rather than an analyzer.[1]

At Montpellier, also, De Candolle planned his "Systema Vegetabilium" — a systematic and detailed account of all known plants, arranged under their natural families, — and

[1] It is curious that De Candolle, who early took to the ideas of Geoffroy in anatomy who founded his morphology of the flower upon the idea of symmetry, and recognized the homology of the floral organs with leaves, and who could have got from the writings of his townsman, Bonnet, enough of phyllotaxy for the purpose, seems never to have thought of connecting the one with the other, nor to have asked himself why a flower is symmetrical.

he there prepared the first volume of this work; thus, with characteristic ardor and courage, but without calculating its immensity, entering upon the grand and most important undertaking of his life, and into that field of labor in systematic and descriptive botany for which he was eminently adapted, by his enterprising disposition and unflagging industry, his capacity for sustained labor, his excellent memory, his spirit of order and method, his quickness of eye, and his great aptitude for generalization.

The overthrow of the Empire, the Restoration, the Hundred Days, and the final fall of Napoleon supervened. De Candolle's life at Montpellier was troubled and his prospects precarious. He naturally turned to his native Geneva, where he had kept up intimate social relations; and when he had ascertained that a place would be provided for him, he exchanged the comparatively ample emoluments of the chair at Montpellier for the very humble salary of one at Geneva, encumbered with the duty of lecturing upon zoölogy as well as botany.

Pending the change he made a visit to England, in 1816, of which a detailed account is given, with reminiscences of the botanists and others whose personal acquaintance he then made. We regret that we have no room left for further extracts: his account of Brown is expressive of the great respect he entertained for him, and that of Salisbury and of Lambert is amusing.

Settled now at Geneva, at the good working age of thirty-eight, the narrative of his steadily industrious and prosperous life, and of his happy surroundings, flows on for nearly two hundred pages, down to the sad overthrow of his health by an overdose of iodine in 1836, his partial convalescence and resumption of botanical work in 1837, and ends with the record of the death of his only brother, at the beginning of the year 1841, only eight months before his own.

These twenty-five years witnessed the publication of the two volumes of the "Systema"; the change of plan to a "Species Plantarum" in a restricted form, more nearly within the limits of a mortal's life and powers; the publica-

tion of the "Organographie" and of the "Physiologie Végétale," and — not to mention a hundred other botanical and sundry miscellaneous writings, of greater or smaller extent — of seven out of the present fifteen volumes of the "Prodromus." Only one botanist of the present century — and one happily who still survives — has accomplished an equal amount of work, and good work, in systematic botany.

Our account has run on to such a length that we cannot touch upon De Candolle's social and domestic life — of which the memoirs reveal pleasant glimpses, nor of his useful and honorable life as a Genevan and Swiss citizen. Nor can we now venture to gather interesting anecdotes from his notices of friends, visitors, pupils, and collaborators; nor notice his methods of working, and his capital arrangements for securing and classifying details and economizing time.

It is not for us to pronounce upon De Candolle's relative rank in the hierarchy of naturalists. He incidentally once speaks of Brown and himself as rivals for the botanical sceptre. It is natural that they should be compared, or rather contrasted; for they were the complements of each other in almost every respect. The fusion of the two would have made a perfect botanist. But De Candolle's facility for generalization, zeal, and industry were as much above, as his depth of insight and analytical power were below Brown's. The one longed, the other loathed, to bring forth all he knew. The editor compares De Candolle's traits of character with those of Linnæus, as delineated by Fabricius, and finds much resemblance. But his impress upon the science, however broad and good, can hardly be compared with that of Linnæus.

BENJAMIN D. GREENE.[1]

Benjamin D. Greene, Esq., of Boston, died on the 14th of October last, at the age of sixty-nine years. He was born in 1793, and graduated at Harvard University in the year 1812. He first pursued legal studies, partly in the then celebrated school at Litchfield, Connecticut, and was duly admitted to the bar in Boston. He then took up the study of medicine, and completed his medical course in the medical schools of Scotland and Paris, taking his medical degree at Edinburgh in the year 1821. The large advantages of such a training having been enjoyed, Mr. Greene did not engage in the practice of either profession. An ample inheritance, which rendered professional exertion unnecessary, conspiring with a remarkably quiet and contemplative disposition and a refined taste, led him to devote his time to literary culture and to scientific pursuits. His fondness for botany, which early developed, was stimulated by personal intercourse with various European botanists, and especially with his surviving friend, the now venerable Sir William Hooker, then professor at the University of Glasgow, to whom he naturally became much attached, and by whom he was highly appreciated.

In botany, as in everything else, Mr. Greene sought to be silently useful. He never himself published any of his discoveries or observations. The few species to which his name is annexed were given to the world at second hand. But his collections were extensive, his original observations numerous and accurate, and both were freely placed at the disposal of working botanists. He early saw that the great obstacles to the advantageous prosecution of botanical investigation in this country, and especially in New England, were the want of books and the want of authentic collections; and these

[1] American Journal of Science and Arts, 2 ser., xxxv. 449. (1863.)

desiderata he endeavored, so far as he could, to supply. He gathered a choice botanical library, he encouraged explorations, and he subscribed to all the large purchasable North American collections, — beginning with those of Drummond in the southern United States and in the then Mexican province of Texas. These being distributed under numbers, among the principal herbaria of the world, and named or referred to in monographs or other botanical works, were of prime importance as standards of comparison. Such collections and such books as Mr. Greene brought together were just the apparatus most needed at that time in this country; and now when our wants are somewhat better supplied, we should not forget the essential service which they have rendered, nor the disinterested kindness with which their most amiable and excellent owner always placed them at the disposal of those who could advantageously use them. Mr. Greene's botanical library and collections have been, by gift and bequest, consigned to the Boston Society of Natural History, of which he was one of the founders and the first president, and by which they will be preserved for the benefit of future New England botanists, by whom his memory should ever be gratefully cherished. The genus Greenea, established by Wight and Arnott upon two rare Rubiaceous shrubs of India, barely anticipated a similar dedication by his old friend Mr. Nuttall, of a curious Grass of Arkansas and Texas, and will perpetuate his name in the annals of the science which he lovingly cultivated.

CHARLES WILKINS SHORT.[1]

Dr. Charles Wilkins Short died at his residence at Louisville, Kentucky, on the 7th of March last, in the sixty-ninth year of his age. He was born in Woodford County of that State, on October 6, 1794, was educated in the school of Mr. Joshua Frye, near Danville, — a distinguished teacher of those days, — pursued his professional studies mainly in Philadelphia, where he took the degree of M. D. from the University of Pennsylvania in the year 1815. For ten years he devoted himself to the practice of medicine, until in the year 1825 he was called to the chair of Materia Medica and Medical Botany in the Transylvania University at Lexington, where he contributed to the reputation of that celebrated school. Relinquishing medical practice, for which he had no liking, he devoted his powers with zeal and success to the more congenial duties of his professorship, and to the cultivation of botany, the favorite pursuit of his life. At the close of the year 1838 he removed, along with some of his distinguished colleagues, to Louisville, filling the same chair in the University of that city until 1849, when he retired from public functions. The remainder of his honorable life was passed at Hayfield, at his tasteful residence near Louisville, in the bosom of his family; in the exercise of kindly but unostentatious hospitality and of all good offices; in quietly enjoying and in causing others to enjoy the blessings of a handsome fortune, to which by inheritance, combined with the fruits of his own industry, he now attained, and in the cultivation of the "amiable science" to which he was devotedly attached.

Dr. Short's botanical publications were neither large nor many. They were chiefly articles contributed to the "Tran-

[1] American Journal of Science and Arts, 2 ser., xxxv. 451. (1863.)

sylvania Journal of Medicine," etc., of which he was for some time one of the editors. The most important is his Catalogue of the plants of his native state (which he widely and assiduously explored), and several supplements; with well considered characters of some new species, and acute and discriminating notes upon several imperfectly known plants. These and the copious manuscript observations which he was for many years accustomed to communicate to his botanical correspondents, showed what he was capable of accomplishing, had not a most retiring and unambitious disposition unduly limited his exertion. It was not activity or persevering labor, but publicity, that he shrunk from. He was a very industrious botanist, and an effectual promoter of our science in this country. His great usefulness in this field was mainly owing to the extent and the particular excellence of his personal collections, and to the generous profusion with which he distributed them far and wide among his fellow-laborers in this and other lands. He and the late Mr. Oakes — the one in the west and the other in the east, but independently — were the first in this country to prepare on an ample scale dried specimens of uniform and superlative excellence and beauty, and in lavish abundance for the purpose of supplying all who could need them. Dr. Short's disinterested activity in these respects has enriched almost every considerable herbarium both at home and abroad, and set an example which has produced large and good results among us. The vast improvement in the character of the dried specimens now generally made by our botanists may be mainly traced to the example and influence of Dr. Short and Mr. Oakes. As might be expected, Dr. Short's own herbarium is a model of taste and neatness. It is also large and important. To one himself so solicitous "to do good and to communicate," contributions from numerous sources naturally flowed abundantly. He, moreover, subscribed to all the North American distributed collections within his reach, and he set on foot or efficiently furthered several distant or difficult botanical explorations. He purchased, at a liberal price, the important botanical collections of Texas and northern Mexico, left by Berlandier, which Lieutenant (now General) Couch acquired of his widow and

sent on to Washington; and, retaining one set for his own herbarium, he caused the rest to be distributed among the botanists to whom they would be most useful, — especially including two Swiss botanists who had contributed to send out Berlandier to Mexico as a collector, but from whom (apparently through Berlandier's dishonesty) they had failed to receive any adequate return. It is understood that Dr. Short's rich herbarium — to which a lifetime of thoughtful attention and much expense were lovingly devoted — is now offered, by a wise bequest, to the custody of the Smithsonian Institution, under instructions that it shall be permanently well cared for and always open to be consulted by botanists. It will there form an excellent and conspicuous nucleus for a collection of American herbaria, such as our science needs, and the country ought to possess.

The natural effects upon his scientific career of a fastidious taste, an unwarrantable diffidence, and a too retiring disposition, were enhanced by a constitutional tendency to depression of spirits. But this never obscured the native kindness of his heart, nor the real though so quiet geniality of his disposition, nor checked an unobtrusive and considerate benevolence. With an uncompromising sense of right and justice, and a keen hatred of everything mean and unworthy, he was never harsh or even cynical. All who knew him well, and also his more intimate correspondents who never enjoyed the privilege of a personal acquaintance, can testify to the nobility and Christian excellence of his character. An appreciative tribute to his memory from the pen of a former colleague will be found in the "Louisville Journal," issued a few days after Dr. Short's lamented death.

Two or three species of Kentucky plants commemorate the name of Dr. Short as their discoverer. Also a new genus Shortia, inhabiting the Alleghany Mountains, was dedicated by him to the present writer. But, alas! too like the botanist for whom it was named, it is so retiring in its habits that it is not known as it ought to be, but lives as yet unseen, except by a single botanist of a former generation, in some secluded recess of the Black Mountain of North Carolina. It will some day be found again and appreciated.

FRANCIS BOOTT.[1]

Francis Boott, M. D., died at his residence in London on Christmas morning, in the seventy-first year of his age. He was born in Boston, on the 26th of September, 1792. His father, Kirk Boott, came to this country early in life, from Derbyshire, England, became a successful merchant in Boston, was one of the pioneers of manufacturing enterprise here, and one of the founders of Lowell, — the type, if not wholly the original of manufacturing towns. His Boston residence was on the site now occupied by the Revere House, of which the Boott mansion forms a part. Francis Boott entered Harvard University in the year 1806, and took his bachelor's degree in 1810. A year after, being then in his nineteenth year, namely, in the summer of 1811, he sailed for England, intending to enter a counting-room in Liverpool, as a preparation for mercantile life. This plan, however, was soon relinquished; and the three succeeding years were mainly spent with his relatives and their friends near Derby, where he made the acquaintance of Mrs. Hardcastle, his future mother-in-law, who was something of a botanist, and where he formed both the scientific and social attachments which determined the aims and secured the happiness of his whole after life. Returning to Boston at the close of the year 1814, he engaged with enthusiasm in botanical pursuits, and amassed a good collection of New England plants. In the summer of 1816 he took a leading part in a botanical exploration of the mountains of New England, ascending in the course of one journey, Wachusett, Monadnock, Ascutney, and Mount Washington; and later in the season Dr. Boott with his brother visited and ascended Moosehillock. His companions in the extended and then formidable tour which culmi-

[1] American Journal of Science and Arts, 2 ser., xxxvii. 288. (1864.)

nated in the White Mountains — then to be reached only by a laborious journey of two days on foot — were Francis C. Gray, Judge Shaw, Nathaniel Tucker, and Dr. Jacob Bigelow, the Nestor of New England botany, now the sole surviving member of the party. An interesting account of the ascent of Mount Washington, written by Dr. Bigelow, was published at the time in the fifth volume of the "New England Journal of Medicine and Surgery."

In the year 1820 Dr. Boott crossed the Atlantic for the last time, and proceeding to London entered upon the study of medicine, under the direction of the late Dr. Armstrong. He continued his medical studies at the University of Edinburgh, where he took the degree of M. D. in 1824. The next year he established himself in London, we believe in the very house in Gower Street where he resided until the day of his death. He was soon associated with his near friend and former teacher in the work of instruction, becoming Lecturer on Botany in the Webb Street School of Medicine, where Dr. Armstrong was Professor of Materia Medica.

"His lectures are said to have been admirable, both in matter and style, and to have excited much enthusiasm; whilst his untiring efforts to promote the welfare of his pupils in other ways were so deeply and generally felt, that, on the eve of his too early withdrawal from the lectureship, they in one day raised a large subscription to present to their friend and teacher; — a tribute which, with the characteristic modesty and consideration, was declined as soon as heard of. He was, however, afterwards persuaded to accept a collection of books instead, in remembrance of their grateful feelings and good will."

The early death of Dr. Armstrong, cutting short a distinguished career, imposed upon his friend the duties of a biographer and expositor. Accordingly, after much preparation, Dr. Boott, in the year 1834, published two octavo volumes, entitled, "Memoir of the Life and Medical Opinions of John Armstrong, M. D.; to which is added an Inquiry into the facts connected with those forms of Fever attributed to Malaria and Marsh Effluvium." He published, besides, in

the year 1827, two introductory Lectures on Materia Medica, which gave a good idea of his excellence as a teacher. Although he did not continue in this career, his interest in medical and scientific education never abated. He was an active promoter of the establishment of London University (now University College), and was for more than a quarter of a century an influential member of its senate and council. He was successfully engaged for some time in medical practice, and was for many years Physician to the American Embassy; but he gradually withdrew from professional cares and toils to more congenial literary and scientific pursuits. As early as the year 1819 he had become a Fellow of the Linnæan Society of London; and afterwards, for the last twenty-five years, he gave it continuous and invaluable service as secretary, treasurer, or vice-president.

At one time it was thought that Dr. Boott might be recalled to his native country and to an active scientific life. Nearly thirty years ago he was offered the chair of natural history in Harvard University, — a chair which had remained vacant since the death of Professor Peck in 1822, although its duties were for several years fulfilled by the late Mr. Nuttall. After Nuttall left Cambridge to explore Oregon and California, arrangements were made to endow the vacant professorship properly in case Dr. Boott would accept the place. Although the offer was declined, we have been told that he intimated a willingness to accept it if the chair were simply that of botany; and when informed that he might practically make it so, although the title was unchangeable, he insisted that he would not be called a professor of natural history, while he could pretend to a knowledge only of botany.

Nearly thirty years ago, Dr. Boott began seriously to devote his energies to the special work upon which his scientific reputation mainly rests, namely, to the study of the vast and intricate genus Carex. The first result of these studies appeared in his elaboration of the Carices of British North America in Sir William Hooker's "Flora Boreali-Americana," published in 1840. Other papers upon Carices were contributed to the "Transactions of the Linnæan Society,"

the "Journal of the Boston Society of Natural History," etc. As it had always been the greatest pleasure, we might say the business of his life to assist others, so now friends and correspondents from all parts of the world hastened to place in his hands the fullest sets of their collections in this difficult genus; and he was able to study, in the unrivaled caricological collection he thus formed, and the various public and private herbaria to which he had access, almost all of the six hundred or more species which the genus was computed by him to comprise, to compare them in numerous specimens of their various forms, and to examine them, group after group, with untiring and closest scrutiny. At length, early in the year 1858, he gave to the world (literally gave to the world) the first volume of his great work, entitled "Illustrations of the Genus Carex," a folio volume with two hundred plates, admirably representing about that number of species. A very large proportion of them were North American species, in which he naturally always took a special interest. In the letter of dedication of this work to his friend, John Amory Lowell, Esq., of Boston, Dr. Boott states that his original design "was limited to the Carices of North America," but that the large collections brought by Dr. Hooker from the East Indies, and placed in his hands for study, caused him to extend his plan, and to endeavor to illustrate the genus at large. With characteristic modesty he makes no allusion to the years of labor and the large amount of money (savings from a moderate income by a simple mode of life) which the volume had cost him; the drawings, engravings, and letter-press having been produced at his sole individual expense, and the larger part of the copies freely given away. Nor did he put forth any promise to continue the work. But in 1860 Part Second quietly appeared, without a word of preface. This contains 110 plates. Two years after, this was followed by Part Third, with 100 plates, making 410 in all; and it is understood that the materials of a fourth volume are left in such forwardness that they may perhaps be published by his surviving family.

Our own estimate of this work has been recorded in the pages of this Journal, as the successive volumes were received.

This motto, which the author placed upon his title-pages, —

> " The man who labors and digests things most,
> Will be much apter to despair than boast,"

is felicitously expressive both of the endless difficulties of the subject, and of his undervaluation of his endeavors to overcome them. A most competent judge briefly declares that, —

" This work is certainly one of the most munificent contributions ever made to scientific botany, besides being one of the most accurate ; on which account it certainly entitles its author to take a much higher place amongst botanists than that of an amateur, which was all that his modesty would allow him to lay claim to."

Dr. Boott's health, which had long been delicate, was much shattered in the winter of 1839–40 by a dangerous attack of pneumonia. " From this time he had repeated slight attacks ; but no alarming symptoms occurred till June, 1863, when the remaining lung gave way, and from that time he never fairly rallied. He died at his residence, 24 Gower Street, on Christmas day, retaining to the last his faculties and all the characteristics of his most admirable life."

Dr. Boott was a man of singular purity, delicacy, and goodness of character, and of the most affectionate disposition. Few men of his ardent temperament and extreme sense of justice ever made fewer enemies or more friends. To the latter he attached himself with entire devotion. If there were any of the former, probably no man ever heard him speak ill of them. His published works suffice to place his name imperishably upon the records of science. But only his contemporaries and friends will know how much he has done to help others, and how disinterestedly and gracefully that aid was ever rendered. He took with him to England, upon his return in the year 1820, a valuable herbarium of New England plants, especially those of the White Mountains, which were then rare and little known. He must have valued this collection highly, and have expected to use it. But he presented the whole of it to Dr., now Sir William Hooker, when he saw how serviceable it would be to him in the preparation of the " Flora Boreali-Americana." His British herbarium was long ago similarly given to a then young American botanist.

Another, who, twenty-five years ago, called to take leave of him upon returning to this country, found, as he left, the seat of his cab loaded with choice botanical books, which Dr. Boott had at the moment sent there from the shelves of his own library, where they were not duplicates. We know of one or two instances where he had commenced a critical study of a particular genus with a view to publication, but, upon learning that another person had taken up the same subject, he dispatched to him his own notes and other materials. The Linnæan Society of London owes no little of its present prosperity to his long and faithful services and his wise counsels. He kept up an active correspondence with his friends in this country; and for more than thirty years our young professional men, naturalists, and others who have visited Europe, have experienced cordial welcome and thoughtful kindness at his hands. The following gives a good idea of the man: —

"When practicing as a physician he discarded the customary black coat, knee-breeches, and silk stockings, for the very good reason that sombre colors could not but suggest gloomy ideas to the sick; and he was one of the first who adopted the custom now universal in the profession, of dressing in the ordinary costume. In doing this, Dr. Boott adopted the blue coat, gilt buttons, and buff vest of the period, which he continued to wear to the last, and with which dress his casual acquaintance, no less than his personal friends, will ever associate him. In person he was so tall and thin as almost to suggest ill-health; and the refinement of his manners, his expression, address, and bearing were in perfect keeping with his polished mind and many accomplishments."

The preceding extracts are all from an excellent article in the "Gardeners' Chronicle" for January 16, to which we are much indebted. In the first volume of the late Dr. Wallich's splendid "Plantæ Asiaticæ Rariores," published in the year 1830, is the figure of a handsome and curious Butomaceous plant, *Boottia cordata*, a genus dedicated "in honorem Francisci Boott, Americani, botanici ardentissimi et peritissimi, amici dilectissimi, non minus animi probitate quam scientiarum cultu, et morum suavitate egregii."

WILLIAM JACKSON HOOKER.[1]

Sir William Jackson Hooker died at Kew, after a short illness, on the 12th of August last, in the eighty-first year of his age.

Seldom, if ever before, has the death of a botanist been so widely felt as a personal sorrow, so extended were his relations, and so strongly did he attach himself to all who knew him. By the cultivators of botany in our own country, at least, this statement will not be thought exaggerated. Although few of our botanists ever had the privilege of personally knowing him, there are none who are not much indebted to him, either directly or indirectly. It is fitting, therefore, that some record of his life and tribute to his memory should appear upon the pages of the "American Journal of Science."

The incidents of his life are soon told. He was born on the 6th of July, 1785, at Norwich, England, where his father — who survived to even a greater age than his distinguished and only son — was at that period confidential clerk in a large business establishment. He was descended from the same family with "the Judicious Hooker," author of the "Ecclesiastical Polity." The name William Jackson was that of our botanist's cousin and god-father, who died young, and was soon followed by both his parents; in consequence of which their estate of Sea-salter, near Canterbury, came to young Hooker while yet a lad at the Norwich High School. He could therefore indulge the taste which he early developed for natural history, at this time mainly for ornithology. But the chance discovery of that rare and curious moss, *Buxbaumia aphylla*, which he took to his eminent townsman Sir James Edward Smith, directed his attention to botany, and fixed the bent of his long and active life. He now made ex-

[1] American Journal of Science and Arts, 2 ser., xli. 1. (1866.)

tensive botanical tours through the wildest parts of Scotland, the Hebrides, and the Orkneys, which his lithe and athletic frame and great activity fitted him keenly to enjoy. Coming up to London, he made the acquaintance of Sir Joseph Banks and of the botanists he had drawn around him, Dryander, Solander, and Robert Brown.

In 1809 he went to Iceland, to explore that then little-known island. The exploration was most successful; but the ship in which he embarked, with all his collections, notes, and drawings, was fired and destroyed, and everything was lost, he himself narrowly escaping with his life. Hooker's earliest work, the "Journal of a Tour in Iceland," in two octavo volumes, published at Yarmouth in 1811, and republished at London two years afterwards, gives an interesting account of his explorations and adventures, along with the history of a singular attempt at the time to revolutionize the island, — with which the disaster to the vessel he returned in was in some way connected, we forget how. Not disheartened by these losses, he now turned from a polar to an equatorial region, and made extensive preparations for going to Ceylon, with Sir Robert Brownrigg, then appointed governor. But the disturbances which broke out in that island, more serious than those which attended the close of his Iceland tour, again frustrated his endeavors.

The strong disposition for travel and distant exploration, frustrated in his own case, came to fruit abundantly in the next generation, in the world-wide explorations of his son. He himself made no more distant journey than to Switzerland, Italy, and France, in 1814, becoming personally acquainted with the principal botanists of the day, and laying the foundation of his wide correspondence and great botanical collections. In 1815 he married the eldest daughter of the late Dawson Turner, of Yarmouth, and established his residence at Halesworth, in Suffolk. The next year, in 1816, besides publishing some of the *Musci* and *Hepaticæ* of Humboldt and Bonpland's collection, he brought to completion his first great botanical work, "The British Jungermanniæ," with colored figures of each species, and microscopical analyses, in

eighty-four plates, all from his own ready pencil, — a work which took rank as a model both for description and illustration. In 1828 he brought out, in conjunction with Dr. Taylor, the well-known "Muscologia Britannica," the second edition of which, issued in 1827, is only recently superseded. The "Musci Exotici," with 176 admirable plates, appeared, the first volume in 1818, the second in 1820. These were his principal works upon Mosses and the like, — an excellent subject for the training of a botanist, and one in which Hooker, with quick eye, skilled hand, and intuitive judgment, was not only to excel but to lay the foundation of high excellence in general descriptive botany.

When arranging for a prolonged visit to Ceylon, it appears that he sold his landed property, and that his investment of the proceeds was unfortunate; so that the demands of an increasing family and of his enlarging collections, for which he always lavishly provided, made it needful for him to seek some remunerative scientific employment. Botanical instruction in Great Britain was then, more than now, nearly restricted to medical classes; the botanical chairs in the universities therefore belonged to the medical faculty, and were filled by members of the profession. But, through the influence of Sir Joseph Banks, as is understood, the Regius Professorship of Botany in the University of Glasgow was offered to Hooker, and was accepted by him. He removed to Glasgow in the year 1820, and assumed the duties of this position. Here, for twenty years — the most productive years of his life — he was not only the most active and conspicuous working botanist of his country and time, but one of the best and most zealous of teachers. The fixed salary was then only fifty pounds; and the class fees at first scarcely exceeded that sum. But his lecture-room was soon thronged with ardent and attached pupils, and the emoluments rose to a considerable sum, enabling him to build up his unrivaled herbarium, to patronize explorers and collectors in almost every accessible region, and to carry on his numerous expensive publications, very few of which could be at all remunerative.

The first publication of these busy years was the "Flora

Scotica," brought out in 1821. The next year but one brought the first of the three volumes of the "Exotic Flora," containing figures and descriptions of new, rare, or otherwise interesting exotic plants, admirably delineated, chiefly from those cultivated in the Glasgow and Edinburgh Botanic Gardens. Here first is manifested the interest in the flora of our own country, which has since identified the name of Hooker with North American botany, a considerable number of our choicest plants, especially of the Orchis family, having been here illustrated by his pencil.

The "Icones Filicum" (in which he was associated with Dr. Greville), in two large folio volumes, with two hundred and forty plates, begun in 1829 and finished in 1831, was his introduction to the great family of Ferns, to which he in later years devoted his chief attention.

In 1830 began, with the "Botanical Miscellany," that series of periodical publications which, continued for almost thirty years, stimulated the activity and facilitated the intercourse of botanists in no ordinary degree. The "Miscellany," in royal octavo, with many plates, closed with its third volume, in 1833. The "Journal of Botany," a continuation of the "Miscellany" in a cheaper form (in ordinary 8vo, issued monthly), took its place in 1834, but was itself superseded during the years 1835 and 1836 by the "Companion to the Botanical Magazine" (2 vols. imp. 8vo). In 1840 (after an interval in which the editor took charge of the botanical portion of Taylor's "Annals of Natural History"), the Journal was resumed and carried on to the fourth volume in 1842. Then, changed in title and enlarged, it appears as the "London Journal of Botany" for seven years, until 1848, and finally, as the "Journal of Botany and Kew Garden Miscellany," for nine years more, or to the close of 1857. The whole was carried on entirely at the editor's cost, he furnishing the MSS. for the letter-press, the drawings, etc., without charge, "so that it may be supposed that his expenses were heavy, while his profits were, as he always anticipated, literally nil."

The plates of the Journal being too few to contain a tithe

of the species in his herbarium which it was desirable to figure, an outlet for these was made by the "Icones Plantarum, or Figures, with brief descriptive Characters and Remarks of New or Rare Plants selected from the Author's Herbarium." Ten volumes of the work were published, with a thousand plates (in octavo), at the author's sole expense, and with no remuneration, between the years 1837 and 1854, the drawings of the earlier volumes by his own hand, of the later, by Mr. Fitch, whom he had trained to the work.

Botanists do not need to be told how rich these journals are in materials illustrative of North American botany, containing as they do accounts of collections made by Scouler, Drummond, Douglas, Geyer, etc. Equally important for the botany of our western coast, especially of California, is "The Botany of Captain Beech's Voyage" (4to), in the elaboration of which Sir William Hooker was associated with Professor Walker-Arnott. But his greatest contribution to North American botany — for which our lasting gratitude is due — was his "Flora Boreali-Americana" (2 vols. 4to, with 238 plates), of which the first part was issued in 1833, the last in 1840. Although denominated "the botany of the Northern parts of British America," it embraced the whole continent from Canada and Newfoundland, and on the Pacific from the borders of California, northward to the Arctic sea. Collections made in the British arctic voyages had early come into his hands, as afterwards did all those made in the northern land expeditions by the late Sir John Richardson, Drummond, etc., and the great western collections of Douglas, Scouler, Tolmie, and others, while his devoted correspondents in the United States contributed everything they could furnish from this region. So that this work marks an epoch in North American botany, which now could be treated as a whole.

We should not neglect to notice that, from the year 1827 down to his death, he conducted that vast repertory of figures of the ornamental plants cultivated in Great Britain, the "Botanical Magazine" (contributing over 2500 plates and descriptions); a work always as important to the botanist as to the cultivator, and under his editorship essential to both.

For the use of students at home, in 1830 he produced the "British Flora," which ran through five or six editions before it was consigned to his successor in the chair at Glasgow, Professor Arnott, who has edited two or three more.

We have enumerated the principal works published before he returned to England, including those which were reëdited or (as the periodicals) continued later. After twenty years' service in the Scotch University, Dr., now Sir William Hooker, K. H. (for in 1836 he accepted from William IV. — the last British sovereign who could bestow it — the honor of Knight of the Hanoverian Order), was appointed by government to take the direction of the Royal Gardens at Kew, until then in the private occupation of the crown, but now to be developed into a national scientific establishment.

Even since the death of Banks and Dryander, and while Aiton, the director, grew old and lost any scientific ambition he may once have had, Kew Gardens had declined in botanical importance. The little they preserved, indeed, was chiefly owing to the scientific spirit and unaided exertions of Mr. John Smith, then a foreman, afterwards for many years the superintending gardener (and well known to botanists for his writings upon Ferns), who, retired from his labors, still survives to rejoice in the changed scene.

The idea of converting Kew Gardens into a great national botanical establishment is thought to have originated either with Sir William Hooker himself, or with his powerful friend, and excellent patron of botany and horticulture, John, Duke of Bedford, the father of the present British Premier. Lord John Russell was in the ministry under Lord Melbourne when this project was pressed upon the authorities, and recommended to Parliament by the report of a scientific commission, and, succeeding to the Premiership, he had the honor of carrying it into execution at the propitious moment, and in the year 1841, of appointing Sir William Hooker to the direction of the new establishment. The choice could hardly have been different, even without such influential political support; indeed his patron and friend, the Duke of Bedford, died two years before the appointment was made; but Hooker's

special fitness for the place was manifest, and his claims were heartily seconded by the only other botanist who could have come into competition with him in this respect. We refer of course to Dr. Lindley. The office, moreover, was no pecuniary prize; the salary being only three hundred pounds a year (less, we believe, than the retiring pension of his unscientific superannuated predecessor), "with two hundred pounds to enable him to rent such a house as should accommodate his herbarium and library, by this time of immense extent, and essential, we need not say, to the working of the establishment, whether in a scientific or economic point of view." The salary, if we mistake not, has since been increased in some moderate proportion to the enlarged responsibilities and cares of the vast concern; but up to his death, so important an auxiliary as his unrivaled herbarium, and the greatest scientific attraction of the institution, was left to be supported (excepting some incidental aid) out of the Director's own private means.

Such record as needs here be made of Sir William Hooker as Director of Kew Gardens can be best and most briefly given mainly in the words of a writer in the "Gardeners' Chronicle" for September 2d, to whose ripe judgment and experience we may defer.

"Sir William entered upon his duties in command of unusual resources for the development of the gardens, such as had never been combined in any other person. Single in purpose and straightforward in action, enthusiastic in manner, and at the same time prepared to advance by degrees, he at once won the confidence of that branch of the government under which he worked. . . . To those in office above him, he imparted much of the zeal and interest he himself felt, which was proved by constant visits to the gardens, resulting in invariable approval of what he was doing, and promises of aid for the future. Another means at his disposal, and which he at once brought to bear on the work in hand, was his extensive foreign and colonial correspondence, including especially that with a large number of students whom he had imbued with a love of botany, and who were scattered over the most remote countries of the globe, and several of whom,

indeed, remained in more or less active correspondence with the Director up to the day of his death. His views were further greatly facilitated by his friendly intercourse with the foreign and colonial offices, the admiralty, and the East India Company, to all of whom he had the means of rendering services, by the recommendation of former pupils to posts in their employment, and by publishing the botanical results of the expeditions they sent out. . . .

"At the time of Sir William's taking office, the gardens consisted of eleven acres, with a most imperfect and generally dilapidated series of ten hot-houses and conservatories. Most of these have since been gradually pulled down, and, with the exception of the great orangery (now used as a museum for woods) and the large architectural house near the garden gates, which has just previously been removed from Buckingham Palace, not one now remains. They have been replaced by twenty-five structures (in most cases of much larger dimensions) exclusive of the Palm-stove and the hitherto unfinished great conservatory in the pleasure grounds.

"To describe the various improvements which have resulted in the present establishment, — including, as it does, a botanic garden of seventy-five acres, a pleasure ground or arboretum of two hundred and seventy acres, three museums, stored with many thousand specimens of vegetable products, and a magnificent library and herbarium, the finest in Europe, placed in the late king of Hanover's house on one side of Kew Green, near the gardens, — would rather be to give a history of the gardens than the life of their director." . . .

"It might be supposed that the twenty-four years of Sir William's life spent at Kew in the above public improvements, added to the daily correspondence and superintendence of the gardens, would have left little time and energy for scientific pursuits. Such, however, was far from being the case. By keeping up the active habits of his early life, he was enabled to get through a greater amount of scientific work than any other botanist of his age."

From this period his contributions to scientific botany, if we except the journals and illustrated works (contained until

lately, and some of them to the last), were mainly restricted to his old favorites, the Ferns. Some years before he removed to Kew, he found the veteran Francis Bauer, then an octogenarian, or near it, employed in drawing under the microscope admirable and faithful illustrations of the fructification of Ferns. He arranged immediately for their publication, drew up the letter-press, and so brought out, between 1838 and 1842, the well-known work entitled, "Genera Filicum, or illustrations of the Ferns and other allied genera." His large quarto, "Filices Exoticæ, in colored figures and descriptions of exotic Ferns, chiefly of such as are cultivated in the Royal Gardens of Kew," (100 plates,) appeared in 1859; — the drawings of these, as of nearly all his illustrated works for the last thirty years, by Walter Fitch, his indefatigable coadjutor, whom he had trained in Scotland, and who soon became "the most distinguished botanical artist in Europe." "A Second Century of Ferns" (imp. 8vo) was published in 1860 and 1861, the First Century being the tenth and closing volume of the "Icones Plantarum."

But the principal systematic work of these later years was his "Species Filicum, being descriptions of the known Ferns, . . . accompanied with numerous figures," in five volumes, 8vo. The first volume of this work appeared in 1846, and the last only a year and a half ago.

The crowd of new Ferns and the new knowledge which has accumulated in the interval of seventeen or eighteen years, demanded large revision and augmentation of the earlier volumes to bring them up to the level of the later ones. Moreover, a compendious work on this favorite class of plants was much needed. Both objects might be well accomplished by a synopsis of known Ferns in a single volume, to be for our day what Swartz's "Synopsis Filicum" was just sixty years ago. To this Sir William Hooker, upon the verge of fourscore, undauntedly turned, as soon as the last sheets of the "Species Filicum" passed from his hands, devoting to it the time that remained after attending to his administrative duties. Upon it he steadily labored, with unabated zeal and with powers almost unimpaired, conscientiously diligent and

constitutionally buoyant to the last. He had made no small progress in the work, and had carried the sheets of the initial number through the press, when an attack of diphtheria, then epidemic at Kew, suddenly closed his long, honored and most useful life.

Our survey of what Sir William Hooker did for science would be incomplete indeed, if it were confined to his published works — numerous and important as they are — and the wise and efficient administration through which, in a short space of twenty-four years, a Queen's flower and kitchen-garden and pleasure-grounds have been transformed into an imperial botanical establishment of unrivaled interest and value. Account should be taken of the spirit in which he worked, of the researches and explorations he promoted, of the aid and encouragement he extended to his fellow-laborers, especially to young and rising botanists, and of the means and appliances he gathered for their use no less than for his own.

The single-mindedness with which he gave himself to his scientific work, and the conscientiousness with which he lived for science while he lived by it, were above all praise. Eminently fitted to shine in society, remarkably good-looking and of the most pleasing address, frank, cordial, and withal of a very genial disposition, he never dissipated his time and energies in the round of fashionable life, but ever avoided the social prominence and worldly distinctions which some sedulously seek. So that, however it may or ought to be regarded in a country where court honors and government rewards have a factitious importance, we count it a high compliment to his sense and modesty that no such distinctions were ever conferred upon him in recognition of all that he accomplished at Kew.

Nor was there in him, while standing in a position like that occupied by Banks and Smith in his early days, the least manifestation of a tendency to overshadow the science with his own importance, or of indifference to its general advancement. Far from monopolizing even the choicest botanical materials which large expenditure of time and toil and money

brought into his hands, he delighted in setting other botanists to work on whatever portion they wished to elaborate; not only imparting freely, even to young and untried men of promise, the multitude of specimens he could distribute, and giving to all comers full access to his whole herbarium, but sending portions of it to distant investigators, so long as this could be done without too great detriment or inconvenience. He not only watched for opportunities for attaching botanists to government expeditions and voyages, and secured the publication of their results, but also largely assisted many private collectors, whose fullest sets are among the treasures of far the richest herbarium ever accumulated in one man's lifetime, if not the amplest anywhere in existence.

One of the later and not least important services which Sir William Hooker has rendered to botany is the inauguration, through his recommendation and influence, of a plan for the publication, under government patronage, of the Floras of the different British colonies and possessions, scattered over every part of the world. Some of these (that of Hongkong and that of the British West Indies) are already completed; others (like that of Australia, and the Cape Flora of Harvey and Sonder, adopted into the series) are in course of publication; and still others are ready to be commenced.

The free and cordial way in which Hooker worked in conjunction with others is partly seen in the various names which are associated with his authorship. This came in part from the wide range of subjects over which his survey extended, a range which must have contributed much to the breadth of his views and the sureness of his judgment. Invaluable as such extent of study is, in the present state and prospects of our science we can hardly expect to see again a botanist so widely and so well acquainted both with Cryptogamic and Phanerogamic botany, or one capable of doing so much for the advancement and illustration of both.

Our narrative of Sir William Hooker's scientific career and our estimate of his influence has, we trust, clearly, though incidentally, informed our readers what manner of man he was. To the wide circle of botanists, in which he has long

filled so conspicuous a place, to his surviving American friends and correspondents, some of whom have known him long and well, — and "none knew him but to love him, or named him but to praise," — it is superfluous to say that Sir William Hooker was one of the most admirable of men, a model Christian gentleman.

There could really be no question as to the succession to the charge of the great botanical establishment at Kew. But we may add, for the information of many of our readers, that the directorship vacated by Sir William's death has been filled by the appointment of his only surviving son, Dr. Joseph Dalton Hooker, whose well-established scientific fame and ability, no less than his lineage, may assure the continued equally successful administration of this most interesting and important trust.

JOHN LINDLEY.[1]

JOHN LINDLEY, one of the most renowned botanists of the age, died at his residence near London, on the 1st of November last, at the age of sixty-six years. He was born at Catton, near Norwich, where his father was a nurseryman, on February 5, 1799; and was educated at the Norwich Grammar School, as was his friend and earliest scientific acquaintance, Sir William Hooker. It was at the house of the latter, soon after his removal to Halesworth, that young Lindley began his career of authorship by translating Richard's "Analyse du Fruit," which was published in 1819. He appears already to have devoted himself to botanical and horticultural pursuits, and, it is said, had arranged to visit Sumatra and the Malayan Islands; but for some reason, perhaps connected with his father's reverses in business, the project was abandoned. At this juncture he was introduced by his friend Hooker to Sir Joseph Banks, who employed him as his assistant librarian. Sir Joseph recommended him to Mr. Cattley, for whom he edited the folio "Collectanea Botanica," illustrating some of the new and curious plants cultivated in Mr. Cattley's collection. He had already published his "Monograph of Roses" (1820) and his "Monograph of Digitalis" (1821), the latter illustrated by plates from Ferdinand Bauer's drawings. The next year (1822) began his connection with the Horticultural Society, as garden assistant secretary, when he took charge of the laying out of the garden at Chiswick. In 1826 he became sole assistant secretary, Mr. Sabine being honorary secretary until 1830, and then Mr. Bentham until 1841; nearly the whole active charge of the establishment falling upon Dr. Lindley. Then, as vice-secretary he conducted the operations of this great and pros-

[1] American Journal of Science and Arts, 2 ser., xli. 265. (1866.)

perous society, with almost undivided responsibility until 1858, when, dropping the laboring oar, he became secretary and member of the council, and took a leading part in the reorganization of the society, until, fairly broken down by overwork, in 1862 he was obliged to retire from the management. Besides his work in the Horticultural Society, sufficient in itself to task any ordinary powers, Dr. Lindley was professor of botany in University College from 1829 to 1861, giving elaborate courses of lectures every year, and also lecturer at the Apothecaries Garden at Chelsea for nearly the same period. He conducted the "Botanical Register" from about 1823 (although his name does not appear upon the title-page until somewhat later) down to its close in 1847; he did the principal botanical work in Loudon's "Encyclopedia of Plants," and wrote the botanical articles for the "Penny Cyclopedia," down to the letter R; contributed to the Transactions and Proceedings of the Horticultural Society, and edited its Journal; prepared the later volumes of Sibthorp's magnificent "Flora Græca," etc., etc.; besides writing and often reëditing his numerous classical botanical works, which, with his lectures to successive classes of pupils inspired by his own ardor, have made his name so famous wherever botany is cultivated. Of these numerous works we can mention only the principal. His "Synopsis of the British Flora" arranged according to the Natural Orders, first issued in 1829, has only a local and historical interest, as a part of his successful endeavors to introduce and popularize the natural system in England, where it had peculiar obstacles to contend against. His "Genera and Species of Orchidaceous Plants," with his "Sertum Orchidaceum," and, later, his "Folia Orchidacea" (which he was able only to commence), embody a portion of his labors upon an important and curious family of plants, upon which he became the paramount authority. His "Introduction to Botany," which ran through four editions, his outlines of the "First Principles of Botany," at length expanded into his "Elements of Botany," and his "School Botany," form a series of introductory works which have done much more for botanical instruction than any others

in the English language. By his "Flora Medica" he supplied to medical students a good botanical account of all the more important plants used in medicine. By his "Theory of Horticulture," explaining the principal operations of gardening upon physiological principles, in connection with his articles upon the subject in the "Gardeners' Chronicle," he may almost be said to have raised this branch of knowledge "from the conditions of an empirical art to that of a developed science." And, finally, in his "Introduction to the Natural System of Botany," the first edition of which, published in 1830, was the earliest systematic exposition of the natural system in the English language, or fairly available to English and American students, and his further development of this work into his classical "Vegetable Kingdom," — the one book which may take the place of a botanical library, — Dr. Lindley made his most important contributions to the advancement of systematic botany. The coming generation of botanists cannot be expected to appreciate the vast influence exerted by the earlier of these works in its day; the latter, however open to adverse criticism in particulars, is still unrivaled and is probably "that by which his name will be best known to posterity." Physiologist, morphologist, and systematist, he displayed equal genius in all these departments of the science, but he worked too rapidly to do himself full justice in any of them. "His power of work was indeed astonishing; whatever he undertook (and his undertakings were wonderful in amount and variety) he did with the utmost conscientiousness, never flagging until he had done it; and he was a splendid example of what can be accomplished by a man of strong will, habitually acting up to his oft-repeated saying, that to method, zeal, and perseverance nothing is impossible." "Until he had passed fifty years of age," it is stated that "he never knew what it was to feel tired either in body or mind." Such persons are sure to be overtasked. The Great Exhibition of 1851, adding protracted and onerous duties to his ordinary work, prostrated him with serious illness; the Second Exhibition, in 1862, in which he took charge of the whole colonial department, fatally injured his bodily and mental powers, and

cut short his scientific career. He was able, however, to enjoy the society of his immediate friends and to keep up an interest in his favorite pursuits quite to the close, which occurred from an apoplectic attack, on the morning of the 1st of November last.

Dr. Lindley was a man of marked character. While his biographer declared that "he was hot in temper and impatient of opposition," he no less truly adds that, "on the other hand, he had the warmest of hearts and the most generous of dispositions." He seemed as incapable of cherishing a resentment, as of repressing the expression of indignation for what he thought wrong; and if at times he made enemies, he was almost sure in time to convert his enemies into friends.

WILLIAM HENRY HARVEY.[1]

William Henry Harvey was born at Summerville, near Limerick, Ireland, on the 5th of February, 1811. His father, Joseph M. Harvey, was a highly respected merchant in that city and a member of the Society of Friends. William Henry was, we believe, the youngest of several children. He received a good education at Ballitore school, — an institution of the Friends, — and on leaving it was engaged for a time in his father's counting-room, devoting, however, all his spare time to natural history, his favorite pursuit even from boyhood. He made considerable attainments in entomology and conchology, and in botany he early turned his attention to Mosses and *Algæ*. To the study of the latter, in which he became pre-eminent, he was attracted from the first by the opportunities which he enjoyed on the productive western coast of Ireland, the family usually spending a good part of the summer at the seaside, mostly on the bold and picturesque shore of Clare. As the late Sir William Hooker's bent for botany was fixed by his accidental discovery of a rare moss, which he took to Sir J. E. Smith, so in turn was Harvey's, by his discovery of two new habitats of another rare moss, the *Hookeria lætevirens*, which led to a correspondence with Hooker, and to a life-long mutual attachment of these most excellent men. Encouraged by his illustrious friend and patron, Harvey sought some position in which he might devote himself to science; and it would appear was selected by Mr. Spring Rice (the late Lord Monteagle) for the post of colonial treasurer at the Cape of Good Hope; that by some accident the appointment was made out in the name of an elder brother, and an inopportune change of ministry frustrated all attempts at rectification. There was no other way but for the brother

[1] American Journal of Science and Arts, 2 ser., xlii. 273. (1866.)

to accept the undesigned appointment, and to take the young botanist with him to the Cape as his assistant. This was done, and the brothers sailed for that colony in the year 1835. But the health of the elder brother suddenly and hopelessly failed within a year, and he died in 1836 on the passage home. William Harvey's appointment to succeed his brother had been sent to the Cape while he was on his homeward voyage: he immediately returned to his post, and fulfilled its duties for three years, devoting his mornings to collecting and his nights to botanical investigation, with such assiduity that his health also gave way, and he was compelled to return home in 1839. The summer of the next year found him reëstablished and on his way to the Cape for the third time. But he could not long endure the sultry climate and the intense application; with broken health he came back in 1841 and gave up the appointment.

After two years of prostration and seclusion he was well again; and in 1844, on the death of Dr. Coulter, he was appointed keeper of the herbarium of Trinity College, Dublin. The most important portion of the herbarium then consisted of the collections, yet unassorted, made by Coulter in northwestern Mexico and California. Harvey generously added his own large collections, for which he was allowed fifty pounds a year, in addition to a slender salary, and he proceeded to build up the herbarium into a first-class establishment. The professorship of botany in the college, which was pretty well endowed, fell vacant about this time; and the college authorities, wishing to elect Harvey to the chair and so to combine the two offices, conferred upon him the necessary degree of M. D. But it was contended that an honorary degree did not meet the requirements, and so Dr. Allman, the present distinguished professor of natural history at Edinburgh, carried the election.

Except for the slenderness of his salary, Dr. Harvey was now well placed for scientific work, the object to which he wished to devote his life; and he entered upon and pursued his distinguished career henceforth with an entire and well-directed energy that never flagged until he was prostrated by mortal disease.

He had already published, at the Cape, in 1838, his "Genera of South African Plants," hastily prepared, solely for local use, but no unworthy beginning of his work in Phænogamous botany; and in his favorite department of the science he had brought out, in 1841, his "Manual of British Algæ," which he reëdited in 1849. He now commenced the first of the series of his greatest works, illustrated by his facile pencil, for he drew admirably. The first (monthly) part of his excellent and beautiful "Phycologia Britannica, a History of British Seaweeds," containing colored figures of all the species inhabiting the shores of the British Islands, appeared in January, 1846; and the undertaking was completed in 1851 in three (or four) volumes, with three hundred and sixty plates, all drawn on stone by his own hand. A similar but less extended work, the "Nereis Australis, or Algæ of the Southern Ocean," which was begun in 1847, was carried only to fifty plates of selected and beautiful species.

In 1848, Dr. Harvey succeeded Dr. Litton as professor of botany in the Royal Dublin Society, to which belonged the bŏtanic garden at Glasnevin; this required him to deliver short courses of lectures annually in Dublin or some other Irish town, and provided a welcome addition to his income.

In 1848, at the request of his friend Van Voorst, the publisher, he wrote his charming little volume, "The Seaside Book," the unsurpassed model of that class of popular scientific books; it was published in 1849, and has passed through several editions. In July of that year, having arranged a visit to this country, and having been invited to deliver a course of lectures before the Lowell Institute, he took steamer for Halifax and Boston, passed the summer and autumn in exploring the shores of the northern States, and in the society of his friends and relatives; for the late Mr. Jacob Harvey, still well and pleasantly remembered in New York, who married the daughter of Dr. Hosack, was his elder brother. In the autumn he gave an admirable course of lectures upon Cryptogamic botany before the Lowell Institute in Boston, and afterwards a shorter course at the Smithsonian Institution at Washington. He then traveled in the southern At-

lantic States, continuing the exploration of our *Algæ* down to Florida and the Keys; and in May, 1850, he returned to Ireland. Under the wise and liberal arrangements made by Professor Henry in behalf of the Smithsonian Institution, and with his own large collections augmented by the contributions which every student or lover of *Algæ* was glad to place in such worthy hands, Professor Harvey now prepared his "Nereis Boreali-Americana, or Contributions to a History of the Marine Algæ of North America." The work is a systematic account of all the known Marine *Algæ* of North America, but with figures only of the leading species. It was issued in three parts: the first part, the *Melanospermeæ*, in 1852, in the third volume of the Smithsonian Contributions to Knowledge; the second, *Rhodospermeæ*, in the fifth volume; and the third, or *Chlorospermeæ*, in the tenth volume of the series, published in 1858; and the three parts, collected for separate issue, compose a thick imperial quarto volume, of five hundred and fifty pages of letter-press and fifty plates. The work remains the principal if not the only guide to the American student of *Algæ*, and one of the most popular as well as useful of the various contributions to knowledge which the well-managed bequest of Smithson has given to the world.

Before the last part of the "Nereis Boreali-Americana" was published, Professor Harvey had sought a wider field of scientific labor and observation. Obtaining a long leave of absence, and some assistance from the University in addition to the continuance of his salary, he left England in August, 1853, by the overland route for Australia, stopping at Aden and Ceylon to collect: he visited the east, south, and west coasts of Australia, as well as Tasmania. Taking advantage of a missionary ship, which was to cruise among the South Sea Islands, and which offered him unexpected facilities, he visited the Fiji, Navigators', and Friendly Islands, touching also at New Zealand. Returning to Sydney, he sailed to Valparaiso, which he reached much prostrated through over-exertion in a warm climate; and when recuperated he returned home by way of the Isthmus, arriving in October,

1856. The algological collections of these three laborious years, or the Australian portion of them, formed the subject of Professor Harvey's third great illustrated work, and one of the most exquisite of the kind, the "Phycologia Australica," the serial publication of which began in 1858 and was concluded in 1863, in five imperial octavo volumes, each of sixty colored plates. All but the last century of plates were put upon stone by the author.

Upon Dr. Harvey's return in 1856 from his long expedition he found the chair of botany in the University of Dublin vacated by the appointment of Dr. Allman to that of natural history in the University of Edinburgh; and he was at once preferred to the position which he had sought when younger and freer, and which he now occupied till his death. The exhausting duties of this chair, and of that which he still held in the Royal Dublin Society, undiminished by the transference to the government Museum of Irish Industry, did not prevent Professor Harvey from entering with unabated ardor upon an undertaking of greater magnitude than any preceding one. This was the "Flora Capensis," a full systematic account of all the plants of the Cape Colony and the adjacent provinces of Caffraria and Natal, — in which he was associated with Dr. Sonder of Hamburg. Three thick octavo volumes of this work have appeared, the last in 1865, including the *Compositæ*. Along with this Dr. Harvey — learning for the purpose another form of lithographic drawing — brought out, between the years 1859 and 1864, two volumes of his "Thesaurus Capensis, or Illustrations of the South African Flora," comprising two hundred plates of interesting phænogamous plants. A complete list of his publications would include several contributions to scientific periodicals, mainly to "Hooker's Journal of Botany," and a few miscellaneous writings.

In April, 1861, Dr. Harvey married Miss Phelps of Limerick. If not robust, he was apparently in good health, in the full maturity of his powers, and it was hoped only at the noonday of his allotted course of usefulness. But ere the lecture season of that summer was over, an attack of hemorrhage

from the lungs gave notice of a serious pulmonary disease. Yet he seemed to recover from this almost completely: he resumed his stated work, and gave his lectures as usual in 1863, and also in the spring of the following year, but with some difficulty. The winter and spring of 1864–65 were spent in the south of France, with only transient benefit. Returning to his home and his herbarium, he worked on still at the Cape Flora, with cheerful spirit but feeble hands, until he could work no longer. Last spring he sought in Devonshire a milder air, and found a peaceful rest. "On Tuesday, the 15th of May, 1866, at the age of fifty-five years, he quietly breathed his last, at the residence of Lady Hooker, the widow of his long-attached friend Sir William J. Hooker, surrounded by kind and anxious relatives and friends, and was buried in the cemetery at Torquay on Saturday, the 19th of May."

Mr. Harvey was one of the few botanists of our day who excelled both in Phænogamic and Cryptogamic botany. In Algology, his favorite branch, he left probably no superior; in systematic botany generally he had won an eminent position. He was a keen observer and a capital describer. He investigated accurately, worked readily and easily with microscope, pencil, and pen, wrote perspicuously, and where the subject permitted, with captivating grace, affording, in his lighter productions, mere glimpses of the warm and poetical imagination, delicate humor, refined feeling, and sincere goodness which were charmingly revealed in intimate intercourse and correspondence, and which won the admiration and the love of all who knew him well. Handsome in person, gentle and fascinating in manners, genial and warm-hearted, but of very retiring disposition, simple in his tastes and unaffectedly devout, it is not surprising that he attracted friends wherever he went, so that his death will be sensibly felt on every continent and in the islands of the sea.

HENRY P. SARTWELL.[1]

Dr. Henry P. Sartwell died at Penn Yan, New York, on the 15th of November, at the age of seventy-five years. He was, we believe, indigenous to the western part of the state of New York, and when a medical student resided at Gorham, Ontario County. More than forty years ago he was established as a physician in the neighboring town of Penn Yan, where he passed his honorable and useful life, engaged to the last in the practice of his profession. It is said that the illness of which he died was brought on by over-exertion in attendance at the bedside of a sick friend. He was fond of all branches of natural history, and a diligent observer and collector in more than one; but in botany he has secured an enduring reputation. He was in his way a model local botanist. He thoroughly explored the district within his reach; he prepared admirable specimens in great numbers, and distributed them with a free hand. Few botanists in this country have contributed to so many herbaria, home or foreign, none more disinterestedly and generously. He accumulated a large herbarium, specially rich and attractive in plants of western New York, in Carices and Ferns. Desirous of insuring its preservation and future usefulness, and needing in his old age the very moderate sum which it would bring him, he a few years ago transferred his herbarium to Hamilton College, where it is valued and well cared for. Most local botanists, when they have nearly exhausted the resources of the district they are confined to, are liable to sink into inactivity. Dr. Sartwell avoided this destiny, and prolonged to the last his enjoyment and usefulness, by making a specialty of the great genus Carex. Dewey, Torrey, Tuckerman, Carey, Boott, all who have published within the last thirty years

[1] American Journal of Science and Arts. 2 ser., xlv. 121. (1868.)

upon this genus, have had frequent occasion to acknowledge their obligations to him; and he erected for himself a monument of his zeal and devotion, and a testimony to his powers of observation, in his "Carices Americæ Septentrionalis Exsiccatæ," the first part of which was issued in the year 1848, the second in 1850; while his interest and activity in the study continued with little abatement down to the last year or two. He published nothing else, we believe, excepting a "Catalogue of the Plants growing in the vicinity of Penn Yan"; but he contributed no little to the value of the publications of others, especially to those of the writer of this notice, and to the "Flora of the State of New York." The Carex which had been dedicated to Dr. Sartwell has proved to be identical with an old European species, but a very distinct and peculiar genus of *Compositæ* found in southwestern Texas, keeps up the name of Sartwellia. His most intimate associate in caricological study survived him only one month.

CHESTER DEWEY.[1]

Professor Chester Dewey died at Rochester, New York, December 13, having completed the eighty-third year of his age. "He was born at Sheffield, Massachusetts, October 25, 1784; was graduated at Williams College in 1806; studied for the ministry; was licensed to preach in 1808, and during the latter half of that year officiated in Tyringham in western Massachusetts. The same year he accepted a tutorship in Williams College, and in 1810 was appointed professor of mathematics and natural philosophy, an office which he discharged for seventeen years. During his connection with the college he did much to advance the standard of scholarship and enlarge the course of study in his own and kindred departments. Between 1827 and 1836 he was principal of the 'Gymnasium,' a high school for boys at Pittsfield, Massachusetts. In the latter year he removed to this city (Rochester), and became principal of the Rochester Collegiate Institute, which post he held until 1850, when he was elected professor of chemistry and natural philosophy in the Rochester University. He was actively engaged in the duties of that position till 1860, when he retired at the age of seventy-seven, though he continued to teach to some extent till his eightieth year. The last four years he has passed in easy and dignified retirement, happy in the society of his family and friends, beloved and respected by all, and occupying himself still with his scientific studies and with meteorological observations, which he conducted with great care and regularity."

Dr. Dewey was an early and a frequent contributor to this Journal, upon several subjects, but especially upon that with which his name is inseparably connected, the Carices of North America. His "Caricography," commenced in 1824,

[1] American Journal of Science and Arts, 2 ser., xlv. 122. (1868.)

was continued year after year with few breaks, down to the close of 1866, when it terminated with a general Index to Species. It is not for us to speak particularly of the merits of this elaborate monograph, patiently prosecuted through more than forty years. This and the monograph of Schweinitz and Torrey laid the foundation and insured the popularity of the study of Sedges in this country. But while the latter systematic arrangement was published as a whole in 1825, Dr. Dewey's, carried on without particular order, extended through a lifetime, and represents both the earlier and the later knowledge. What is needed to render these stores of observation and their permanent results most available, is a systematic digest or synopsis, something like that which the author contributed to Wood's Botany, in the article Carex, but with all the more important references. Hopes were entertained that he might be able to crown his life's work in this way. But at past fourscore and ten this could not be expected. Beyond this favorite genus, Professor Dewey's botanical writings were few; the most considerable was his "History of the Herbaceous Plants of Massachusetts," published under the authority of the State, being the companion volume to the better known "Report on the Trees and Shrubs," by Mr. Emerson. Botany was one of the occupations of Professor Dewey's leisure hours; his long life was mainly devoted to education. Turning his attention to a special yet almost inexhaustible subject, however, and laboring perseveringly and faithfully, although under many disadvantages, he has permanently and honorably impressed his name upon the science in which the Californian Umbelliferous genus Deweya records his services. He was an excellent, simple-hearted, devout man, a fine specimen of the western New Englander of the old school.

The lovers of Carex, so numerous in this country, will cherish the memory of these two venerable men, Sartwell and Dewey, long associated in congenial pursuits, and gone to their rest together. May the turf of the Sedges they loved, and which cover or ought to cover the low mounds under which their dust reposes, keep them perennially green, and adorn them each returning spring with their sober blossoms!

GEORGE A. WALKER-ARNOTT.[1]

George A. Walker-Arnott, Professor of Botany in the University at Glasgow, died on the 17th of June last, in the seventieth year of his age. He was born in Edinburgh, February 6, 1799, educated at the celebrated high school of that city, and at the university, where he took high rank as a scholar, especially in the mathematics, — publishing two papers in Tilloch's "Philosophical Magazine" in 1817 and 1818, while yet a student in arts, — and then, turning to law studies, he was called to the bar as a member of the faculty of advocates in the year 1821. He hardly entered, however, upon the duties of his profession, his taste for natural history having been early developed under the lectures of Professor Jameson and of Mr. Stewart, — the latter a well-known teacher of botany at that time, and his patrimonial estate of Arlary in Kinros-shire suffering for his support, so that he could devote himself to botany, as he did, with unsurpassed ardor and success. His earliest botanical paper, upon some Brazilian Mosses, was written in France, and published in a journal at Paris in 1823. In 1826 and 1827 he contributed to the "Edinburgh New Philosophical Journal" a lively narrative of a botanical tour to the south of France and the Pyrenees. He resided for some time at Montpellier and in Paris, examining the principal herbaria there, also that of De Candolle at Geneva, and in 1828 the herbaria at St. Petersburg. In 1831 he married and established himself with his collections at Arlary, where he resided until, in 1845, he accepted the professorship of botany in the University of Glasgow. It was during these fourteen years that the vast amount of scientific work he was able to accomplish was mainly done. He wrote the article "Botany" in the seventh edition of the "Encyclo-

[1] American Journal of Science and Arts, 2 ser., xlvii. 140. (1869.)

pedia Britannica," — the best treatise of the kind of its day in the English language, and one of the most influential. In conjunction with his early friend, Sir William Hooker, he wrote the "Contributions to the Flora of South America," etc., which form a long series of articles in the "Botanical Miscellany," "Journal of Botany," and other similar periodical or serial publications edited by Sir William. He took a similar part in the "Botany of Beechey's Voyage"; in connection with Dr. Wight he brought out the first volume of the "Prodromus Floræ Peninsulæ Indiæ Orientalis"; and made numerous contributions to various periodicals. Up to 1845 or somewhat later Dr. Arnott was one of the foremost botanists of the time, one of the most zealous and sagacious, versed alike in European and exotic botany. But upon assuming the duties of his chair at Glasgow he appears soon to have abandoned the field in which he had won the highest honors, and in which much more was justly expected. He assumed, however, the joint authorship of Hooker's "British Flora," taking, we believe, the whole charge and responsibility of the later editions. As he began with Mosses, so for the last fifteen or twenty years of his life he devoted himself principally to the *Diatomaceæ*, bringing to their investigation all the ardor of his nature and the keenest powers of observation, combined with indomitable patience and unwearied care. So that he became in this department of microscopical research one of the highest authorities, and amassed one of the richest collections extant. As a professor he was greatly esteemed and respected, although he may be thought to have come almost too late in life to the professor's chair. In his later years he was much withdrawn from general botanical intercourse; but his surviving correspondents and friends on this side of the ocean cherish very pleasant memories of him.

NATHANIEL BAGSHAW WARD.[1]

Nathaniel Bagshaw Ward, Fellow of the Royal and Linnæan societies, after whom, as its inventor, the Wardian case is named, died at the ripe age of seventy-seven years, on the 4th of June last. He was born in the east end of London, where his father was a medical practitioner of repute, and where for the greater part of his busy and most useful life he laboriously devoted himself to the same profession. About twenty years ago he exchanged the smoke-charged atmosphere and dingy dwellings of Wellclose Square for the pleasant and airy suburb of Clapham Rise, but still actively engaged almost to the last in professional practice, and in his various official duties, mainly in connection with the Apothecaries Society, filling in succession nearly all its important offices. The renovation and even the maintenance of the celebrated Apothecaries Garden at Chelsea — the oldest botanical establishment of the country — is probably mainly due to his counsels and exertions. We cannot here enter into the interesting history of the now familiar Wardian case, — a discovery which grew out of Mr. Ward's persistent endeavors to cultivate the plants he delighted in under the smoke and soot of the dingiest part of London, and which resulted in providing for the poor as well as the rich denizens of the smoky towns of the old world the inexpensive but invaluable luxury or comfort of being surrounded at all seasons with growing plants and fresh flowers. Nor is the invention less applicable to house-culture, especially of Ferns, under the clearer and purer air of our own country rendered arid by the cold of winter, as hundreds could testify who have enjoyed the benefit, perhaps without knowing even the name of their benefactor. Equally important is the application of the Wardian case to the conveyance of living plants between distant countries. The writer well remembers the

[1] American Journal of Science and Arts, 2 ser., xlvii. 141. (1869.)

first case of growing plants sent to New York thirty-five years ago, which arrived as fresh and healthy as when they left London; and the transmission was quite successful between England and Australia, when the voyage, confined to sailing-ships, was far longer than now. So useful has this contrivance proved to be in this respect, that the director of Kew Gardens "feels safe in saying that a large proportion of the most valuable economic and other tropical plants now cultivated in England would, but for these cases, not yet have been introduced." The earliest published account of the Wardian case was given by Mr. Ward in the form of a letter to his near friend, the late Sir William Hooker, and was printed in the "Companion to the Botanical Magazine" for May, 1836. His volume "On the Growth of Plants in Closely Glazed Cases" appeared in the year 1842, and a second edition, considerably enlarged and suitably illustrated, was published a few years later. These were, we believe, Mr. Ward's only scientific publications, excepting reports of communications to various societies with which he was connected, several of them relating to a subject near to his heart: the improvement of the dwellings of the poor in England, and the amelioration, in other respects, of their hard condition. A most enthusiastic and, in some departments, a learned botanist, his contributions to his favorite avocation were not in the form of authorship, to which he seemed averse: a man "given to hospitality" indeed, but as unpretending as it was cordial and unlimited. The coming generation will hardly appreciate the extent of the influence he exerted and the strength of the attachment he inspired so widely among the cultivators of natural science, nor understand, perhaps, how it could be said of him, and without exaggeration, that "for very many years his hospitable house, first in Wellclose Square, and latterly at Clapham Rise, was the most frequented metropolitan resort of naturalists from all quarters of the globe of any since Sir Joseph Banks' day." But while any survive of those who have had the privilege of knowing him personally, or in the friendly correspondence he delighted in, Mr. Ward will be remembered as "one of the gentlest, kindest, and purest," and in the highest sense one of the best of men.

MOSES ASHLEY CURTIS.[1]

Moses Ashley Curtis was born in Stockbridge, Massachusetts, on the 11th of May, 1808. His father was the Rev Jared Curtis, of Stockbridge, afterward for many years chaplain of the state prison at Charlestown. His mother was a daughter of General Moses Ashley. He was fitted for college chiefly under his father's tuition, and was graduated at Williams in the class of 1827. Three years afterward he went to Wilmington, North Carolina, as a tutor in the family of Governor Dudley, while at the same time he studied divinity. There he resided until the year 1841, with the exception of a year and a half passed with his father in Charlestown. In the autumn of 1834 he married Miss De Rosset, of Wilmington, who survives him. He took holy orders at Richmond, Virginia, in the summer of 1835; became rector of the Protestant Episcopal Church at Hillsborough, North Carolina, in 1841, and fulfilled the duties of this station for the remainder of his life, with the exception of ten years, from 1847 to 1857, during which he had the pastoral charge of a parish at Society Hill, South Carolina. The degree of Doctor of Divinity was conferred on him by the University of North Carolina, at Chapel Hill. His health for a few years past was sensibly impaired; but he was able to perform his professional duties, and, in a measure, to prosecute his scientific studies, until the 10th of April last, when he died suddenly, probably of heart-disease.

Dr. Curtis's attention must have early been attracted to botany, and his predilection fixed by his residence at Wilmington, one of the richest and most remarkable botanical stations in the United States. For it was in the year 1843, after only three years' residence there, that he communicated

[1] American Journal of Science and Arts, 3 ser., v. 391. (1873.)

to the Boston Society of Natural History his first botanical work, namely, his "Enumeration of Plants growing spontaneously around Wilmington, North Carolina, with remarks on some New and Obscure Species." This was printed in the first volume and second number of that society's Journal; but the original impression having been mainly destroyed by fire, important additions and emendations were made in the subsequent reprint. The author's powers of observation and aptitude for research are well shown in this publication, and it is one of the first of the kind in this country in which the names are accented. In his note upon the structure of Dionæa, or Venus's Fly-trap, — a plant found only in the district around Wilmington, — Dr. Curtis corrected the account of the mode of its wonderful action which had prevailed since the time of Linnæus, and confirmed the statement and inferences of the first scientific describer, Ellis, namely, that this plant not only captures insects, but consumes them, enveloping them in a mucilaginous fluid which appears to act as a solvent. Extending his botanical observations to the western borders of his adopted State, Dr. Curtis was among the first to retrace the steps and rediscover the plants found and published by the elder Michaux, in the higher Alleghany Mountains. But for the last twenty-five years his scientific studies were mainly given to mycology, in which he became a proficient, and the highest American authority. His papers upon *Fungi*, some of which are large, and all are important, were mainly published by the American Philosophical Society, and by the Linnæan Society of London. Several of them are the joint productions of Dr. Curtis and the able English mycologist Mr. Berkley.

His other published writings mainly are "A Commentary on the Natural History of Dr. Hawks's 'History of North Carolina,'" — a good specimen of his appreciation of exact research and of sharpness of wit without acerbity; two papers in Silliman's Journal on "New and Rare Plants of the Carolinas"; and the botanical portion of the "Geological and Natural History Survey of North Carolina," in two parts; — the first a popular account of the trees and shrubs, issued in

1860 ; the other, a catalogue of all the plants of the State, in 1867. This includes the lower Cryptogamia, especially the *Fungi*, of which he enumerates almost 2400 species, while the phænogamous plants are less than 1900. All our associate's work was marked by ability and conscientiousness. With a just appreciation both of the needs of the science and of what he could best do under the circumstances, when he had exhausted the limited field in Phænogamous botany within his reach, he entered upon the inexhaustible ground of mycology, which had been neglected in this country since the time of Schweinitz. In this difficult department he investigated and published a large number of new species, as well as determined the old ones, and amassed an ample collection, the preservation of which is most important, comprising as it does the specimens, drawings, and original notes which are to authenticate his work. By his unremitting and well-directed labors, filling the intervals of an honored and faithful professional life, he has richly earned the gratitude of the present and ensuing generations of botanists. Several years ago he prepared drawings of the edible *Fungi* of the country, with a view to making them better known in an accessible and popular publication ; but he was unable to find a publisher. He was much impressed with their importance as a source of food. During the hardships of the Rebellion he turned his knowledge of them to useful account for his family and neighborhood ; and he declared that he could have supported a regiment upon excellent and delicious food which was wasting in the fields and woods around him.

HUGO VON MOHL.[1]

Hugo von Mohl, the acknowledged chief of the vegetable anatomists of this generation, died on the first day of April last. He was born at Stuttgart, April 8, 1805, the youngest of four brothers who all became men of mark in political and scientific life; Julius the orientalist and Hugo the botanist being the most distinguished. The latter was educated at the Stuttgart Gymnasium and Tübingen University, where he studied medicine as well as natural history and physics. His first publication, while a student, in the year 1827, was his "Essay on the Structure and Coiling of Tendrils and Twiners," written in response to a prize-question offered by the Tübingen Medical Faculty. In it he divined the real nature of the movements which coiling stems and tendrils execute, as has recently been clearly made out. In the following year appeared his inaugural dissertation on the "Pores of the Cellular Tissue of Plants," in which his later views and discoveries, respecting the structure, growth, and component parts of cells, as subsequently developed, are already foreshadowed. About this time his choice was made for a scientific rather than a medical career; and he went to Munich to prosecute more advantageously his favorite studies. Here the late Von Martius and Zuccarni were his botanical masters, and Agassiz, Karl Schimper, Braun, and Engelmann his fellow-students. Here he made those researches upon the anatomy of Ferns, Cycads, and especially of Palms, — the latter a most important contribution to Martius's great work upon Palms, the former also contributed to another work by Martius, — which first displayed his remarkable talents for histological investigation, to which his subsequent scientific life was mainly devoted. His merits were promptly recognized by a call to the Imperial

[1] American Journal of Science and Arts, 3 ser., v. 393. (1873.)

Botanic Garden of St. Petersburg, as assistant to its director, Dr. Fischer, and to the chair of physiology in the Academy of Berne. He accepted the latter in 1832, and occupied it until 1835. Then, upon the death of Schubler, he returned to Tübingen, accepted the professorship of botany in its high school, in which chair and in that of Tübingen the rest of his life was passed. Invitations to more prominent and lucrative positions, as, for example, to the botanical chair at Berlin University, when vacated by the death of the veteran Link, were unhesitatingly declined. Although he published numerous (about ninety) special papers or articles, most of them important and timely, and some of great pith and moment, he resolutely declined to bring out any general work. His "Mikrographie" (1846) and his "Principles of the Anatomy and Physiology of the Vegetable Cell" are his only writings which may claim to be such. The latter, an admirable and still invaluable treatise, appeared as an article in Rudolf Wagner's "Cyclopædia of Physiology," but is best known to English readers in its separate form, in a translation made by the late Professor Henfrey, with the author's sanction, issued by Van Voorst in 1852. A year or two later it was for a time understood, to the great satisfaction of botanists, that Mohl had agreed to take a prominent part in the production of a general manual of the anatomy and physiology of plants; but his promise was soon withdrawn. For thirty years he was one of the editors of the "Botanische Zeitung"; but the editorial labor must have devolved mainly upon Schlechtendal and his successor, although occasional articles from Mohl's pen appeared as late as the year 1871. During that year his health became seriously impaired; yet as the new year advanced, apprehension disappeared. Upon Easter Monday he was apparently well, and so retired to nightly rest; in the morning he was found to have died in sleep.

ROBERT WIGHT.[1]

Robert Wight, M. D., died at his residence, near Reading, England, May 26, 1872, at the age of seventy-six years. He was born in East Lothian, Scotland, educated at the Edinburgh High School, and professionally at Edinburgh University, where he took his medical degree in 1816. He went to India, the field of his botanical career and most useful administrative activity for forty years, in 1819. He was first assistant surgeon and afterward full surgeon of a native regiment in the East India Company's service, but was soon transferred to the charge of the Botanic Garden at Madras, and finally to that of the important cotton plantations at Coimbatoor. His earliest botanical contributions occupy a conspicuous place in Hooker's "Botanical Miscellany," commencing in 1830, and in the continuation of that work under other names and firms. In 1834, after a temporary sojourn in his native city, appeared the first volume of a model flora, the "Prodromus Floræ Peninsulæ Indiæ Orientalis," by Dr. Wight and Mr. (afterwards Professor) Arnott, of which their successors in the field remarked, that it is the most able and valuable contribution to Indian botany which has ever appeared, and one which has few rivals in the whole domain of botanical literature. Dr. Wight returned to India immediately after the publication of this initial volume of the work, which was never continued. In India, assisted by native artists whom he had trained, he brought out two quarto volumes of "Illustrations of Indian Botany," with one hundred and eighty-two colored plates; his "Spicilegium Nielgherrense," of similar character; and finally his "Icones Plantarum Indiæ Orientalis," in six volumes, with 2101 uncolored lithographic plates, and elaborate analysis, of unequal merit, many of them

[1] American Journal of Science and Arts, 3 ser., v. 395. (1873.)

truly excellent, but all wonderful, under the circumstances of their production. When he returned to England, nearly twenty years ago, his productive season, as it proved, was nearly over. But he distributed his collections with a liberal hand, as indeed he had always done, and in spite of a failing health enjoyed in a serene and happy old age the quiet country residence to which he retired.

FREDERIK WELWITSCH.[1]

Frederik Welwitsch, M. D., died in London, on the 20th of October last, in the sixty-ninth year of his age. He was a native of Carinthia; was educated at Vienna; was commissioned by the Würtemberg Unio Itineraria to collect the plants of the Azores and Cape Verde Islands; but on reaching Lisbon and finding good employment there, he made Portugal the field of his investigations, until, in 1850, he was sent by the Portuguese government to explore the natural history of its possessions on the west coast of Africa. His exploration of Angola and Benguela was rewarded by the discovery of more highly curious plants, probably, than any other that has been undertaken since Australia was opened to botanists; among them, and strangest of all, the genus which commemorates the discoverer, *Welwitschia mirabilis*, which Dr. Hooker, who described and illustrated it, does "not hesitate to consider the most wonderful, in a botanical point of view, that has been brought to light during the present century." Perhaps the limitation in the latter clause of the sentence is needless. This inhabits a most arid waste. In another district, under almost opposite conditions, Welwitsch had the good fortune to find the only Cactaceous plant indigenous out of America, namely, *Rhipsalis Cassytha*, and in a lake a new and most remote habitat of our *Brasenia peltata*.' In his "Sertum Angolense," a splendid memoir published by the Linnæan Society, with twenty-five plates, some of his most interesting discoveries are described: but the still unpublished portions of his collections must furnish most important contributions to the "Flora of Tropical Africa," now in progress under the orders of the British Colonial department and the editorship of Professor Oliver of Kew. It is to be hoped that they are to be more fully available for this flora than they have thus far been.

[1] American Journal of Science and Arts, 3 ser., v. 396. (1873.)

JOHN TORREY.[1]

John Torrey, M. D., LL. D., died at New York, on the 10th of March, 1873, in the seventy-seventh year of his age. He has long been the chief of American botanists, and was at his death the oldest, with the exception of the venerable ex-President of the American Academy (Dr. Bigelow), who entered the botanical field several years earlier, but left it to gather the highest honors and more lucrative rewards of the medical profession, about the time when Dr. Torrey determined to devote his life to scientific pursuits.

The latter was of an old New England stock, being, it is thought, a descendant of William Torrey, who emigrated from Combe St. Nicholas, near Chard, in Somersetshire, and settled at Weymouth, Massachusetts, about the year 1640.[2]

His grandfather, John Torrey, with his son William, removed from Boston to Montreal at the time of the enforce-

[1] Proceedings American Academy of Arts and Science, ix. 262. (1873.)

[2] In some notes furnished by a member of the family, the descent is endeavored to be traced through the eldest of the five sons who survived their parent, namely, Samuel, who came with him from England, became a minister of the Gospel, and had the unprecedented honor of preaching three election sermons (in 1674, 1683, and 1695), as well as of having three times declined the presidency of Harvard College (after Hoar, after Oakes, and after Rogers). Although educated at the college, he was not a graduate, because he left it in 1650, after three years' residence, just when the term for the A. B. degree was lengthened to four years. The tradition has it, that, "at the prayer-meetings of the students, he was generally invited to make the concluding prayer," — for which an obvious reason suggests itself, — for, "such was his devotion of spirit that, after praying for two hours, the regret was that he did not continue longer." Students of the present day are probably less exacting.

The desire to claim a descent through so eminent a member of the family is natural. But our late venerable associate, Mr. Savage, in his Dictionary of early New England families, states that he could not ascertain that Samuel had any children.

ment of the Boston Port Bill. But neither of them was disposed to be a refugee. For the son, then a lad of seventeen years, ran away from Canada to New York, joined his uncle, Joseph Torrey, a major of one of the two light infantry regiments of regulars (called Congress's Own) which were raised in that city; was made an ensign, and was in the rearguard of his regiment on the retreat to White Plains; served in it throughout the war with honor, and until at the close he reentered the city upon Evacuation Day, when he retired with the rank of captain. Moreover, the father soon followed the son, and became quartermaster of the regiment. Captain Torrey, in 1791, married Margaret Nichols, of New York.

The subject of this biographical notice was the second of the issue of this marriage, and the oldest child who survived to manhood. He was born in New York, on the 15th of August, 1796. He received such education only as the public schools of his native city then afforded, and was also sent for a year to a school in Boston. When he was fifteen or sixteen years old his father was appointed fiscal agent of the state prison at Greenwich, then a suburban village, to which the family removed.

At this early age he chanced to attract the attention of Amos Eaton, who soon afterwards became a well-known pioneer of natural science, and with whom it may be said that popular instruction in natural history in this country began. He taught young Torrey the structure of flowers and the rudiments of botany, and thus awakened a taste and kindled a zeal which were extinguished only with his pupil's life. This fondness soon extended to mineralogy and chemistry, and probably determined the choice of a profession. In the year 1815, Torrey began the study of medicine in the office of the eminent Dr. Wright Post, and in the College of Physicians and Surgeons, in which the then famous Dr. Mitchill and Dr. Hosack were professors of scientific repute; he took his medical degree in 1818; opened an office in his native city, and engaged in the practice of medicine with moderate success, turning the while his abundant leisure to scientific pursuits, especially to botany. In 1817, while yet a medical student,

he reported to the Lyceum of Natural History — of which he was one of the founders — his "Catalogue of the Plants growing spontaneously within Thirty Miles of the City of New York," which was published two years later; and he was already, or very soon after, in correspondence with Kurt Sprengel and Sir James Edward Smith abroad, as well as with Elliott, Nuttall, Schweinitz, and other American botanists. Two mineralogical articles were contributed by him to the very first volume of the "American Journal of Science and Arts" (1818–1819), and several others appeared a few years later, in this and in other journals.

Elliott's "Sketch of the Botany of South Carolina and Georgia" was at this time in course of publication, and Dr. Torrey planned a counterpart systematic work upon the botany of the northern States. The result of this was his "Flora of the Northern and Middle Sections of the United States," *i. e.*, north of Virginia, — which was issued in parts, and the first volume concluded in the summer of 1824. In this work Dr. Torrey first developed his remarkable aptitude for descriptive botany, and for the kind of investigation and discrimination, the tact and acumen, which it calls for. Only those few — now, alas, very few — surviving botanists who used this book through the following years can at all appreciate its value and influence. It was the fruit of those few but precious years which, seasoned with pecuniary privation, are in this country not rarely vouchsafed to an investigator, in which to prove his quality before he is haply overwhelmed with professional or professorial labors and duties.

In 1824, the year in which the first volume (or nearly half) of his Flora was published, he married Miss Eliza Robinson Shaw, of New York, and was established at West Point, having been chosen professor of chemistry, mineralogy, and geology in the United States Military Academy. Three years later he exchanged this chair for that of chemistry and botany (practically that of chemistry only, for botany had already been allowed to fall out of the medical curriculum in this country) in the College of Physicians and Surgeons, New York, then in Barclay Street. The Flora of the Northern

States was never carried further; although a Compendium, a pocket volume for the field, containing brief characters of the species which were to have been described in the second volume, along with an abridgment of the contents of the first, was issued in 1826. Moreover, long before Dr. Torrey could find time to go on with the work, he foresaw that the natural system was not much longer to remain, here and in England, an esoteric doctrine, confined to profound botanists, but was destined to come into general use and to change the character of botanical instruction. He was himself the first to apply it in this country in any considerable publication.

The opportunity for this, and for extending his investigations to the Great Plains and the Rocky Mountains on their western boundary, was furnished by the collections placed in Dr. Torrey's hands by Dr. Edwin James, the botanist of Major Long's expedition in 1820. This expedition skirted the Rocky Mountains belonging to what is now called Colorado Territory, where Dr. James, first and alone, reached the charming alpine vegetation, scaling one of the very highest summits, which from that time and for many years afterward was appropriately named James's Peak; although it is now called Pike's Peak, in honor of General Pike, who long before had probably seen, but had not reached it.

As early as the year 1823, Dr. Torrey communicated to the Lyceum of Natural History descriptions of some new species of James's collection, and in 1826 an extended account of all the plants collected, arranged under their natural orders. This is the earliest treatise of the sort in this country, arranged upon the natural system; and with it begins the history of the botany of the Rocky Mountains, if we except a few plants collected early in the century by Lewis and Clark, where they crossed them many degrees farther north, and which are recorded in Pursh's Flora. The next step in the direction he was aiming was made in the year 1831, when he superintended an American reprint of the first edition of Lindley's "Introduction to the Natural System of Botany," and appended a catalogue of the North American genera arranged according to it.

Dr. Torrey took an early and prominent part in the investigation of the United States species of the vast genus Carex, which has ever since been a favorite study in this country. His friend, Von Schweinitz, of Bethlehem, Pennsylvania, placed in his hands and desired him to edit, during the author's absence in Europe, his "Monograph of North American Carices." It was published in the "Annals of the New York Lyceum," in 1825, much extended, indeed almost wholly rewritten, and so much to Schweinitz's satisfaction that he insisted that this classical monograph "should be considered and quoted in all respects as the joint production of Dr. Torrey and himself." Ten or eleven years later, in the succeeding volume of the "Annals of the New York Lyceum," appeared Dr. Torrey's elaborate Monograph of the other North American *Cyperaceæ*, with an appended revision of the Carices, which meanwhile had been immensely increased by the collections of Richardson, Drummond, etc., in British and arctic America. A full set of these was consigned to his hands for study (along with other important collections), by his friend Sir William Hooker, upon the occasion of a visit which he made to Europe in 1833. But Dr. Torrey generously turned over the Carices to the late Professor Dewey, whose rival Caricography is scattered through forty or fifty volumes of the "American Journal of Science and Arts"; and so had only to sum up the results in this regard, and add a few southern species at the close of his own monograph of the order.

About this time, namely, in the year 1836, upon the organization of a geological survey of the State of New York upon an extensive plan, Dr. Torrey was appointed botanist, and was required to prepare a Flora of the State. A laborious undertaking it proved to be, involving a heavy sacrifice of time, and postponing the realization of long-cherished plans. But in 1843, after much discouragement, the "Flora of the State of New York," the largest if by no means the most important of Dr. Torrey's works, was completed and published, in two large quarto volumes, with one hundred and sixty-one plates. No other State of the Union has produced a Flora to

compare with this. The only thing to be regretted is that it interrupted, at a critical period, the prosecution of a far more important work.

Early in his career Dr. Torrey had resolved to undertake a general Flora of North America, or at least of the United States, arranged upon the natural system, and had asked Mr. Nuttall to join him, who, however, did not consent. At that time, when little was known of the regions west of the valley of the Mississippi, the ground to be covered and the materials at hand were of comparatively moderate compass; and in aid of the northern part of it, Sir William Hooker's Flora of British America — founded upon the rich collections of the arctic explorers, of the Hudson's Bay Company's intelligent officers, and of such hardy and enterprising pioneers as Drummond and Douglas — was already in progress. At the actual inception of the enterprise, the botany of eastern Texas was opened by Drummond's collections, as well as that of the coast of California by those of Douglas, and afterwards those of Nuttall. As they clearly belonged to our own phyto-geographical province, Texas and California were accordingly annexed botanically before they became so politically.

While the field of botanical operations was thus enlarging, the time which could be devoted to it was restricted. In addition to his chair in the Medical College, Dr. Torrey had felt obliged to accept a similar one at Princeton College, and to all was now added, as we have seen, the onerous post of state botanist. It was in the year 1836 or 1837 that he invited the writer of this notice — then pursuing botanical studies under his auspices and direction — to become his associate in the Flora of North America. In July and in October, 1838, the first two parts, making half of the first volume, were published. The great need of a full study of the sources and originals of the earlier published species was now apparent; so, during the following year, his associate occupied himself with this work in the principal herbaria of Europe. The remaining half of the first volume appeared in June, 1840. The first part of the second volume followed in 1841; the second, in the spring of 1842; and in February, 1843,

came the third and the last; for Dr. Torrey's associate was now also immersed in professorial duties and in the consequent preparation of the works and collections which were necessary to their prosecution.

From that time to the present the scientific exploration of the vast interior of the continent has been actively carried on, and in consequence new plants have poured in year by year in such numbers as to overtask the powers of the few working botanists of the country, nearly all of them weighted with professional engagements. The most they could do has been to put collections into order in special reports, revise here and there a family or a genus monographically, and incorporate new materials into older parts of the fabric, or rough-hew them for portions of the edifice yet to be constructed. In all this Dr. Torrey took a prominent part down almost to the last days of his life. Passing by various detached and scattered articles upon curious new genera and the like, but not forgetting three admirable papers published in the "Smithsonian Contributions to Knowledge" (Plantæ Fremontianæ, and those on Batis and Darlingtonia), there is a long series of important, and some of them very extensive, contributions to the reports of government explorations of the western country, — from that of Long's expedition, already referred to, in which he first developed his powers, through those of Nicollet, Fremont, and Emory, Sitgreaves, Stansbury, and Marcy, and those contained in the ampler volumes of the Surveys for Pacific Railroad routes, down to that of the Mexican Boundary, the botany of which forms a bulky quarto volume, of much interest. Even at the last, when he rallied transiently from the fatal attack, he took in hand the manuscript of an elaborate report on the plants collected along our Pacific coast in Admiral Wilkes's celebrated expedition, which he had prepared fully a dozen years ago, and which (except as to the plates) remains still unpublished through no fault of his. There would have been more to add, perhaps of equal importance, if Dr. Torrey had been as ready to complete and publish, as he was to investigate, annotate, and sketch. Through undue diffidence and a constant desire for a greater perfec-

tion than was at the time attainable, many interesting observations have from time to time been anticipated by other botanists.

All this botanical work, it may be observed, has reference to the Flora of North America, in which, it was hoped, the diverse and separate materials and component parts, which he and others had wrought upon, might some day be brought together in a completed system of American botany.

It remains to be seen whether his surviving associate of nearly forty years will be able to complete the edifice. To do this will be to supply the most pressing want of the science, and to raise the fittest monument to Dr. Torrey's memory.

In the estimate of Dr. Torrey's botanical work, it must not be forgotten that it was nearly all done in the intervals of a busy professional life; that he was for more than thirty years an active and distinguished teacher, mainly of chemistry, and in more than one institution at the same time; that he devoted much time and remarkable skill and judgment to the practical applications of chemistry, in which his counsels were constantly sought and too generously given; that when, in 1857, he exchanged a portion, and a few years later the whole, of his professional duties for the office of United States Assayer, these requisitions upon his time became more numerous and urgent.[1] In addition to the ordinary duties of his office, which he fulfilled to the end with punctilious faithfulness (signing the last of his daily reports upon the very day of his death, and quietly telling his son and assistant that it would not be necessary to bring him any more), he was frequently requested by the head of the Treasury Department to undertake the solution of difficult problems, especially those relating to counterfeiting, or to take charge of some delicate or confidential commission, the utmost reliance being placed upon his skill, wisdom, and probity.

[1] It ought to be added, that, when the government Assay Office at New York was established, the Secretary of the Treasury selected Dr. Torrey to be its superintendent, — which would have given to the establishment the advantage of a scientific head. But Dr. Torrey resolutely declined the less laborious and better paid post, and took in preference one the emoluments of which were much below his worth and the valuable extraneous services he rendered to the government, — simply because he was unwilling to accept the care and responsibility of treasure.

In two instances these commissions were made personally gratifying, not by pecuniary payment, which, beyond his simple expenses, he did not receive, but by the opportunity they afforded to recruit failing health and to gather floral treasures. Eight years ago he was sent by the Treasury Department to California, by way of the Isthmus; and last summer he went again across the continent, and in both cases enjoyed the rare pleasure of viewing in their native soil, and plucking with his own hands, many a flower which he had himself named and described from dried specimens in the herbarium, and in which he felt a kind of paternal interest. Perhaps this interest culminated last summer, when he stood on the flank of the lofty and beautiful snow-clad peak to which a grateful former pupil and ardent explorer, ten years before, gave his name, and gathered charming alpine plants which he had himself named forty years before, when the botany of the Colorado Rocky Mountains was first opened. That age and fast-failing strength had not dimmed his enjoyment, may be inferred from his remark when, on his return from Florida the previous spring, with a grievous cough allayed, he was rallied for having gone to seek Ponce de Leon's Fountain of Youth. "No," said he, "give me the fountain of old age. The longer I live, the more I enjoy life." He evidently did so. If never robust, he was rarely ill, and his last sickness brought little suffering, and no diminution of his characteristic cheerfulness. To him, indeed, never came the "evil days" of which he could say, "I have no pleasure in them."

Evincing in age much of the ardor and all of the ingenuousness of youth, he enjoyed the society of young men and students, and was helpful to them long after he ceased to teach, — if, indeed, he ever did cease. For, as Emeritus Professor in Columbia College (with which his old Medical School was united), he not only opened his herbarium, but gave some lectures almost every year, and as a trustee of the college for many years he rendered faithful and important service. His large and truly invaluable herbarium, along with a choice botanical library, he several years ago made over to Columbia College, which charges itself with its safe preservation and maintenance.

Dr. Torrey leaves three daughters, a son, who has been appointed United States assayer in his father's place, and a grandson.

This sketch of Dr. Torrey's public life and works, which it is our main duty to exhibit, would fall short of its object if it did not convey, however briefly and incidentally, some just idea of what manner of man he was. That he was earnest, indefatigable, and able, it is needless to say. His gifts as a teacher were largely proved and are widely known through a long generation of pupils. As an investigator he was characterized by a scrupulous accuracy, a remarkable fertility of mind, especially as shown in devising ways and means of research, and perhaps by some excess of caution.

Other biographers will doubtless dwell upon the more personal aspects and characteristics of our distinguished and lamented associate. To them, indeed, may fittingly be left the full delineation and illustration of the traits of a singularly transparent, genial, delicate, and conscientious, unselfish character, which beautified and fructified a most industrious and useful life, and won the affection of all who knew him. For one thing, they cannot fail to notice his thorough love of truth for its own sake, and his entire confidence that the legitimate results of scientific inquiry would never be inimical to the Christian religion, which he held with an untroubled faith, and illustrated, most naturally and unpretendingly, in all his life and conversation. In this, as well as in the simplicity of his character, he much resembled Faraday.

Dr. Torrey was an honorary or corresponding member of a goodly number of the scientific societies of Europe, and was naturally connected with all prominent institutions of the kind in this country. He was chosen into the American Academy in the year 1841. He was one of the corporate members of the National Academy at Washington. He presided in his turn over the American Association for the Advancement of Science; and he was twice, for considerable periods, president of the New York Lyceum of Natural History, which was in those days one of the foremost of our scientific societies. It has been said of him that the sole

distinction on which he prided himself was his membership in the order of the Cincinnati, the only honor in this country which comes by inheritance.

As to the customary testimonial which the botanist receives from his fellows, it is fortunate that the first attempts were nugatory. Almost in his youth a genus was dedicated to him by his correspondent, Sprengel: this proved to be a Clerodendron, misunderstood. A second, proposed by Rafinesque, was founded on an artificial dismemberment of Cyperus. The ground was clear, therefore, when, thirty or forty years ago, a new and remarkable evergreen tree was discovered in our own southern States, which it was at once determined should bear Dr. Torrey's name. More recently a congener was found in the noble forests of California. Another species had already been recognized in Japan, and lately a fourth in the mountains of northern China. All four of them have been introduced, and are greatly prized as ornamental trees in Europe. So that, all round the world, *Torreya taxifolia*, *Torreya Californica*, *Torreya nucifera*, and *Torreya grandis* — as well as his own important contributions to botany, of which they are a memorial — should keep our associate's memory as green as their own perpetual verdure.

WILLIAM STARLING SULLIVANT.[1]

William Starling Sullivant, LL. D., died at his residence in Columbus, Ohio, on the 30th of April, ultimo. In him we lose the most accomplished bryologist which this country has produced; and it can hardly be said that he leaves behind him anywhere a superior.

He was born, January 15, 1803, at the little village of Franklinton, then a frontier settlement in the midst of primitive forest, near the site of the present city of Columbus. His father, a Virginian, and a man of marked character, was appointed by government to survey the lands of that district of the Northwestern Territory which became the central part of the now populous State of Ohio; and he early purchased a large tract of land, bordering on the Scioto River, near by, if not including, the locality which was afterwards fixed upon for the state capital.

William, his eldest son, in his boyhood, if he endured some of the privations, yet enjoyed the advantages of this frontier life, in the way of physical training and early self-reliance. But he was sent to school in Kentucky; he received the rudiments of his classical education at the so-called Ohio University at Athens, upon the opening of that seminary; and was afterward transferred to Yale College, where he was graduated in the year 1823. His plans for studying a profession were frustrated by the death of his father in that year. This required him to occupy himself with the care of the family property, then mainly in lands, mills, etc., and demanding much and varied attention. He became surveyor and practical engineer, and indeed took an active part in business down to a recent period. Leisure is hardly to be had in a

[1] Proceedings of the American Academy of Arts and Sciences, ix. 271. (1873.)

newly settled country, and least of all by those who have possessions. Mr. Sullivant must have reached the age of nearly thirty years, and, having married early,[1] was established in his suburban residence in a rich floral district, before his taste for natural history was at all developed. His brother Joseph, next in age, was already somewhat proficient in botany as well as in conchology and ornithology; and when in some way his own interest in the subject was at length excited, he took it up with characteristic determination to know well whatever he undertook to know at all. He collected and carefully studied the plants of the central part of Ohio, made neat sketches of the minuter parts of many of them, especially of the Grasses and Sedges, entered into communication with the leading botanists of the country, and in 1840 he published "A Catalogue of Plants, Native or Naturalized, in the vicinity of Columbus, Ohio," (pp. 63,) to which he added a few pages of valuable notes. His only other direct publication in Phænogamous botany is a short article upon three new plants which he had discovered in that district, contributed to the "American Journal of Science and Arts," in the year 1842. The observations which he continued to make were communicated to his correspondents and friends, the authors of the Flora of North America, then in progress. As soon as the flowering plants of his district had ceased to afford him novelty, he turned to the Mosses, in which he found abundant scientific occupation, of a kind well suited to his bent for patient and close observation, scrupulous accuracy, and nice discrimination. His first publication in his chosen department, the "Musci Alleghanienses," was accompanied by the specimens themselves of Mosses and *Hepaticæ* collected in a botanical expedition through the Alleghany Mountains from Maryland to Georgia, in the summer of 1843, the writer of this notice being his companion. The specimens were not only critically determined, but exquisitely prepared and mounted, and with letter-press of great perfection; the whole forming two quarto volumes, which well deserve the encomium bestowed

[1] His first wife, Jane Marshall of Kentucky, was a niece of Chief Justice Marshall. She died a few years after marriage.

by Pritzel in his Thesaurus.[1] It was not put on sale, but fifty copies were distributed with a free hand among bryologists and others who would appreciate it.[2]

In 1846, Mr. Sullivant communicated to the American Academy the first part, and in 1849, the second part of his "Contributions to the Bryology and Hepaticology of North America," which appeared, one in the third, the other in the fourth volume (new series) of the Academy's Memoirs, each with five plates, from the author's own admirable drawings. These plates were engraved at his own expense, and were generously given to the Academy.

When the second edition of Gray's "Manual of the Botany of the Northern United States" was in preparation, Mr. Sullivant was asked to contribute to it a compendious account of the *Musci* and *Hepaticæ* of the region; which he did, in the space of about one hundred pages, generously adding, at his sole charge, eight copperplates crowded with illustrations of the details of the genera, — thus enhancing vastly the value of his friend's work, and laying a foundation for the general study of bryology in the United States, which then and thus began.

So excellent are these illustrations, both in plan and execution, that Schimper, then the leading bryologist of the Old World, and a most competent judge, since he has published hundreds of figures in his "Bryologia Europæa," not only adopted the same plan in his "Synopsis of the European Mosses," but also the very figures themselves (a few of which were, however, originally his own), whenever they would serve his purpose, as was the case with most of them. A separate edition was published of this portion of the Manual, under the title of "The Musci and Hepaticæ of the United States east

[1] "Huic splendidæ impressæ 292 specierum enumerationi accedit elegantissima speciminum omnium exsiccatorum collectio."

[2] A tribute is justly due to the memory of the second Mrs. (Eliza G. Wheeler) Sullivant, a lady of rare accomplishments, and, not least, a zealous and acute bryologist, her husband's efficient associate in all his scientific work until her death, of cholera, in 1850 or 1851. Her botanical services are commemorated in *Hypnum Sullivantiæ* of Schimper, a new Moss of Ohio.

of the Mississippi River" (New York, 1856, imperial 8vo), upon thick paper, and with proof-impressions directly from the copperplates. This exquisite volume was placed on sale at far less than its cost, and copies are now of great rarity and value. It was with regret that the author of the Manual omitted this Cryptogamic portion from the ensuing editions, and only with the understanding that a separate "Species Muscorum," or Manual for the Mosses of the whole United States, should replace it. This most needful work Mr. Sullivant was just about to prepare for the press.

About the same time that Mr. Sullivant thus gave to American students a text-book for our Mosses, he provided an unequaled series of named specimens for illustrating them. The ample stores which he had collected or acquired, supplemented by those collected by M. Lesquereux (who was associated with him from the year 1848) in a journey through the mountainous parts of the southern States under his auspices, after critical determination were divided into fifty sets, each of about three hundred and sixty species or varieties, with printed tickets, title, index, etc., and all except a few copies for gratuitous distribution were generously made over, to be sold at less than cost, for his esteemed associate's benefit, and still more for that of the botanists and institutions who could thus acquire them. The title of this classical work and collection is "Musci Boreali Americani quorum specimina exsiccati ediderunt W. S. Sullivant et L. Lesquereux; 1856." Naturally enough the edition was immediately taken up.

In 1865 it was followed by a new one, or rather a new work, of between five and six hundred numbers, many of them Californian species, the first-fruits of Dr. Bolander's researches in that country. The sets of this unequaled collection were disposed of with the same unequaled liberality, and with the sole view of advancing the knowledge of his favorite science. This second edition being exhausted, he recently and in the same spirit aided his friend Mr. Austin, both in the study and in the publication of his extensive "Musci Appalachiani."

To complete here the account of Mr. Sullivant's bryological labors illustrated by "exsiccati," we may mention his "Musci

Cubenses," named, and the new species described in 1861, from Charles Wright's earlier collections in Cuba, and distributed in sets by the collector. His researches upon later and more extensive collections by Mr. Wright remain in the form of notes and pencil sketches, in which many new species are indicated. The same may be said of an earlier still unpublished collection, made by Fendler in Venezuela. Another collection, of great extent and interest, which was long ago elaborately prepared for publication, and illustrated by very many exquisite drawings, rests in his portfolios, through delays over which Mr. Sullivant had no control; namely, the Bryology of Rodgers's United States North Pacific Exploring Expedition, of which Charles Wright was botanist. Brief characters of the principal new species were, however, duly published in this as in other departments of the botany of that expedition. It is much to be regretted that the drawings which illustrate them have not yet been engraved and given to the scientific world.

This has fortunately been done in the case of the South Pacific Exploring Expedition, under Commodore Wilkes. For, although the volume containing the Mosses has not even yet been issued by government, Mr. Sullivant's portion of it was published in a separate edition in the year 1859. It forms a sumptuous imperial folio, the letter-press having been made up into large pages, and printed on paper which matches the plates, twenty-six in number.

One volume of the Pacific Railroad Reports, *i. e.* the fourth, contains a paper by Mr. Sullivant, being his account of the Mosses collected in Whipple's Exploration. It consists of only a dozen pages of letter-press, but is illustrated by ten admirable plates of new species.

The "Icones Muscorum," however, is Mr. Sullivant's crowning work. It consists, as the title indicates, of "Figures and Descriptions of most of those Mosses peculiar to eastern North America which have not been heretofore figured," and forms an imperial octavo volume, with one hundred and twenty-nine copperplates, published in 1864. The letter-press and the plates (upon which last alone several

thousand dollars and immense pains were expended) are simply exquisite and wholly unrivaled; and the scientific character is acknowledged to be worthy of the setting. Within the last few years most of the time which Mr. Sullivant could devote to science has been given to the preparation of a second or supplementary volume of the Icones. The plates, it is understood, are completed, the descriptions in a good degree written out, and the vernal months in which his mortal life closed were to have been devoted to the printing. The Manual of North American Mosses was speedily to follow.

He was remarkably young for his years, so that the hopes and expectations in which we were indulging seemed reasonable. But in January, not far from his seventieth birthday, he was prostrated by pneumonia, from the consequences of which, after some seeming convalescence, he died upon the last day of April. He leaves a wife, Mrs. Caroline E. (Sutton) Sullivant, children, grandchildren, and great-grandchildren, to inherit a stainless and honored name, and to cherish a noble memory.

In personal appearance and carriage, no less than in all the traits of an unselfish and well-balanced character, Mr. Sullivant was a fine specimen of a man. He had excellent business talents, and was an exemplary citizen; he had a refined and sure taste, and was an accomplished draughtsman. But after having illustrated his earlier productions with his own pencil, he found that valuable time was to be gained by employing a trained artist. He discovered in Mr. A. Schrader a hopeful draughtsman, and he educated him to the work, with what excellent results the plates of the Icones and of his other works abundantly show. As an investigator he worked deliberately, slowly indeed and not continuously, but perseveringly. Having chosen his particular department, he gave himself undeviatingly to its advancement. His works have laid such a broad and complete foundation for the study of bryology in this country, and are of such recognized importance everywhere, that they must always be of classical authority; in fact, they are likely to remain for a long time

unrivaled. Wherever Mosses are studied, his name will be honorably remembered; in this country it should long be remembered with peculiar gratitude.

In accordance with his wishes, his bryological books and his exceedingly rich and important collections and preparations of Mosses are to be consigned to the Gray Herbarium of Harvard University, with a view to their preservation and long-continued usefulness. The remainder of his botanical library, his choice microscopes, and other collections are bequeathed to the State Scientific and Agricultural College, just established at Columbus, and to the Starling Medical College, founded by his uncle, of which he was himself the senior trustee.

Mr. Sullivant was chosen into the American Academy in the year 1845. He received the honorary degree of Doctor of Laws from Gambier College, in his native State. His oldest botanical associates long ago enjoyed the pleasure of bestowing the name of *Sullivantia Ohionis* upon a very rare and interesting but modest and neat Saxifragaceous plant, which he himself discovered in his native State, on the secluded banks of a tributary of the river which flows by the place where he was born, and where his remains now repose.

JEFFRIES WYMAN.[1]

When we think of the associate and friend whose death this Society now deplores, and remember how modest and retiring he was, how averse to laudation and reticent of words, we feel it becoming to speak of him now that he is gone, with much of the reserve which would be imposed upon us if he were living. Yet his own perfect truthfulness and nice sense of justice, and the benefit to be derived from the contemplation of such a character by way of example, may be our warrant for reasonable freedom in the expression of our judgments and our sentiments, taking care to avoid all exaggeration.

Appropriate and sincere eulogies and expressions of loss, both official and personal, have, however, already been pronounced or published; and among them one from the governors of that institution to which, together with our own Society, most of Professor Wyman's official life and services were devoted, — which appears to me to delineate in the fewest words the truest outlines of his character. In it the President and Fellows of Harvard University "recall with affectionate respect and admiration the sagacity, patience, and rectitude which characterized all his scientific work, his clearness, accuracy, and conciseness as a writer and teacher, and the industry and zeal with which he labored upon the two admirable collections which remain as monuments of his rare knowledge, method, and skill. They commend to the young men of the University this signal example of a character modest, tranquil, dignified, and independent, and of a life simple, contented, and honored."

What more can be or need be said? It is left for me, in

[1] An address delivered at a memorial meeting of the Boston Society of Natural History, October 7, 1874.

compliance with your invitation, Mr. President, to say something of what he was to us, and has done for us, and to put upon record, for the use of those who come after us, some account of his uneventful life, some notice, however imperfect, of his work and his writings. I could not do this without the help of friends who knew him well in early life, and of some of you who are much more conversant than I am with most of his researches. Such aid, promptly rendered, has been thankfully accepted and freely used.

Our associate's father, Dr. Rufus Wyman, — born in Woburn, graduated at Harvard College in 1799, and in the latter part of his life physician to the McLean Asylum for the Insane, — was a man of marked ability and ingenuity. Called to the charge of this earliest institution of the kind in New England at its beginning, he organized the plan of treatment and devised excellent mechanical arrangements, which have since been developed, and introduced into other establishments of the kind. His mother was Ann Morill, daughter of James Morill, a Boston merchant. This name is continued, and is familiar to us, in that of our associate's elder brother.

Jeffries Wyman, the third son, derived his baptismal name from the distinguished Dr. John Jeffries of Boston, under whom his father studied medicine. He was born on the 11th of August, 1814, at Chelmsford, a township of a few hundred inhabitants in Middlesex County, Massachusetts, not far from the present city of Lowell. As his father took up his residence at the McLean Asylum in 1818, when Jeffries was only four years old, he received the rudiments of his education at Charlestown, in a private school, but afterwards went to the Academy at Chelmsford, and in 1826 to Philips Exeter Academy, where, under the instruction of Dr. Abbot, he was prepared for college. He entered Harvard College in 1829, the year in which Josiah Quincy took the presidency, and was graduated in 1833, in a class of fifty-six, six of whom became professors in the University. He was not remarkable for general scholarship, but was fond of chemistry, and his preference for anatomical studies was already developed. Some

of his classmates remember the interest which was excited among them by a skeleton which he made of a mammoth bull-frog from Fresh Pond, probably one which is still preserved in his museum of comparative anatomy. His skill and taste in drawing, which he turned to such excellent account in his investigations and in the lecture room, as well as his habit of close observation of natural objects met with in his strolls, were manifested even in boyhood.

An attack of pneumonia during his senior year in college caused much anxiety, and perhaps laid the foundation of the pulmonary affection which burdened and finally shortened his life. To recover from the effects of the attack, and to guard against its return, he made, in the winter of 1833–34, the first of those pilgrimages to the coast of the southern States, which in later years were so often repeated. Returning with strength renewed in the course of the following spring, he began the study of medicine under Dr. John C. Dalton, who had succeeded to his father's practice at Chelmsford, but who soon removed to the adjacent and thriving town of Lowell. Here, and with his father at the McLean Asylum and at the Medical College in Boston, he passed two years of profitable study. At the commencement of the third year he was elected house-student in the medical department, at the Massachusetts General Hospital, — then under the charge of Drs. James Jackson, John Ware, and Walter Channing, — a responsible position, not only most advantageous for the study of disease, but well adapted to sharpen a young man's power of observation.

In 1837, after receiving the degree of Doctor of Medicine, he cast about among the larger country towns for a field in which to practise his profession. Fortunately for science he found no opening to his mind; so he took an office in Boston, on Washington Street, and accepted the honorable but far from lucrative post of demonstrator of anatomy under Dr. John C. Warren, the Hersey professor. His means were very slender, and his life abstemious to the verge of privation; for he was unwilling to burden his father, who indeed had done all he could in providing for the education of two sons.

It may be interesting to know, that, to eke out his subsistence, he became at this time a member of the Boston Fire Department, under an appointment of Samuel A. Eliot, Mayor, dated September 1, 1838. He was assigned to Engine No. 18. The rule was that the first-comer to the engine-house should bear the lantern, and be absolved from other work. Wyman lived near by, and his promptitude generally saved him from all severer labor than that of enlightening his company.

The turning-point in his life, *i. e.*, an opportunity which he could seize of devoting it to science, came when Mr. John A. Lowell offered him the curatorship of the Lowell Institute, just brought into operation, and a course of lectures in it. He delivered his course of twelve lectures upon comparative anatomy and physiology in the winter of 1840–41 ; and with the money earned by this first essay in instructing others, he went to Europe to seek further instruction for himself. He reached Paris in May, 1841, and gave his time at once to human anatomy at the School of Medicine, and comparative anatomy and natural history at the Garden of Plants, attending the lectures of Flourens, Majendie, and Longet on physiology, and of De Blainville, Isidore St. Hilaire, Valenciennes, Duméril, and Milne-Edwards on zoölogy and comparative anatomy. In the summer, when the lectures were over, he made a pedestrian journey along the banks of the Loire, and another along the Rhine, returning through Belgium, and by steamer to London. There, while engaged in the study of the Hunterian collections at the Royal College of Surgeons, he received information of the alarming illness of his father ; he immediately turned his face homeward, but on reaching Halifax he learned that his father was no more.

He resumed his residence in Boston, and devoted himself mainly to scientific work, under circumstances of no small discouragement. But in 1843 the means of a modest professional livelihood came to him in the offer of the chair of anatomy and physiology in the medical department of Hampden-Sidney College, established at Richmond, Virginia. One advantage of this position was that it did not interrupt his

residence in Boston except for the winter and spring; and during these months the milder climate of Richmond was even then desirable. He discharged the duties of the chair most acceptably for five sessions, until in 1847 he was appointed to succeed Dr. Warren as Hersey professor of anatomy in Harvard College, the Parkman professorship in the Medical School in Boston being filled by the present incumbent, Dr. Holmes. Thus commenced Professor Wyman's most useful and honorable connection as a teacher with the University, of which the President and Fellows speak in the terms I have already recited. He began his work in Holden Chapel, the upper floor being the lecture room, the lower containing the dissecting room and the anatomical museum of the college, with which he combined his own collections and preparations, which from that time forward increased rapidly in number and value under his industrious and skillful hands. At length Boylston Hall was built for the anatomical and the chemical departments, and the museum, lecture and working rooms were established commodiously in their present quarters; and Professor Wyman's department assumed the rank and importance which it deserved. Both human and comparative anatomy were taught to special pupils, some of whom have proved themselves worthy of their honored master, while the annual courses of lectures and lessons on anatomy, physiology, and for a time the principles of zoölogy, imparted highly valued instruction to undergraduates and others.

In the formation and perfecting of his museum — the first of the kind in the country, arranged upon a plan both physiological and morphological — no pains and labors were spared, and long and arduous journeys and voyages were made to contribute to its riches. In the summer of 1849, — having replenished his frugal means with the proceeds of a second course of lectures before the Lowell Institute (namely, upon comparative physiology, a good condensed shorthand report of which was published at the time), — he accompanied Captain Atwood of Provincetown, in a small sloop, upon a fishing voyage high up the coast of Labrador. In the winter of 1852, going to Florida for his health, he began his fruitful series of

explorations and collections in that interesting district. In 1854, accompanied by his wife, he traveled extensively in Europe, and visited all the museums within his reach. In the spring of 1856, with his pupils Green and Bancroft as companions and assistants, he sailed to Surinam, penetrated far into the interior in canoes, made important researches upon the ground, and enriched his museum with some of its most interesting collections. These came near being too dearly bought, as he and his companions took the fever of the country, from which he suffered severely, and recovered slowly. Again, in 1858–9, accepting the thoughtful and generous invitation of Captain J. M. Forbes, he made a voyage to the La Plata, and ascended the Uruguay and the Parana in a small iron steamer which Captain Forbes brought upon the deck of his vessel; then, with his friend George Augustus Peabody as a companion, he crossed the pampas to Mendosa, and the Cordilleras to Santiago and Valparaiso, whence he came home by way of the Peruvian coast and the Isthmus.

By such expeditions many of the choice materials of his museum and of his researches were gathered, at his own expense, to be carefully prepared and elaborated by his own unaided hands. A vast neighboring museum is a splendid example of what munificence, called forth by personal enthusiasm, may accomplish. In Dr. Wyman's we have an example of what one man may do, unaided, with feeble health and feebler means, by persistent and well directed industry, without eclat, and almost without observation. . While we duly honor those who of their abundance cast their gifts into the treasury of science, let us not — now that he cannot be pained by our praise — forget to honor one who in silence and penury cast in more than they all.

Of penury in a literal sense we may not speak: for although Professor Wyman's salary, derived from the Hersey endowment, was slender indeed, he adapted his wants to his means, foregoing neither his independence nor his scientific work; and I suppose no one ever heard him complain. In 1856 came unexpected and honorable aid from two old friends of his father, who appreciated the son, and wished him to go on

with his scientific work without distraction. One of them, the late Dr. William J. Walker, sent him ten thousand dollars outright; the other, the late Thomas Lee, who had helped in his early education, supplemented the endowment of the Hersey professorship with an equal sum, stipulating that the income thereof should be paid to Professor Wyman during life, whether he held the chair or not. Seldom, if ever, has a moderate sum produced a greater benefit.

Throughout the later years of Professor Wyman's life a new museum has claimed his interest and care, and is indebted to him for much of its value and promise. In 1866, when failing strength demanded a respite from oral teaching, and required him to pass most of the season for it in a milder climate, he was named by the late George Peabody one of the seven trustees of the museum and professorship of American archæology and ethnology, which this philanthropist proceeded to found in Harvard University; and his associates called upon him to take charge of the establishment. For this he was peculiarly fitted by all his previous studies, and by his predilection for ethnological inquiries. These had already engaged his attention, and to this class of subjects he was thereafter mainly devoted, — with what sagacity, consummate skill, untiring diligence and success, his seventh annual report, — the last published just before he died, — his elaborate memoir on shell-heaps, now printing, and especially the Archæological Museum in Boylston Hall, abundantly testify. If this museum be a worthy memorial of the founder's liberality and foresight, it is no less a monument to Wyman's rare ability and devotion. Whenever the enduring building which is to receive it shall be erected, surely the name of its first curator and organizer should be inscribed, along with that of the founder, over its portal.

Of Professor Wyman's domestic life, let it here suffice to record, that in December, 1850, he married Adeline Wheelwright, who died in June, 1855, leaving two daughters; that in August, 1861, he married Anna Williams Whitney, who died in February, 1864, shortly after the birth of an only and a surviving son.

Of his later days, of the slow yet all too rapid progress of fatal pulmonary disease, it is needless to protract the story. Winter after winter, as he exchanged our bleak climate for that of Florida, we could only hope that he might return. Spring after spring he came back to us invigorated, thanks to the bland air and to open life in boat and tent, which acted like a charm; — thanks, too, to the watchful care of his attached friend, Mr. Peabody, his constant companion in Florida life. One winter was passed in Europe, partly in reference to the Archæological Museum, partly in hope of better health; but no benefit was received. The past winter in Florida produced the usual amelioration, and the amount of work which Dr. Wyman undertook and accomplished last summer might have tasked a robust man. There were important accessions to the archæological collections, upon which much labor, very trying to ordinary patience, had to be expended. And in the last interview I had with him, he told me that he had gone through his own museum of comparative anatomy, which had somewhat suffered in consequence of the alterations in Boylston Hall, and had put the whole into perfect order. It was late in August when he left Cambridge for his usual visit to the White Mountain region, by which he avoided the autumnal catarrh; and there, at Bethlehem, New Hampshire, on the 4th of September, a severe hemorrhage from the lungs suddenly closed his valuable life.

Let us turn to his relations with this society. He entered it in October, 1837, just thirty-seven years ago, and shortly after he had taken his degree of Doctor in Medicine. He was recording secretary from 1839 to 1841; curator of ichthyology and herpetology from 1841 to 1847; of herpetology from 1847 to 1845; of comparative anatomy from 1855 to 1874. While in these later years his duties may have been almost nominal, it should be remembered that in the earlier days a curator not only took charge of his portion of the museum, but in a great degree created it. Then for fourteen years, from 1856 to 1870, he was the president of this society, as assiduous in all its duties as he was wise in council; and he resigned the chair which he so long adorned and digni-

fied, only when the increasing delicacy of his health, to which night exposure was prejudicial, made it unsafe for him any longer to undertake its duties. The record shows that he has made here one hundred and five scientific communications, several of them very important papers, every one of some positive value; for you all know that Professor Wyman never spoke or wrote except to a direct purpose, and because there was something which it was worth while to communicate. He bore his part also in the American Academy of Arts and Sciences, of which he was a Fellow from the year 1843, and for many years a councilor. To it he made a good number of communications; among them one of the longest and ablest of his memoirs.

Then he was from the first a member of the faculty of the Museum of Comparative Zoölogy, where his services and his advice were highly valued. He was chosen president of the American Association for the Advancement of Science for the year 1857, but did not assume the duties of the office.

Some notice — brief and cursory though it must be — of such portion of Dr. Wyman's scientific work as is recorded in his published papers, should form a part of this account of his life.

His earliest publication, so far as we know, was an article in the "Boston Medical and Surgical Journal," in 1837, signed only with the initials of his name. It is upon "The indistinctness of images formed from oblique rays of light," and the cause of it. The handling of the subject is as characteristic as that of any later paper. In January, 1841, we find his first recorded communication of this society, "On the Cranium of a Seal." The first to the American Academy is the account of his dissection of the electrical organs of a new species of Torpedo, in 1843, part of a paper by his friend Dr. Storer, published in Silliman's Journal. In the course of that year, eleven communications were made to our society, beside the annual address, which he delivered on the 17th of May. The most important of these was the memoir, by Dr. Savage and himself, on the Black Orang or Chimpanzee of Africa, *Troglodytes niger*, published in full in the Journal of

this society, the anatomical part by Professor Wyman. Two other papers of that early year, on the anatomy of two Mollusca, *Tebennophorus Carolinensis* and *Glandina truncata*, published in the fourth volume of the society's Journal, each with a copper plate, are noteworthy, as showing that he possessed from the first that happy faculty of clear, terse, and closely relevant exposition, and that skill and neatness of illustration with his pencil, which characterize all his work, both of research and instruction.

Another paper of that year, "On the microscopic structure of the teeth of the *Lepidostei*, and their analogies with those of the *Labyrinthodonts*," read to this society in August, and published in Silliman's Journal in October, 1843, was important and timely. In it he demonstrated that the Labyrinthine structure of the teeth, considered at the time to be peculiar to certain sauroid reptiles, equally belong to gar-fishes, and consequently that many fossil teeth which had been referred by the evidence of this character alone to a group of reptiles founded upon this peculiarity, might as well belong to ancient sauroid fishes.

Although not of any importance now to remember, I may here mention his report to this society on the *Hydrachos Sillimani* of Koch, a factitious Saurian of huge length, successfully exhibited in New York and elsewhere under high auspices, and I think also in Germany, but which Dr. Wyman exposed at sight, showing that it was made up of an indefinite number of various cetaceous vertebræ, belonging to many individuals, which (as was afterward ascertained) were collected from several localities.

But the memoir by which Professor Wyman assured his position among the higher comparative anatomists was that communicated to and published by this society in the summer of 1847, in which the Gorilla was first named and introduced to the scientific world, and the distinctive structure and affinities of the animal so thoroughly made out from the study of the skeleton, that there was, as the great English anatomist remarked, "very little left to add, and nothing to correct." In this memoir the "Description of the habits of *Troglodytes*

Gorilla" is by Dr. Thomas S. Savage, to whom, along with Dr. Wilson, "belongs the credit of the discovery;" the Osteology of the same and the introductory history are by Dr. Wyman. Indeed, nearly all since made known of the Gorilla's structure, and of the affinities soundly deduced therefrom, has come from our associate's subsequent papers, founded on additional crania brought to him in 1849, by Dr. George A. Perkins of Salem; on a nearly entire male skeleton of unusual size, received in 1852 from the Rev. William Walker, and now in Wyman's museum; and on a large collection of skins and skeletons placed at his disposal in 1859, by Du Chaillu, along with a young Gorilla in spirits, which he dissected. It is in the account of this dissection that Professor Wyman brings out the curious fact that the skull of the young Gorilla and Chimpanzee bears closer resemblance to the adult than to the infantile human cranium.

In Professor Wyman's library, bound up with a quarto copy of the Memoir by Dr. Savage and himself, is a terse but complete history of this subject, in his neat and clear handwriting, and with copies of the letters of Dr. Savage, Professor Owen, Mr. Walker, and M. du Chaillu.

In the introductory part of the Memoir, Professor Wyman states that "the specific name, Gorilla, has been adopted, a term used by Hanno in describing wild men found on the coast of Africa, probably one of the species of the Orang." The name *Troglodytes Gorilla* is no doubt to be cited as of Savage and Wyman, and it was happily chosen by Professor Wyman, after consultation with his friend, the late Dr. A. A. Gould, for the reason just stated. But it is interesting to see, in the correspondence before me, how strenuously each of the joint authors deferred to the other the honor of nomenclature. Dr. Savage from first to last insists, in repeated and emphatic terms, that the scientific name shall be given by Dr. Wyman as the scientific describer, and that he could not himself honestly appropriate it. Professor Wyman in his MS. account, after mentioning what his portion of the Memoir was, and that "the determination of the differential characters on which the establishment of the species rests was prepared by me,"

briefly and characteristically adds: "In view of this last fact, Dr. Savage thought, as will be seen in his letter, that the species should stand in my name; but this I declined."

This Memoir was read before this society on the 18th of August, 1847, and was published before the close of the year. But it had not, as it appears, come to Professor Owen's knowledge when the latter presented to the London Zoölogical Society, on the 22d of February, 1848, a memoir founded on three skulls of the same species, just received from Africa through Captain Wagstaff. When Professor Owen received the earlier memoir, he wrote to compliment Professor Wyman upon it, substituted in a supplementary note the specific name imposed by Savage and Wyman, and reprinted in an appendix the osteological characters set forth by the latter. "It does not appear, however [adds Dr. Wyman], either in the Proceedings or the Transactions of the [Zoölogical] Society, at what time our Memoir was published, nor that we had anticipated him in our description."

It is safe to assert that in this and the subsidiary papers of Dr. Wyman may be found the substance of all that has since been brought forward, bearing upon the osteological resemblances and differences between men and apes. After summing up the evidence he concludes: —

"The organization of the anthropoid Quadrumana justifies the naturalist in placing them at the head of the brute creation and placing them in a position in which they, of all the animal series, shall be nearest to man. Any anatomist, however, who will take the trouble to compare the skeletons of the Negro and Orang, cannot fail to be struck at sight with the wide gap which separates them. The difference between the cranium, the pelvis, and the conformation of the upper extremities of the Negro and Caucasian, sinks into comparative insignificance when compared with the vast difference which exists between the conformation of the same parts in the Negro and the Orang. Yet it cannot be denied, however wide the separation, that the Negro and Orang do afford the points where man and the brute, when the totality of their organization is considered, most nearly approach each other."

Selecting now for further comment only some of the more noticeable contributions to science, we should not pass by his investigations of the anatomy of the blind fish of the Mammoth Cave. The series began in that prolific year, 1843, with a paper published in Silliman's Journal, and closed with an article in the same Journal in 1854. Although Dr. Fellkamph had preceded him in ascertaining the existence of rudimentary eyes and the special development of the fifth pair of nerves, yet for the whole details of the subject, and the minute anatomy, we are indebted to Professor Wyman. Many of the details, however, as well as the admirable drawings illustrating them, remained unpublished until 1872, when he placed them at Mr. Putnam's disposal, and they were brought out in his elaborate article in the "American Naturalist." Here the extraordinary development of tactile sense, taking the place of vision, and perfectly adapting the animal to its subterranean life, is completely demonstrated.

If Professor Wyman's first piece of anatomical work was the preparation of a skeleton of a bull-frog, in his undergraduate days, his most elaborate memoir is that on the anatomy of the nervous system of the same animal (*Rana pipiens*), published in the "Smithsonian Contributions," in 1852.

Anything like an analysis of this capital investigation and exposition would much overpass our limits. For, although the special task he assigns to himself is the description of the nervous system of a single Batrachian, chiefly of its peripheral portion, and of the changes undergone during metamorphosis, he is led on to the consideration of several abstruse or controverted questions; — such, for instance, as the attempts that have been made to homologize the nervous system of Articulates with that of Vertebrates, upon which he has some acute criticism; — the theories that have been propounded respecting the functions of the cerebellum and its relation to locomotion, which he tests in a characteristic way by a direct appeal to facts; — the supposition of Cuvier that the special enlargements of the spinal cord are in proportion to the force of the respective limbs supplied therefrom; which he controverts

decisively by similar appeal, an extract from which I beg leave to append in a note.[1]

So, in describing the structure of the optic nerves in the frog, and the development of the eye and optic lobes, he proceeds to remark: —

"The instances of Proteus and Amblyopsis naturally suggest the questions, whether one and the same part may not combine functions wholly different in different animals, and whether the same may not hold true with regard to the cerebral organs which is known to obtain with regard to the skeleton, the teeth, the tongue, and the nose, that identical or homologous parts in different animals may perform functions wholly distinct. If the doctrine here suggested can be admitted (and if this were the place, facts could be cited in support of it), may we not find in it an explanation of many inconsistencies which now exist between the results of comparative anatomy and of physiology?"

Then, in his chapter on the philosophical anatomy of the cranial nerves and skull, after showing that there are but three pairs of cranio-spinal nerves, he takes up the controverted question as to the number of vertebræ which compose the

[1] "If by force is meant the muscular energy and development of the limbs, this statement does not appear to be sustained in the present instance, nor in many other instances brought to notice by comparative anatomy. In man the brachial enlargement is always larger than the crural, though the legs are so much more powerfully developed than the arms, and the same is true of the greater number of mammals. In frogs there is a still greater disproportion between legs and arms, yet there is not a corresponding difference in the size of the bulgings. They cannot, therefore, be said to be in proportion to the muscular force only of the limbs, but correspond far more nearly to the acuteness of the sense of touch, which in man and mammals is more delicate in the hands and arms than in the legs and feet. In bats, it is true that the muscular force of the arms is greater than that of the legs, and that the brachial far surpasses the crural enlargement; but, at the same time, the sense of touch in the membranes of the wings is exalted to a most extraordinary degree. In birds the posterior bulging is almost universally the largest, though this condition is in part dependent upon the presence of the rhomboidal sinus. In these animals, while the muscular energy of the wings is the most developed, the sensibility of the feet is the more acute."

skull, and supports the opinion that they also are only three in a characteristic manner.[1]

Of this whole memoir it is thought that, notwithstanding the great advance which has been made in comparative anatomy during the twenty-five years which have elapsed since it was published, its importance to the student has not at all diminished.

Next to this in extent and value may be ranked Professor Wyman's paper on the development of the common skate of our waters (*Raia Batis*), communicated to the American Academy in 1864, and published among its Memoirs. It gives an account of the peculiar egg-case of the Selachians, and of the several stages of the development of the embryo skate, expressed in the concise and clear language — as little technical as possible — for which he was distinguished, and leading up to not a few problems in comparative anatomy, morphology, or systematic zoölogy, — problems which Professor Wyman never evaded when they came directly in his way, and seldom handled without making some real contribution to their elucidation. For instance, in describing the external branchial fringes of the young skate, he notes the agreement of this character in the Batrachians; and in studying the seven branchial fissures of the embryo, he is

[1] "The conclusions which have been drawn from the statements made above are as follows: that in frogs the vagus comprises the glosso-pharyngeal and accessory nerves; that the trigeminus comprises the facial, the abducens and in the salamanders the patheticus and portions of the motor communis; that other evidence sustains the hypothesis, that the whole of the motor communis is a dependence of the trigeminus; if to these we add the hypoglossus (which in frogs is exceptionally a spinal nerve), we shall have three pairs of cranial nerves, each having all the characters of a common spinal nerve, namely, motor and sensitive roots and a ganglion; that there are no nerves to indicate a fourth vertebra, unless the special sense nerves are considered; if these are admitted as indications, then we must presuppose either two pairs of nerves to each vertebra, or the existence of six vertebræ, which is a larger number than can be accounted for on an osteological basis. The functions and mode of development of the special sense nerves we have taken as affording sufficient grounds for considering them as of a peculiar order, and not to be classified with common spinal nerves."

brought into contact with the view of Huxley, that the formation of the external ear is by involution of the integument. After confirming the contrary observations of Reichert on the embryo pig, he concludes that "the first of the seven branchial fissures of the embryo skate is converted into the spiracle, which is the homologue of the Eustachian tube and the outer ear-canal." After a full discussion of the homology of the upper jaw in sharks and skates, under the light afforded by his investigation of the embryo skate, he suggests that the cartilage which extends from the olfactory fossæ towards the pectoral fin is the probable homologue of a maxillary bone, and that in the lobe, the homologue of an intermaxillary; that, if so, the skates and proteiform reptiles agree in having the nostrils open in front of the dental arch; that while in all Batrachians the nasal groove becomes closed, in the skate it remains permanently open; and finally, that this view, if confirmed, "will add another feature which justifies Owen, Agassiz, and others, in dissenting from Cuvier so far as to give the Selachians a place in the zoölogical series higher than that of the bony fishes. But at the same time, it will give corroborative proof of the correctness of Cuvier's view, that 'the rudiments of the maxillaries and intermaxillaries . . . are evident in the skeleton.'"

In attempting these analyses, I am drifting into a fault which Professor Wyman never committed, that of being too long. So I must leave many of his papers unmentioned, and barely refer to two or three others which cannot be passed over. The most noteworthy of the shorter papers, however, are upon less technical or more generally interesting topics, so that we have need only to be reminded of them. Among them are his "Observations on the Development of the Surinam Toad," the paper on *Anableps Gronovii*, and that "On some unusual Modes of Gestation." The importance of these papers lies, not in being accounts of some of the most striking curiosities of the animal world, but in the sagacity and quickness with which he discerned, and the clearness with which he taught the lessons to be learned from them. Any good zoölogist, with the same excellent opportunities, would have

worked out all the details of the development of the Surinam toads in the skin of the back of their mother, and would equally have noted the morphological significance of the branchiæ and tail, that are never to know anything of the element they are adapted for; but Dr. Wyman remarks upon the development of the limbs independently of the vertebral axis, as showing that whatever view be taken of their homology, they are something superadded to it, and not evolved from it; he notes how the whole yelk mass is moulded into a spiral intestine; and that the embryo at the end of incubation forms a larger and heavier mass than existed in the egg when it commenced, — showing that there was an absorption of material furnished by the dermal sac of the mother, — "a solitary instance among Batrachians, if not among reptiles generally, in which the embryo is nourished at the expense of materials derived from the parent." From this he is led (in the last paper above mentioned) to infer the probability that the developed larvæ of *Hylodes lineatus* — carried about inland upon the back of their mother, and destitute of limbs adapted to terrestrial locomotion — may depend upon a secretion from the body for needful sustenance, — an interesting and rudimentary foreshadowing of mammalian life, of which he discerned the bearings.

His "Description of a Double Fœtus" (in the "Boston Medical and Surgical Journal," March, 1866) gives him the opportunity of briefly recording some of the results of his studies of the development of double monsters, and to bring out his view, that "the force, whatever it be, which regulates the symmetrical distribution of matter in a normal or abnormal embryo, has its analogy, if anywhere, in those known as polar forces;" that, "studying the subject in the most general manner, there are striking resemblances between the distribution of matter capable of assuming a polar condition, and free to move around a magnet, and the distribution of matter around the nervous axis of an embryo." That this is not one of those vague conceptions by which many speculators set about to explain that of which they know little by means of that of which they know less, but that he had striking

parallelisms to adduce, the close of this striking paper shows.

The subject of fore and hind symmetry, thus brought directly under notice, had been broached by Dr. Wyman several years before. He returned to it the year following, in his very important morphological paper, "On Symmetry and Homology in Limbs," read to this society in June, 1867, and published in the Proceedings of that date. It is interesting to observe with what caution and restraint he handled this doctrine of "reversed repetitions," which has since been freely developed by one of his pupils who has a special predilection for speculative morphology, Professor Burt Wilder.

Professor Wyman's "Notes on the Cells of the Bee," in the "Proceedings of the American Academy" for January, 1866, is a characteristic specimen of his way of coming directly down to the facts, and making them tell their own story. I could not recapitulate his results much more briefly than he records them in his paper. I need not recall to you how neatly he made this investigation, and represented some of the results, filling the comb with plaster-of-paris and then cutting it across midway, so that the observations might be made and the cells measured just where they are most nearly perfect; and then printing impressions of the comb upon the wood-block, he reproduces on the pages of his article the exact outlines of the cells, with all their irregularities and imperfections. But I cannot refrain from citing a portion of his remarks at the close: —

"Here, as is so often the case elsewhere in nature, the type-form is an ideal one; and with this, real forms seldom or never coincide. . . . An assertion, like that of Lord Brougham, that there is in the cell of the bee 'perfect agreement' between theory and observation, in view of the analogies of nature, is more likely to be wrong than right; and his assertion in the case before us is certainly wrong. Much error would have been avoided if those who have discussed the structure of the bee's cell had adopted the plan followed by Mr. Darwin, and studied the habits of the cell-making insects comparatively, beginning with the cells of the humble-

bee, following with those of wasps and hornets, then with those of the Mexican bees (Melipona), and finally with those of the common hive-bee. In this way, while they would have found that there is a constant approach to the perfect form, they would at the same time have been prepared for the fact, that even in the cell of the hive-bee perfection is not reached. The isolated study of anything in natural history is a fruitful source of error."

Let me add to this important aphorism its fellow, which I have from him, but know not if he ever printed it. "*No single experiment in physiology is worth anything.*"

The spirit of these aphorisms directed all his work. It is well exemplified in his experimental researches — the last which I can here refer to — upon "The formation of Infusoria in boiled solutions of organic matter, inclosed in hermetically sealed vessels and supplied with pure air," and its supplement, "Observations and Experiments on Living Organisms in Heated Water," published in the "American Journal of Science and Arts," the first in the year 1862, the other in 1867. Milne-Edwards could not have known the man, when he questioned the accuracy of the first series because they do not agree with those of Pasteur, and thought the difference in the results depended upon a defective mode of conducting the experiments. As Dr. Wyman remarks, in a note to the second series, "the recent experiments of Dr. Child of Oxford, and those reported in this communication, are sufficient answer to the criticisms of M. Edwards." Then, as to his thoroughness, most persons would have rested on the results of his thirty-three well-devised experiments proving "that the boiled solutions of organic matter made use of, exposed only to air which has passed through tubes heated to redness, became the seat of infusorial life;" but all would not have concluded that, after all, they "throw but little light on the immediate source from which the organisms have been derived," nor would many have closed an impartial summary of the opposing views in this judicial way: —

"If, on the one hand, it is urged that all organisms, in so far as the early history of them is known, are derived from

ova, and therefore from analogy we must ascribe a similar origin to these minute beings the early history of which we do not know, it may be urged with equal force, on the other hand, that all ova and spores, in so far as we know anything about them, are destroyed by prolonged boiling; therefore from analogy we are equally bound to infer that Vibrios, Bacterians, etc., could not have been derived from ova, since these would have all been destroyed by the conditions to which they have been subjected. The argument from analogy is as strong in the one case as in the other."

Returning to the subject again a few years later, with a critical series of twenty experiments, each of three, five, ten, fifteen, or even twenty flasks, used by way of checks and comparisons, — a rigorous experimenter would have been satisfied when he had proved that sealed solutions subjected to a heat of at least 212° for from one to four hours, became the seat of infusorial life, at least of such as Vibrios, Bacterians, and Monads, while all infusoria having the faculty of locomotion were shown by a special series of experiments to lose this at a temperature of 120° or at most 134° Fahr. But Professor Wyman carried the boiling up to five hours, and in these flasks no infusoria of any kind appeared. The question of abiogenesis stands to-day very much where Professor Wyman left it seven years ago.

I must omit all notice of the ethnological work which has occupied his later years, merely referring to the seven annual "Reports of the Trustees of the Peabody Museum of American Archæology and Ethnology," of which he was curator. The last of these, issued just before the writer's death, contain the principal results of his investigations of the human remains he collected in the shell-heaps of east Florida, and convincing evidence of the cannibalism of those who made them. A fuller memoir, embodying all his observations of the last six winters upon the Florida shell-mounds, was sent to the printer just before he died.

The thought that fills our minds upon a survey even so incomplete as this is: How much he did, how well he did it all, and how simply and quietly! We know that our associate,

though never hurried, was never idle, and that his great repose of manner covered a sustained energy; but I suspect that none of us, without searching out and collecting his published papers, had adequately estimated their number and their value. There is nothing forth-putting about them, nothing adventitious, never even a phrase to herald a matter which he deemed important.

His work as a teacher was of the same quality. He was one of the best lecturers I ever heard, although, and partly because, he was most unpretending. You never thought of the speaker, nor of the gifts and acquisitions which such clear exposition were calling forth, — only of what he was simply telling and showing you. Then to those who, like his pupils and friends, were in personal contact with him, there was the added charm of a most serene and sweet temper. He was truthful and conscientious to the very core. His perfect freedom, in lectures as well as in writing, and no less so in daily conversation, from all exaggeration, false perspective, and factitious adornment, was the natural expression of his innate modesty and refined taste, and also of his reverence for the exact truth.

It has been a pleasure to learn, from former college students, who hardly ever saw him except in the lecture room, that he gave to them much the same impression of his gifts and graces and sterling worth, that he gave us who knew him intimately — so transparent was he and natural.

With all his quick sense of justice, and no lack of occasion for controversy, it seemed to cost him no effort to avoid it altogether. He made no enemies, and was surrounded by troops of life-long friends. When he first went abroad, in 1841, he was told by some near friends, who recognized his promise, that a chair of natural history in his alma mater would soon have to be filled, and that he should be presented as a candidate. In the winter following, the present incumbent, responding to an invitation to visit Boston, which he had never seen, and to consider if he would be a candidate, then first heard of Wyman's name and his friends' expectations or hopes; whereupon he dismissed the subject from his

mind. Probably he felt more surprise than did Dr. Wyman when notified, a few months afterward, of the choice of the corporation. The exigencies of the botanic garden probably overbore other considerations. I doubt if Dr. Wyman ever had an envious feeling. Certain it is that no one welcomed the new professor with truer cordiality, or proved himself a more constant friend.

In these days it is sure to be asked how an anatomist, physiologist, and morphologist like Professor Wyman regarded the most remarkable scientific movement of his time, the revival and apparent prevalence of doctrines of evolution. As might be expected, he was neither an advocate or an opponent. He was not one of those persons who quickly make up their minds, and announce their opinions, with a confidence inversely proportionate to their knowledge. He could consider long, and hold his judgment in suspense. How well he could do this appears from an early, and so far as I know, his only published presentation of the topic, in a short review of Owen's "Monograph of the Aye-Aye" (in Am. Journ. Science, Sept., 1863) — the paper in which Professor Owen's acceptance of evolution, but not of natural selection, was promulgated. Dr. Wyman compares Owen's view with that of Darwin (to whom he had already communicated interesting and novel illustrations of the play of natural selection); and he adds some acute remarks upon a rather earlier speculation by Mr. Agassiz, in which the latter suggests that the species of animals might have been created as eggs rather than as adults. He states the case between the two general views with perfect impartiality, and the bent of his own mind is barely discernible. In due time he satisfied himself as to which of them was the more probable, or, in any case, the more fertile hypothesis. As to this I may venture to take the liberty to repeat the substance of a conversation which I had with him some time after the death of the lamented Agassiz, and not long before his own. I report the substance only, not the words.

Agassiz repeated to me, he said, a remark made to him by Humboldt, to the effect that Cuvier made a great mistake,

and missed a great opportunity, when he took the side he did in the famous controversy with Geoffroy St. Hilaire; he should have accepted the doctrines of morphology, and brought his vast knowledge of comparative anatomy and zoölogy, and his unequaled powers, to their illustration. Had he done so, instead of gaining by his superior knowledge some temporary and doubtful victories in a lost cause, his preëminence for all our time would have been assured and complete. I thought, continued Wyman, that there was a parallel case before me, that if Agassiz had brought his vast stores of knowledge in zoölogy, embryology, and palæontology, his genius for morphology, and all his quickness of apprehension and fertility in illustration, to the elucidation and support of the doctrine of the progressive development of species, science in our day would have gained much, some grave misunderstandings been earlier rectified, and the permanent fame of Agassiz been placed on a broader and higher basis even than it is now.

Upon one point Wyman was clear from the beginning. He did not wait until evolutionary doctrines were about to prevail, before he judged them to be essentially philosophical and healthful, "in accordance with the order of nature as commonly manifested in her works," and that they need not disturb the foundations of natural theology.

Perhaps none of us can be trusted to judge of such a question impartially, upon the bare merits of the case; but Wyman's judgment was as free from bias as that of any one I ever knew. Not at all, however, in this case from indifference or unconcern. He was not only, philosophically, a convinced theist, in all hours and under all "variations of mood and tense," but personally a devout man, an habitual and reverent attendant upon Christian worship and ministrations.

Those of us who attended his funeral must have felt the appropriateness for the occasion of the words which were there read from the Psalmist: —

"The heavens declare the glory of God, and the firmament showeth his handy-work. . . . O Lord, how manifold are thy works! In wisdom hast thou made them all; the earth is full of thy riches; so is this great and wide sea, wherein are

things creeping innumerable, both great and small beasts. Thou sendest forth thy spirit, they are created, and thou renewest the face of the earth."

These are the works which our associate loved to investigate, and this the spirit in which he contemplated them. Not less apposite were the Beatitudes that followed:

"Blessed are the meek; blessed are the peace-makers; blessed are the merciful; blessed are the pure in heart."

Those who knew him best, best know how well he exemplified them.

DANIEL HANBURY.[1]

Not long ago we called attention to a most valuable book, the "Pharmacographia, a History of Drugs," by Professor Flückiger of Strasburg and Daniel Hanbury of London, the first-fruits of much investigation, the precursor, as was hoped, of more extended similar works by the English author. We have now sadly to record the decease of Mr. Hanbury, of enteric typhoid, on the 24th of March, at his residence on Clapham Common, in the fiftieth year of his age. The obituary and biographical notices which have appeared in the London scientific journals and in the Proceedings of the learned societies, as well as loving individual tributes to an endeared memory, have given expression to the loss which has been sustained, and delineated the outlines of a most worthy and winning character. The loss is deplored, personally and scientifically, over wider circles and on this side of the Atlantic. The pupil and friend of Pareira and his successor in his line of work, an adept in pharmaceutical knowledge, a keen botanist, and a most assiduous and conscientious investigator, a man of simple and pure tastes, and happily of sufficient means, he had just withdrawn wholly from business in the noted house in which he had an inherited share, so that he might devote his powers and acquisitions without distraction to the natural history of drugs and useful vegetable products. He had already done much: more than sixty articles were contributed by him to a single journal, the editor of which declares that "the quality of what he did was almost faultless," that "he never wrote without having original information to impart, and his papers uniformly bear evidence of careful investigation and thorough knowledge." The Transactions and Journal of the Linnæan Society (of which he was repeat-

[1] American Journal of Science and Arts, 3 ser., ix. 476. (1875.)

edly a councilor and the treasurer at the time of his death) contain several of his papers. His first published paper, "On Turnsole," appeared in 1850; his latest, on the "Countess Chinchon and the Cinchona Genus," appeared, since his death, in "The Academy" of the 3d of April. An ardent botanist and lover of plants, he traveled much in the south of Europe, accompanied Dr. Hooker in his explorations of Lebanon, and took an active interest especially in the introduction of officinal plants and in ornamental cultivation. . With one villa-garden on the shores of the Mediterranean — that of his brother at Mortola — his memory to us is indelibly associated. Although remarkably self-reliant, Mr. Hanbury was the opposite of self-asserting or ambitious; but his sterling worth was soon recognized by the principal learned societies and associations. He was early chosen into the Royal Society and served upon its council in 1873. Born and educated in the Society of Friends, he remained a devout and attached member of it, while the graces and the goodness of his character endeared him to all who knew him. With the sense of personal loss his scientific comrades mingle deep regrets that a career of unusual usefulness and promise is cut short, and that in a line in which, it is to be feared, he leaves no successor. *Hanburia Mexicana*, a striking Cucurbitaceous genus, commemorated his services to botany.

ALEXANDER BRAUN.[1]

We announce with sorrow the death of this excellent botanist, which took place in Berlin, on the 29th of March, after a short illness. Systematic botanists of the first class are everywhere rare, and especially in Germany, where they have gone out of fashion, all attention being turned to histology and the like. In Braun's earlier days there was a goodly array of systematic botanists in Germany; at his decease there are very few of mark, although signs of revival are apparent. Alexander Braun was born at Ratisbon, May 10, 1805, but was brought up at Carlsruhe, where his father became a trusted officer in the post-office department. Fifty years ago there was a knot of closely-allied students at the university of Heidelberg, consisting of Braun, Carl Schimper, Agassiz, and Engelmann. Two of them were transferred to our own soil; the latter is now the sole survivor. Three of them went soon to Munich, where Oken, Schelling, Dollinger, and Martius were teaching; but Braun, Agassiz, and Engelmann met again as fellow-students at Paris in 1832. The first two became allied afterwards by the marriage of Agassiz with Braun's sister. About the time that Dr. Engelmann came to the United States, Braun was made professor of botany and zoölogy in the Polytechnic School of Carlsruhe. In 1846 he took the chair of botany in the university of Freiburg in the Breisgau; was transferred to Giessen in 1850; but in the spring of 1851 was called to Berlin, as the successor to Link and Kunth, taking charge of the botanic garden as well as of the professorship. Although he had nearly reached the age of seventy-two, and felt the full weight of his years, yet he was assiduously attending to his official duties when he was suddenly prostrated by acute disease of the chest, terminating

[1] American Journal of Science and Arts, 3 ser., xiii. 471. (1877.)

fatally. Although learned in almost every department of his science, his forte, like that of Agassiz, was morphology, and his systematic work mainly among the higher and some of the lower Cryptogamia, Marsilia, Isoetes, Chara, etc. Although his communications to the scientific journals began as early as the year 1822, when he was only seventeen years old, his first contribution to science, of much extent and of high and permanent value, was his memoir on the arrangement of the scales of Pine-cones, etc., published in 1830. With this publication began the present knowledge of phyllotaxis. It is well understood that the first steps were taken by his fellow-student Carl Schimper, and that the early investigations were pursued in common by the two. But Schimper published nothing, or next to nothing, either then or since, although he lived until the year 1867. His name in connection with the subject is preserved by the favorable mention of his companions and contemporaries; but Braun's treatise was timely and fruitful, and became classical. Braun's ability for the philosophical treatment of vegetable morphology and development was manifested in his next large paper, namely, in his memoir entitled "Rejuvenescence in Nature," especially in the life and development of plants. This was first published at Freiburg in 1849–50, and again at Leipsic in 1851, and an English translation of it was published by the Ray Society in 1853. Of a similar character, and marked with equal acuteness, is his essay on "The Vegetable Individual in its relation to Species," etc., published in 1853, at Berlin, and which, in a translation by a pupil of mine, was mainly reproduced in this Journal (May and September, 1855). He reaches the conclusion — which would now be more confidently expressed — "that the individual appears in its full import only in the higher steps of the series of created beings."

In his systematical work, Braun was exceedingly laborious, persevering, and conscientious. When we add that throughout the riper and what should have been the most productive years of his life, he was overtasked with official duties and cares, we shall not wonder that much which he hoped to accomplish is left undone. His work upon Marsilia, Pilularia,

and Isoetes may be essentially complete. But his prolonged studies of Chara, which began forty years ago, and the completion of which would have crowned his career, have probably not been finished, or brought into such form that the results may be fully secured.

His influence as a teacher is said to have been great; as an investigator he stood in the first rank among the botanists of our time; as a man, his simple, earnest, and transparently truthful character won the admiration and love of all who knew him.

CHARLES PICKERING.[1]

Charles Pickering, M. D., died in Boston, of pneumonia, on the 17th of March, 1878, in the seventy-third year of his age. He was of a noted New England stock, being a grandson of Colonel Timothy Pickering, a member of Washington's military family, and of his first cabinet as President; and he was elected into this Academy under the presidency of his uncle, John Pickering. He was born on Starucca Creek, on the Upper Susquehanna, in the northern part of Pennsylvania, at a settlement made on a grant of land taken up by his grandfather, who then resided there. His father, Timothy Pickering, Jr., died at the age of thirty years, leaving to the care of the mother — who lived to a good old age — the two sons, Charles and his brother Edward, who were much united in their earlier and later lives, and were not long divided in death, the subject of this notice having been for only a year the survivor.

Dr. Pickering was a member of the class of 1823 at Harvard College, but left before graduation. He studied medicine, and took the degree of M. D. at the Harvard Medical School in 1826. Living in these earlier years at Salem, he was associated with the late William Oakes in botanical exploration; and it is believed that the two first explored the White Mountains together, following in the steps of the first botanist to ascend Mount Washington, Dr. Manasseh Cutler of Essex County, and of Francis Boott and the still surviving Dr. Bigelow. His taste for natural history showed itself in boyhood, both for botany and zoölogy, and probably decided his choice of a profession. He may have intended to practise medicine for a livelihood, when, about the year 1829, he took

[1] Proceedings of the American Academy of Arts and Sciences, xiii. 414. (1878.)

up his residence at Philadelphia; but it is probable that he was attracted thither more by the facilities that city offered for the pursuit of natural history than by its renown as a centre of medical education. We soon find him acting as one of the curators of the Academy of Natural Sciences, and also as librarian, and with reputation established as the most erudite and sharp-sighted of all the young naturalists of that region. His knowledge then, as in mature years, was encyclopædic and minute; and his bent was toward a certain subtlety and exhaustiveness of investigation, which is characteristic of his later writings. Still, in those days in which he was looked up to as an oracle, and consulted as a dictionary by his co-workers, he had published nothing which can now be recalled, except a brief essay on the geographical distribution and leading characteristics of the United States flora, which very few of our day have ever seen.

When the United States surveying and exploring expedition to the South Seas, which sailed under command of then Lieutenant Charles Wilkes in the autumn of 1838, was first organized under Commodore T. Ap-Catesby Jones, about two years before, Dr. Pickering's reputation was such that he was at once selected as the principal zoölogist. Subsequently, as the plan expanded, others were added. Yet the scientific fame of that expedition most largely rests upon the collections and the work of Dr. Pickering and his surviving associate, Professor Dana, the latter taking, in addition to the geology, the Corals and the Crustacea, other special departments of zoölogy being otherwise provided for by the accession of Mr. Couthouy and Mr. Peale. Dr. Pickering, although retaining the ichthyology, particularly turned his attention during the three and a half years' voyage of circumnavigation to anthropology, and to the study of the geographical distribution of animals and plants; to the latter especially affected by or as evidence of the operations, movements, and diffusion of the races of man. To these, the subjects of his predilections and to investigations bearing upon them, all his remaining life was assiduously devoted. The South Pacific exploring expedition visited very various parts of the world; but it

necessarily left out regions of the highest interest to the anthropological investigator, those occupied in early times by the race to which we belong, and by the peoples with which the Aryan race has been most in contact. Desirous to extend his personal observations as far as possible, Dr. Pickering, a year after the return of the expedition, and at his own charges, crossed the Atlantic, visited Egypt, Arabia, the eastern part of Africa, and western and northern India. Then, in 1848, he published his volume on "The Races of Man, and their Geographical Distribution," being the ninth volume of the "Reports of the Wilkes' Exploring Expedition." Some time afterward, he prepared, for the fifteenth volume of this series, an extensive work on the "Geographical Distribution of Animals and Plants." But, in the course of the printing, the appropriations by Congress intermitted or ceased, and the publication of the results of the celebrated expedition was suspended. Publication it could hardly be called: for Congress printed only one hundred copies, in a sumptuous form, for presentation to states and foreign courts; and then the several authors were allowed to use the types and copperplates for printing as many copies as they required, and could pay for. Under this privilege, Dr. Pickering brought out in 1854 a small edition of the first part of his essay,— perhaps the most important part,— and in 1876 a more bulky portion, "On Plants and Animals in their Wild State," which is largely a transcript of the note-book memoranda as jotted down at the time of observation or collection.

These are all his publications, excepting some short communications to scientific journals and the proceedings of learned societies to which he belonged. But he is known to have been long and laboriously engaged upon a work for which, under his exhaustive treatment, a lifetime seems hardly sufficient; a digest, in fact, of all that is known of all the animals and plants with which civilized man has had to do from the earliest period traceable by records. When Dr. Pickering died, he was carrying this work through the press at his own individual expense, had already in type five or six hundred quarto pages, and it is understood that the remainder,

of about equal extent, is ready for the printer. This formidable treatise is entitled "Man's Record of his own Existence." Its character is indicated in the brief introductory sentences: —

"In the distribution of species over the globe, the order of Nature has been obscured through the interference of man. He has transported animals and plants to countries where they were previously unknown; extirpating the forest and cultivating the soil, until at length the face of the globe itself is changed. To ascertain the amount of this interference, displaced species must be distinguished, and traced each to its original home. Detached observations have already been given in the twenty-first and succeeding chapters of my 'Races of Man'; but when such observations are extended to all parts of the globe, the accumulated facts require some plan of arrangement. A list will naturally assume the chronological order, beginning with Egypt, the country that contains the earliest records of the human family, and receding geographically from the same central point of reference."

Then, starting with "4713 B. C.," and "4491 B. C., beginning of the first Great Year in the Egyptian reckoning," he begins the list, which, under the running heading of "Chronological Arrangement of Accompanying Animals and Plants," first treats of the vegetables and animals mentioned in the book of Genesis, and of the "Commencement of Bedouin or Nomadic Life in the Desert;" passes to the "Colonization of Egypt," and to critical notices (philological and natural-historical) of its plants and animals, as well their earliest mention as their latest known migrations; reaches the beginning of the Christian era at about the 470th page; and so proceeds, till our wonder at the patience and the erudition of the writer passes all bounds. We are ready to agree with a biographer who declares that our associate was "a living encyclopædia of knowledge," — that there never was a naturalist "who had made more extended and minute original explorations;" and we fully agree that "no one ever had less a passion or a gift for display;" "that he was engaged during a long life in the profoundest studies, asking neither fame

nor money, nor any other reward, but simply the privilege of gaining knowledge and of storing it up in convenient forms for the service of others;" that "the love of knowledge was the one passion of his life," and that "he asked no richer satisfaction than to search for it as for hidden treasure." He was singularly retiring and reticent, very dry in ordinary intercourse, but never cynical; delicate and keen in perception and judgment; just, upright, and exemplary in every relation; and to those who knew him well communicative, sympathetic, and even genial. In the voyage of circumnavigation he was the soul of industry, and a hardy explorer. The published narrative of the commander shows that he took a part in every fatiguing excursion or perilous ascent. Perhaps the most singular peril (recorded in the narrative) was that in which this light-framed man once found himself on the Peruvian Andes, when he was swooped upon by a condor, evidently minded to carry off the naturalist who was contemplating the magnificent ornithological specimen.

Dr. Pickering married in the year 1851, and leaves a widow, but no children to inherit his honored name.

ELIAS MAGNUS FRIES.[1]

Elias Magnus Fries died at Upsal on February 8, in the eighty-fourth year of his age, five months after the celebration, in which he was able to take some part, of the four hundredth anniversary of the foundation of that university, and a month after the hundredth anniversary of the death of Linnæus. Born, as was Linnæus, in Smoland, a southern province of Sweden, and like him called in middle age to the renowned Scandinavian university, he might be regarded as the most distinguished of Linnæus's successors, except for the fact that he did not occupy the chair of Linnæus; for when, more than forty years ago, Fries, then demonstrator of botany at Lund, was called to Upsal, Wahlenberg was in the botanical chair, and Fries was made professor of practical economy. His son, however, by the retirement of Areschoug, is now botanical professor.

Fries's earliest work, the first part of his Novitiæ, appeared in the year 1814, when the author was only twenty years old. His last of any moment, a new edition of his "Hymenomycetes Europæi," was published on his eighty-first birthday, August 15, 1874. Most of the sixty intervening years are marked by some publication from his busy and careful hand. His work was wholly in systematic botany, and of the highest character of its kind. In Phænogamous botany, it related chiefly to the Scandinavian flora, in which for critical judgment he had no superior; in Mycology, of which he was the reformator and to a good degree in Lichenology, he had no rival except as regards microscopical research. The modern microscope did not exist when he began his work, and, while showing how much can be done without it, he may too long

[1] Proceedings of the American Academy of Arts and Science, xiii. 453. (1878.)

have underrated its value. But he lived to see it confirm many conclusions which his insight foresaw, and solve riddles which he had pondered, but was unable to divine. He was the prince, Nestor, and last survivor of an excellent school of systematic botanists, whose teachers were taught by Linnæus or his contemporaries.

JACOB BIGELOW.[1]

Dr. Jacob Bigelow died at his residence in Boston, on the 10th of January last, near the close of the ninety-second year of his age.

While we would pay the tribute due to his memory as by far the most venerable of American botanists, the last survivor of a school in this country which culminated half a century ago, it should also be remembered that he was even at that time distinguished in their scientific avocations, and that from middle to old age he was among the most eminent of physicians. It is not often that we can contemplate a life so long, so richly various, and so well-rounded as his. He was born in Sudbury, Massachusetts, on February 27, 1787; and his father was the minister of the town. That almost goes without saying, most of our distinguished professional men of his and the preceding generations in New England having been the sons of country ministers. He was graduated at Harvard College in the year 1806, Alexander H. Everett and the late Dr. J. G. Cogswell being among the most notable of his classmates, all of whom he long survived. He directly took up the study of medicine, was licensed as a practitioner in 1809, and after attending one course of lectures in Philadelphia, took his degree of M. D. at Harvard in 1810, and established himself in Boston. There he was a practising physician for about sixty years, and since the death of his senior, Dr. James Jackson, probably the most eminent one. What turned his attention to botany we know not. He early showed an abiding taste for poetry. His commencement part was a poem, and he delivered a Φ. B. K. poem not long after. At about the same time, however, he gave a course of popular botanical lectures in Boston, in connection with Professor Peck, who must have been installed as natural-history professor at

[1] American Journal of Science and Arts, 3 ser., xvii. 263. (1879.)

Cambridge while Dr. Bigelow was a medical student. The latter possessed the gift of exposition which Dr. Peck lacked; and it naturally came to pass that Dr. Bigelow repeated this course of lectures alone for a year or two afterward.

In the spring of 1814 he brought out the first edition of his "Florula Bostoniensis," the book which, mainly in its second edition, has been the manual for New England herborization down to a recent day, or rather to a day which seems to us recent. The original volume, of 268 octavo pages, describes the plants which "have been collected during the two last seasons in the vicinity of Boston, within a circuit of from five to ten miles," exceeding those limits only in the case of Magnolia (from Manchester) and one or two more remarkable plants. We know of no other Flora of the kind which has been prepared so quickly and so well. The characters are short diagnoses, and in good part compiled. But the descriptive matter must have been original; and it shows that aptitude for seizing the best points of character or most available distinctions, and of indicating them in few and clear words, which has made this Manual so deservedly popular. Similar merits distinguish, on its botanical side, Dr. Bigelow's "American Medical Botany," a quarto work which was published, in three parts or volumes, between 1817 and 1821, with colored plates — at that time thought to be very good ones indeed — of the principal medicinal plants of the country. He also brought out an American edition of Sir James Edward Smith's "Introduction to Botany"; and his botanical knowledge, along with that of the Materia Medica generally and his classical scholarship, placed him at the head, or at the laboring oar, of the committee which in 1820 formed the American Pharmacopœia. The writer used this volume in his medical student days, and remembers dimly how the account of minor preparations, coming down to jams and conserves, ended with the classical "Jam satis est mihi."

The second edition of the "Florula Bostoniensis," published in 1824, while retaining its modest title, was nearly doubled in size and in the number of plants it contained, the whole area of New England included; and it became the manual of

botany for the region. What a popular and satisfactory work it was, especially to hundreds of amateur botanists, some still living may testify.

The third and last edition, issued in 1840, was a reprint, with various additions and corrections, furnished mainly from those who had learned their botany from the preceding one. This is the last Flora or Manual of this and perhaps any other country, arranged upon the Linnæan artificial system. Much later in life the author contemplated a revision of the work, brought up to the time and illustrated by chromo-lithographic plates, such as we have lately seen turned to good account. But after some consideration the project was abandoned. He did not propose himself to undertake the editorial work; for he had long since passed from actual service into the emeritus or honorary rank of botanists; and his active professional life, already verging to its close, was diversified or relieved by other avocations. Indeed, some of these were taken up very early. He became Rumford professor of the applications at Cambridge in 1816, and delivered annual courses of lectures until 1827, when he published the substance of them in a volume entitled "Elements of Technology," here coining this apt word. During all this time, and much longer, he was professor of Materia Medica in the medical school of Harvard University, namely from 1815 to 1855; for many of these years one of the physicians of the Massachusetts General Hospital; through all of them, and until old age disabled him, a leading physician of Boston. From the year 1847 to 1863 he was president of the American Academy of Arts and Sciences, of which body he was a member for sixty-seven years!

We cannot here refer to Dr. Bigelow's various professional and literary writings. They are not numerous, but are weighty. His treatise on "Nature in Disease," which contains the famous discourse "On Self-limited Disease," is the most important of them; and an address "On the Limits of Education," delivered in the year 1865 before the Massachusetts Institute of Technology, is notable. It has been said of the latter, that never before was the depreciation of classical study or general

culture, as a preparation for technical scientific education, undertaken by so ripe a classical scholar or so wide-cultured a man. His many essays in English and Latin verse, some of which have been privately printed, ought to be collected. Dr. Bigelow lived, honored and trusted, to a good old age before infirmities touched his frame, and only toward the close was the brightness of his acute mind dimmed. The candle at length burnt down, the flame flickered awhile in the socket, and the light went out.

The name will abide in botanical nomenclature. First appeared in Rees' Cyclopedia the Bigelowia of Smith, founded on the Adelia of Michaux. But that is Forestiera. Then Sprengel, in 1821, founded a genus Bigelovia on a Brazilian plant which he took to be a Rhamnacea, but it is a species of Casearia. Again, in 1824, Sprengel gave the name to a part of Spermacoce, the Borreria of G. Meyer. Then De Candolle, in 1824, was proposing a Bigelowia on *Solea concolor*, of our own New England, as the "Prodromus" records, when he found he had to refer it to Noisettia. Lastly, in 1836, De Candolle bestowed the name of Bigelovia upon some golden-flowered *Compositæ* of the southern United States, which had borne the name of an Old World genus, Chrysocoma (Anglice, Golden-tuft), and he added the complimentary phrase: "A Chrysocoma separatum dicavi cl. J. Bigelow qui floræ Americanæ auream coronam flora Bostoniensi et medica addidit." Although this genus was founded upon only two or three species, it has been vastly extended by the exploration of the western regions of our country, where it forms a conspicuous and characteristic portion of the low shrubby vegetation. More than thirty North American species of Biglovia, besides one of Mexico and two of the Andes of South America, now commemorate our venerable late associate. Most of them were introduced to the genus by the present writer.

JOHN CAREY.[1]

JOHN CAREY — of whom few of the botanists of our day can have a personal remembrance — died at Blackheath, near London, March 26 ult., in the 83d year of his age. He came from London to the United States, in the spring of 1830, accompanied by three young and motherless children and by his brother, Samuel T. Carey, who was also addicted to botany. Both, we believe, were Fellows of the Linnæan Society, and were near friends of Thomas Bell, afterwards the president of that society, who also lived to a good old age, dying only a few weeks earlier than the subject of this notice. Samuel T. Carey remained in the city of New York, in active business, and so was only an amateur botanist. His brother John went into the country, first to Towanda, in the northern part of Pennsylvania, then to Bellows Falls, Vermont, where, giving much of his leisure to botanical pursuits, he resided until the year 1836, when he removed to New York, upon the entrance of his sons into Columbia College. He did not enter into business, but his administrative talents and great worth were so appreciated that he was at various times called to very responsible temporary positions. These positions, although unsought, were not unwelcome, for no small part of the moderate property he had brought from England had been lost in investments made through reliance upon the honor and probity of defaulting States.

From the time of his arrival in the United States down to the year of his return to England in 1852, most of his leisure was given to botany, and much of it in the companionship of the present writer, who was generously and greatly assisted by him in many critical studies. The proofs of the writer's first botanical book were revised by him, and to the first

[1] American Journal of Science and Arts, 3 ser., xix. 421. (1880.)

edition of the "Manual of Botany" Mr. Carey contributed the articles on Salix and on Carex, at that time the two most difficult parts of the work. In the year 1841 the two made a botanical journey together into the mountains of North Carolina, extending to the Grandfather and to the Roan, though a mishap upon the former mountain prevented Mr. Carey from reaching the latter. After the establishment of the writer at Cambridge, Mr. Carey was his frequent guest and invaluable companion. His botanical career may have said to have closed in the year 1852. In that year he returned to England alone, having successively lost his aged mother and his two younger sons, and seen the elder son happily established in marriage. He engaged for several years in business, in connection with a friend of his youth, whose daughter he soon married, but lost within three years, after the birth of the second of the two children, the solace and comfort and care of a serene old age, who survive to perpetuate his name, we trust, on that side of the ocean also. Mr. Carey's first herbarium was destroyed by a calamitous fire in New York, at the time of the death of his youngest son. American botanists vied with each other in the endeavor to repair this serious loss, and another large collection of United States plants was formed, critically studied, and carefully annotated. This was presented to the Kew Gardens herbarium eleven years ago. Several species of United States plants commemorate this honored name, among them a Saxifrage, which was discovered upon the excursion to the mountains of North Carolina, where the survivor of the party re-collected it last summer. The almost sole survivor of a botanical circle, of which Torrey was the centre, sadly but serenely pays the tribute of this brief note to the memory of a near and faithful friend, an accomplished botanist, a genial and warm-hearted and truly good man.

THOMAS POTTS JAMES.[1]

Thomas Potts James died at his residence in Cambridge, February 22, 1882, in the seventy-ninth year of his age. He had been a Fellow of the Academy for only four years, most of his life having been spent in Philadelphia, in the neighborhood of which city he was born on the 1st of September, 1803. His paternal and maternal ancestors were notable persons among the earlier settlers of Pennsylvania. For forty years he was engaged in business in Philadelphia as a wholesale druggist, on the relinquishment of which he removed to Cambridge, bringing his wife and their four children to her paternal home. From his youth he was more or less devoted to botany; but of late years, having more leisure for the indulgence of his taste, and wishing to be more than an amateur, he devoted himself exclusively and most sedulously to bryology, in which he became a proficient. After the death of Mr. Sullivant in 1873, Mr. James and our associate, Lesquereux, were looked to as the principal authorities upon Mosses in this country; and the duty appropriately devolved upon them of preparing the systematic work upon North American Bryology which Mr. Sullivant had planned. Owing to the preoccupation of Mr. Lesquereux in vegetable palæontology, the laboring oar fell to Mr. James. He had already published some papers upon the subject in the "Transactions of the American Philosophical Society," of which he had long been an active member, and he had contributed to Mr. Watson's "Botany of Clarence King's Exploration on the Fortieth Parallel" a notable article on the Mosses of that survey. Our own Academy has also published some of the results of the joint study of these two veteran bryologists. The characters of Mosses in these days are mostly drawn

[1] Proceedings American Academy of Arts and Science, xvii. 405. (1882.)

from their minute structure. Hundreds of species and varieties in numerous specimens had to be patiently scrutinized under the compound microscope, the details sketched and collated, and the differences weighed. To this task Mr. James gave himself with single and untiring devotion. He had nearly brought this protracted labor of microscopical analysis to a conclusion, and was actually engaged in this work, when the eye suddenly was dimmed and the pencil dropped from his hand. Partial paralysis was soon followed by coma, and he died within a few hours. So very much has been done, that it is confidently hoped that his coadjutor may soon bring the work to a completion, and give to bryological students the Manual of North American Mosses which is greatly needed, and to which a vast amount of faithful research has been devoted. The name of Mr. James will thereby be inseparably associated with the advancement of an interesting branch of botany. He was not often seen at our meetings, but he is greatly missed by his associates in study, and his memory is cherished by all who in the various relations of life came to know this diligent and conscientious student of nature, and most estimable, simple-hearted, kindly, and devout man.

JOHN AMORY LOWELL.[1]

JOHN AMORY LOWELL died at his residence in Boston, on the 31st of October last, when he had almost completed the eighty-third year of his age, for he was born on the 11th of November, 1798. A few years of his boyhood — from 1803 to 1806 — were passed in Paris, where he was a spectator of some of the glorifications of the First Empire, especially on the occasion of the return from Austerlitz. He entered Harvard College in 1811, Messrs. Sparks, Parsons, and Palfrey being among his classmates, and after graduation he entered a mercantile house. He was elected into this Academy on the 10th of November, 1841, at the same time with two other Fellows assigned to the botanical section. One was William Oakes, of Ipswich, who died seven years afterward; to the other is assigned the duty of preparing this memorial. When the Fellows of the Academy were arranged in classes and sections, the pronounced tastes inherited from his father and cultivated by his own studies made it natural that he should belong to the small section of botany. But he might with equal propriety have been relegated to more than one section of the third class. For, notwithstanding his devotion to business affairs, his classical and linguistic knowledge were always well kept up, and his authority upon economical and financial questions was great.

The family has always had a marked representation in this Academy. To mention only the direct line, the subject of our notice was chosen into it very shortly after the death of his father, — the John Lowell who, after achieving distinction and a competency at the bar, retired from active practice at the age of thirty-four, to be known through his valuable writings as "The Norfolk Farmer," and as a principal promoter,

[1] Proceedings American Academy of Arts and Science, xvii. 408. (1882.)

if not the founder, of scientific agriculture and horticulture in New England. John Lowell — the father of John Amory Lowell — was elected into the Academy in the year 1804, soon after the decease of his father, the Hon. John Lowell, first judge of the United States District Court of Massachusetts, under a commission from Washington. This office is now held by his great-grandson, the eldest son of our deceased associate, who has been a Fellow since the year 1877, thus continuing the line from the very foundation of the Academy, for Judge Lowell was one of the sixty-two members incorporated by the charter in 1780. In tracing the genealogy one step farther back, we come (as is almost universal in New England families of note) upon a clergyman, the Rev. John Lowell, of Newbury, a man of mark in his day.

Mr. Lowell was the fourth of his family to be a member of the Corporation of Harvard University, to which he gave a continuous and most valuable service of forty years. He was for more than fifty years one of the directors of the Suffolk Bank, which was chartered in his time, and which early established a very useful plan for the redemption of the currency of the New England banks in Boston. Not to mention other important public trusts, — as of the Athenæum, of the Massachusetts General Hospital, of the Agricultural Trustees, of the Provident Institution for Savings, to all of which he rendered assiduous and wise service, — nor to refer here to the very important part which he has taken for a lifetime in the development of the manufacturing interests of Massachusetts, especially as prosecuted in the town which was named in commemoration of similar services by his cousin, — we proceed to speak of that most important "corporation sole" founded by that cousin, the Lowell Institute. This trust was specifically consigned to our late associate and to such successor as he should appoint, — with preference to the family and the name of Lowell, — subject to no other than a formal visitatorial control, mainly for auditorship. And "to him, single and alone, it fell to shape the whole policy and take the whole direction of this great educational foundation," the history of which for almost half a century has justly been said to be a

"record of his own intellectual breadth and scope, as well as of his large administrative capacity." We all know with what good judgment, with what liberality, and with what success this peculiar trust has been administered, and how on the one hand a series of most distinguished men have been attracted into its service, while on the other the efforts of younger men have been stimulated and rewarded at the period when such encouragement was most important to them. Suffice it to mention the names of Lyell and Agassiz, — the former early and also a second time brought from England for courses of lectures at the Lowell Institute, the latter a permanent acquisition to us and to our country. Through Mr. Lowell's discernment, moreover, the first encouragement to devote his life to scientific pursuits was afforded to Jeffries Wyman, by the offer of the curatorship of the Institute as well as of a lectureship. The intellectual and the financial interests of this trust have equally prospered in Mr. Lowell's hands; for while the number of lecture-courses has been doubled, and various subsidiary lines of instruction have been developed, the principal of the fund has been increased to thrice its original amount.

Mr. Lowell's fondness for botany developed shortly after he left college, and was incited by the botanical intercourse between his father and the late Dr. Francis Boott, with whom he maintained a lifelong friendship. But it was only in about the year 1844 or 1845 that he began the formation of an herbarium and botanical library; and this was actively prosecuted for several years, in evident expectation of comparative leisure which he could devote to scientific studies. He subscribed liberally to the botanical explorations in our newly-acquired or newly-opened western territories; and when in Europe, in 1850 and 1851, he added largely to his store of rare and costly botanical books. But just when he was ready to use the choice materials and appliances which had been brought together, the financial crisis of 1857 remanded him to business. The grave duties and responsibilities which he resumed he carried up nearly to the age of fourscore — carried as it were with the vigor of early manhood and the cheerful

ease that attends "a real love of work for the work's own sake." And when it became evident that the comparatively unbroken attention requisite for serious botanical study was not to be secured, and as soon as a building was prepared for their reception, he presented all his botanical books which were needed to the herbarium of Harvard University; and the remainder, with his herbarium, to the Boston Society of Natural History, — not giving up the while his studious habits, but transferring his attention back to the Latin and the French classics, and in a certain degree to German and Italian literature.

As his father was one of the leading promoters of the establishment of the Botanic Garden of Harvard University, Mr. Lowell was also its most efficient supporter through its years of sorest need; and, in memory of his father, he bequeathed to it the sum of $20,000 in order to make his annual subvention perpetual. He made a legacy of equal amount to the general Library of the University, which he along with his father and grandfather had served in a most responsible trust for seventy years. He never sought or accepted any office in city or state; but few men were more sought for responsible trusts, or ever served their day and generation more devotedly, disinterestedly, and wisely. He seemed always to have a firm confidence in his own judgment, and that confidence appears not to have been misplaced.

CHARLES DARWIN.[1]

CHARLES DARWIN died on the 19th of April last, a few months after the completion of his seventy-third year; and on the 26th, the mortal remains of the most celebrated man of science of the nineteenth century were laid in Westminster Abbey, near to those of Newton.

He was born at Shrewsbury, February 12, 1809, and was named Charles Robert Darwin. But the middle appellation was omitted from his ordinary signature and from the title-pages of the volumes which, within the last twenty-five years, have given such great renown to an already distinguished name. His grandfather, Dr. Erasmus Darwin, — who died seven years before his distinguished grandson was born, — was one of the most notable and original men of his age; and his father, also a physician, was a person of very marked character and ability. His maternal grandfather was Josiah Wedgwood, who, beginning as an artisan potter, produced the celebrated Wedgwood ware, and became a Fellow of the Royal Society and a man of much scientific mark. The importance of heritability, which is an essential part of Darwinism, would seem to have had a significant illustration in the person of its great expounder. He was educated at the Shrewsbury Grammar School and at Edinburgh University, where, following the example of his grandfather, he studied for two sessions, having the medical profession in view, and where, at the close of the year 1826, he made his first contribution to natural history in two papers (one of them on the ova of Flustra). Soon finding the medical profession not to his liking, he proceeded to the University of Cambridge, entering Christ's College, and took his bachelor's degree in

[1] Proceedings of the American Academy of Arts and Science, xvii. 449. (1882.)

1831; that of M. A. in 1837, after his return from South America.

It is said that Darwin was a keen fox-hunter in his youth, — not a bad pursuit for the cultivation of the observing powers. There is good authority for the statement — though it has nowhere been made in print — that at Cambridge he was disposed at one time to make the Church his profession, following the example of Buckland and of his teacher, Sedgwick. But in 1831, just as he was taking his bachelor's degree, Captain Fitzroy offered to receive into his own cabin any naturalist who was disposed to accompany him in the Beagle's surveying voyage round the world. Mr. Darwin volunteered his services without salary, with the condition only that he should have the disposal of his own collections. And this expedition of nearly five years — from the latter part of September, 1831, to the close of October, 1836 — not only fixed the course and character of the young naturalist's life-work, but opened to his mind its principal problems and suggested the now familiar solution of them. For he brought back with him to England a conviction that the existing species of animals and plants are the modified descendants of earlier forms, and that the internecine struggle for life in which these modifiable forms must have been engaged would scientifically explain the changes. The noteworthy point is that both the conclusion and the explanation were the legitimate outcome of real scientific investigation. It is an equally noteworthy fact, and a characteristic of Darwin's mind, that these pregnant ideas were elaborated for more than twenty years before he gave them to the world. Offering fruit so well ripened upon the bough, commending the conclusions he had so thoroughly matured by the presentation of very various lines of facts, and of reasonings close to the facts, unmixed with figments and *à priori* conceptions, it is not so surprising that his own convictions should at the close of the next twenty years be generally shared by scientific men. It is certainly gratifying that he should have lived to see it, and also have outlived most of the obloquy and dread which the promulgation of these opinions aroused.

Mr. Darwin lived a very quiet and uneventful life. In 1839 he married his cousin, Emma Wedgwood, who with five sons and two daughters survives him; he made his home on the border of the little hamlet of Down, in Kent, — "a plain but comfortable brick house in a few acres of pleasure-ground, a pleasantly old-fashioned air about it, with a sense of peace and silence;" and here, attended by every blessing except that of vigorous health, he lived the secluded but busy life which best suited his chosen pursuits and the simplicity of his character. He was seldom seen even at scientific meetings, and never in general society; but he could welcome his friends and fellow-workers to his own house, where he was the most charming of hosts.

At his home, without distraction and as continuously as his bodily powers would permit, Mr. Darwin gave himself to his work. At least ten of his scientific papers, of greater or less extent, had appeared in the three years between his return to England and his marriage; and in the latter year (1839) he published the book by which he became popularly known, namely, the "Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage of the Beagle," which has been pronounced "the most entertaining book of genuine travels ever written," and it certainly is one of the most instructive. His work on "Coral Reefs" appeared in 1842, but the substance had been communicated to the Geological Society soon after his return to England; his papers on "Volcanic Islands," on the "Distribution of Erratic Boulders and Contemporaneous Unstratified Deposits in South America," on the "Fine Dust which falls on Vessels in the Atlantic Ocean," and some other geological as well as zoölogical researches, were published previously to 1851. Between that year and 1855 he brought out his most considerable contributions to systematic zoölogy, his monographs on the Cirripedia and the fossil Lepadidæ.

We come to the first publication of what is now known as Darwinism. It consists of a sketch of the doctrine of Natural Selection, which was drawn up in the year 1839, and copied and communicated to Messrs. Lyell and Hooker in

1844, being a part of the manuscript of a chapter in his "Origin of Species;" also of a private letter addressed to the writer of this memorial in October, 1857, — the publication of which (in the Journal of the Proceedings of the Linnæan Society, Zoölogical Part, iii. 45–53, issued in the summer of 1858) was caused by the reception by Darwin himself of a letter from Mr. Wallace, inclosing a brief and strikingly similar essay on the same subject, entitled "On the Tendency of Varieties to depart indefinitely from the Original Type." Mr. Darwin's action upon the reception of this rival essay was characteristic. His own work was not yet ready, and the fact that it had been for years in preparation was known only to the persons above mentioned. He proposed to have the paper of Mr. Wallace (who was then in the Moluccas) published at once, in anticipation of his own leisurely prepared volume; and it was only under the solicitation of his friends cognizant of the case that his own early sketch and the corroboratory letter were printed along with it.

The precursory essays of Darwin and Wallace, published in the Proceedings of a scientific society, can hardly have been read except by a narrow circle of naturalists. Most thoughtful investigating naturalists were then in a measure prepared for them. But toward the close of the following year (in the autumn of 1859) appeared the volume "On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life," the first and most notable of that series of duodecimos which have been read and discussed in almost every cultured language, and which within the lifetime of their author have changed the face and in some respect the character of natural history, — indeed have almost as deeply affected many other lines of investigation and thought.

In this Academy, where the rise and progress of Darwinian evolution have been attentively marked and its bearings critically discussed, and at this date, when the derivative origin of animal and vegetable species is the accepted belief of all of us who study them, it would be superfluous to give any explanatory account of these now familiar writings; nor, indeed,

would the pages which we are accustomed to consecrate to the memory of our recently deceased associates allow of it. Let us note in passing that the succeeding volumes of the series may be ranked in two classes, one of which is much more widely known than the other. One class is of those which follow up the argument for the origination of species through descent with modification, or which widen its base and illustrate the *modus operandi* of Natural Selection. Such are the two volumes on "Domesticated Animals and Cultivated Plants," illustrating Variation, Inheritance, Reversion, Interbreeding, etc.; the volume on the "Descent of Man, and Selection in Relation to Sex," — which extended the hypothesis to its logical limits — and that, "On the Expression of the Emotions in Man and the Lower Animals," published in 1872, which may be regarded as the last of this series. Since then Mr. Darwin appears to have turned from the highest to the lower forms of life, and to have entered upon the laborious cultivation of new and special fields of investigation, which, although prosecuted on the lines of his doctrine and vivified by its ideas, might seem to be only incidentally connected with the general argument. But it will be found that all these lines are convergent. Nor were these altogether new studies. The germ of the three volumes upon the Relation of Insects to Flowers and its far-reaching consequences, is a little paper, published in the year 1858, "On the Agency of Bees in the Fertilization of Papilionaceous Flowers, and on the Crossing of Kidney Beans;" the first edition of the volume on "The various Contrivances by which Orchids are Fertilized by Insects" appeared in 1862, thus forming the second volume of the whole series; and the two volumes "On the Effects of Cross and Self-Fertilization in the Vegetable Kingdom," and "The different Forms of Flowers on Plants of the same Species," which, along with the new edition of "The Fertilization of Orchids," were all published in 1876 and 1877, originated in two or three remarkable papers contributed to the "Journal of the Linnæan Society" in 1862 and 1863, but are supplemented by additional and protracted experiments. The volume on "Insectivorous Plants," and the noteworthy

conclusions in respect to the fundamental unity, and therefore common source, of vegetable and animal life, grew out of an observation which the author made in the summer of 1860, when he "was surprised by finding how large a number of insects were caught by the leaves of the common Sun-dew (*Drosera rotundifolia*), on a heath in Sussex." Almost everybody had noticed this; and one German botanist (Roth), just a hundred years ago, had observed and described the movement of the leaf in consequence of the capture. But nothing came of it, or of what had been as long known of our Dionæa, beyond a vague wonderment, until Mr. Darwin took up the subject for experimental investigation. The precursor of his volume on "The Movements and Habits of Climbing Plants," published in 1875, as well as of the recent and larger volume on "The Power of Movement in Plants," 1880, was an essay published in the "Journal of the Linnæan Society" in 1865; and this was instigated by an accidental but capital observation made by a correspondent, in whose hands it was sterile; but it became wonderfully fertile when touched by Darwin's genius.[1] His latest volume, on "The Formation of Vegetable Mould through the Action of Worms," is a development, after long years, of a paper which he read before the Geological Society of London in 1837.

These subsidiary volumes are less widely known than those

[1] Mr. Darwin's quickness in divining the meaning of seemingly unimportant things is illustrated in his study of Dionæa. Noting that the trap upon irritation closes at first imperfectly, leaving some room within and a series of small interstices between the crossed spines, but after a time, if there is prey within, shuts down close, he at once inferred that this was a provision for allowing small insects to escape, and for retaining only those large enough to make the long process of digestion remunerative. To test the surmise, he asked a correspondent to visit the habitat of Dionæa at the proper season, and to ascertain by the examination of a large number of the traps in action whether any below a certain considerable size were to be found in them. The result confirmed the inference, a comparatively trivial but characteristic illustration of Darwin's confidence in the principle of utility, and a good example of the truth of the dictum, which was by some thought odd when first made, namely, that Darwin had restored teleology to natural history, from which the study of morphology had dissevered it.

of the other class; but they are of no less interest, and they are very characteristic of the author's genius and methods, — characteristic also of his laboriousness. For the amount of prolonged observation, watchful care, and tedious experiment they have demanded is as remarkable as the skill in devising simple and effectual modes of investigation is admirable. That he should have had the courage to undertake and the patience to carry on new inquiries of this kind after he had reached his threescore and ten years of age, and after he had attained an unparalleled breadth of influence and wealth of fame, speaks much for his energy and for his devotion to knowledge for its own sake. Indeed, having directed the flow of scientific thought into the new channel he had opened, along which the current set quicker and stronger than he could have expected, he seems to have taken up with fresh delight studies which he had marked out in early years, or topics which from time to time had struck his acute attention. To these he gave himself, quite to the last, with all the spirit and curiosity of youth. Evidently all this amount of work was done for the pure love of it; it was all done methodically, with clear and definite aim, without haste, but without intermission.

It would confidently be supposed that in this case genius and industry were seconded by leisure and bodily vigor. Fortunately Darwin's means enabled him to control the disposition of his time. But the voyage of the Beagle, which was so advantageous to science, ruined his health. A sort of chronic sea-sickness, under which all his work abroad was performed, harassed him ever afterwards. The days in which he could give two hours to investigation or writing were counted as good ones, and for much of his life they were largely outnumbered by those in which nothing could be attempted. Only by great care and the simplest habits was he able to secure even a moderate amount of comfortable existence. But in this respect his later years were the best ones, and therefore the busiest. In them also he had most valuable filial aid. There was nothing to cause much anxiety until his seventy-third birthday had passed, or to excite alarm until the week before his death.

It may without exaggeration be said that no scientific man, certainly no naturalist, ever made an impression at once so deep, so wide, and so immediate. The name of Linnæus might suggest comparison; but readers and pupils of Linnæus over a century ago were to those of Darwin as tens are to thousands, and the scientific as well as the popular interest of the subjects considered were somewhat in the same ratio. Humboldt, who, like Darwin, began with research in travel, and to whom the longest of lives, vigorous health, and the best opportunities were allotted, essayed similar themes in a more ambitious spirit, enjoyed equal or greater renown, but made no deep impression upon the thought of his own day or of ours. As one criterion of celebrity, it may be noted that no other author we know of ever gave rise in his own active lifetime to a special department of bibliography. Dante-literature and Shakespeare-literature are the growth of centuries; but *Darwinismus* had filled shelves and alcoves and teeming catalogues while the unremitting author was still supplying new and ever novel subjects for comment. The technical term which he chose for a designation of his theory, and several of the phrases originated in explanation of it only twenty-five years ago, have already been engrafted into his mother tongue, and even into other languages, and are turned to use in common as well as in philosophical discourse, without sense of strangeness.

Wonderful indeed is the difference between the reception accorded to Darwin and that met with by his predecessor, Lamarck. But a good deal has happened since Lamarck's day; wide fields of evidence were open to Darwin which were wholly unknown to his forerunner; and the time had come when the subject of the origin and connection of living forms could be taken up as a research rather than as a speculation. Philosophizers on evolution have not been rare; but Darwin was not one of them. He was a scientific investigator, — a philosopher, if you please, but one of the type of Galileo. Indeed very much what Galileo was to physical science in his time, Darwin is to biological science in ours. This without reference to the fact that the writings of both conflicted with

similar prepossessions; and that the Darwinian theory, legitimately considered, bids fair to be placed in this respect upon the same footing with the Copernican system.

An English poet wrote that he awoke one morning and found himself famous. When this happened to Darwin, it was a genuine surprise. Although he had addressed himself simply to scientific men, and had no thought of arguing his case before a popular tribunal, yet "The Origin of Species" was too readable a book upon too sensitive a topic to escape general perusal; and this, indeed, must in some sort have been anticipated. But the avidity with which the volume was taken up, and the eagerness of popular discussion which ensued, were viewed by the author — as his letters at the time testify — with a sense of amused wonder at an unexpected and probably transient notoriety.

The theory he had developed was presented by a working naturalist to his fellows, with confident belief that it would sooner or later win acceptance from the younger and more observant of these. The reason why these moderate expectations were much and so soon exceeded are not far to seek, though they were not then obvious to the world in general. Although mere speculations were mostly discountenanced by the investigating naturalists of that day, yet their work and their thoughts were, consciously or unconsciously, tending in the direction of evolution. Even those who manfully rowed against the current were more or less carried along with it, and some of them unwittingly contributed to its force. Most of them in their practical studies had worked up to, or were nearly approaching, the question of the relation of the past inhabitants of the earth to the present, and of the present to one another, in such wise as to suggest inevitably that, somehow or other, descent with modification was eventually to be the explanation. This was the natural outcome of the line of thought of which Lyell early became the cautious and fair-minded expositor, and with which he reconstructed theoretical geology. If Lyell had known as much at first hand of botany or zoölogy as he knew of geology, it is probable that his celebrated chapter on the permanence of species in the "Prin-

ciples" would have been reconsidered before the work had passed to the ninth edition in 1853. He was convinced that species went out of existence one by one, through natural causes, and that they came in one by one, bearing the impress of their immediate predecessors; but he saw no way to connect the two through natural operations. Nor, in fact, had any of the evolutionists been able to assign real causes capable of leading on such variations as are of well-known occurrence to wider and specific or generic differences. Just here came Darwin. When upon the spot he had perceived that the animals of the Galapagos must be modified forms derived from the adjacent continent, and he soon after worked out the doctrine of natural selection. This supplied what was wanting for the condensation of opinions and beliefs, and the collocation of rapidly accumulating facts, into a consistent and workable scientific theory, under a principle which unquestionably could directly explain much, and might indirectly explain more.

It is not merely that Darwin originated and applied a new principle. Not to speak of Wallace, his contemporary, who came to it later, his countryman, Dr. Wells, as Mr. Darwin points out, "distinctly recognizes the principle of natural selection, and this is the first recognition which has been indicated; but he applied it only to the races of men, and to certain characters alone." Darwin, like the rest of the world, was unaware of this anticipation until he was preparing the fourth edition of his "Origin of Species," in 1866, when he promptly called attention to it, perhaps magnifying its importance. However this be, Darwin appears to have been first and alone in apprehending and working out the results which necessarily come from the interaction of the surrounding agencies and conditions under which plants and animals exist, including, of course, their action upon each other. Personifying the *ensemble* of these and the consequences, — namely, the survival only of the fittest in the struggle for life, — under the term of Natural Selection, Mr. Darwin, with the instinct of genius divined, and with the ability of a master worked out its pregnant and far-reaching applications. He not only

saw its strong points, but he foresaw its limitations, indicated most of the objections in advance of his opponents, weighed them with judicial mind, and where he could not obviate them, seemed never disposed to underrate their force. Although naturally disposed to make the most of his theory, he distinguished between what he could refer to known causes and what thus far is not referable to them. Consequently, he kept clear of that common confusion of thought which supposes that natural selection originates the variations which it selects. He believed, and he has shown it to be probable, that external conditions induce the actions and changes in the living plant or animal which may lead on to the difference between one species and another; but he did not maintain that they produced the changes, or were sufficient scientifically to explain them. Unlike most of his contemporaries in this respect, he appears to have been thoroughly penetrated by the idea that the whole physiological action of the plant or animal is a response of the living organism to the action of the surroundings.

The judicial fairness and openness of Darwin's mind, his penetration and sagacity, his wonderful power of eliciting the meaning of things which had escaped questioning by their very commonness, and of discerning the great significance of causes and interactions which had been disregarded on account of their supposed insignificance, his method of reasoning close to the facts and in contact with the solid ground of nature, his aptness in devising fruitful and conclusive experiments, and in prosecuting nice researches with simple but effectual appliances, and the whole rare combination of qualities which made him *facile princeps* in biological investigation, — all these gifts are so conspicuously manifest in his published writings, and are so fully appreciated, that there is no need to celebrate them in an obituary memorial. The writings also display in no small degree the spirit of the man, and to this not a little of their persuasiveness is due. His desire to ascertain the truth, and to present it purely to his readers, is everywhere apparent. Conspicuous, also, is the absence of all trace of controversy and of everything like

pretension; and this is remarkable, considering how censure and how praise were heaped upon him without stint. He does not teach didactically, but takes the reader along with him as his companion in observation and in experiment. And in the same spirit, instead of showing pique to an opponent, he seems always to regard him as a helper in his search for the truth. Those privileged to know him well will certify that he was one of the most kindly and charming, unaffected, simple-hearted, and lovable of men.

How far and how long the Darwinian theory will hold good, the future will determine. But in its essential elements, apart from *à priori* philosophizing, with which its author had nothing to do, it is an advance from which it is evidently impossible to recede. As has been said of the theory of the Conservation of Energy, so of this: "The proof of this great generalization, like that of all other generalizations, lies mainly in the fact that the evidence in its favor is continually augmenting, while that against it is continually diminishing, as the progress of science reveals to us more and more of the workings of the universe."

JOSEPH DECAISNE.[1]

Joseph Decaisne, the oldest member of the Botanical Section on the foreign list, died at Paris, on the 8th of February last, in the seventy-fifth year of his age. He was elected into this Academy in August, 1846, along with Agassiz and De Verneuil. He was born at Brussels, March 11, 1807, the second of three brothers, one of whom became a distinguished painter, and the other the head of the medical department of the Belgian army. He came to Paris and entered the Jardin des Plantes when a lad of seventeen years, and in its service his whole subsequent life was passed. The young employé attracted the attention of Adrien de Jussieu, who, seeing his promise and unusual botanical knowledge, soon placed him at the head of the seed department, and in 1833 made him his *Aide-naturaliste*, thus giving the young gardener opportunity for the studies and researches by which he won a place among the foremost botanists of the time. For more than forty years the administration of the Jardin des Plantes and the duties of the chair of Culture at the Museum were in his hands, he having supplied the place of Mirbel through the closing years of the latter's life, and succeeded him as professor in the year 1851; and these duties he continued to fulfill to the last. He was elected a member of the Institute in 1847, in succession to Dutrochet; for forty years he was one of the editors of, and since the death of his colleague, Adolphe Brongniart, he was the sole editor of the botanical portion of the "Annales des Sciences Naturelles." In the Annales he had published some good botanical papers, the earliest in the year 1831. But his first distinction was gained by his anatomical and physiological researches upon the Madder-plant, a monograph containing the results of which appeared at Brussels in 1837, and was said to be "one of the most able memoirs that has ever been published on the physiological history of plants and their bearing on

[1] Proceedings American Academy of Arts and Science, xvii. 458. (1882.)

practical cultivation and manufactures." Two years later, in connection with the chemist Péligot, he published an investigation of the anatomical structure of the Sugar-beet. His classical memoir upon the structure and development of the Mistletoe appeared in 1840, and is of purely scientific interest. In the year 1841 he showed that the Corallines, which had been wrongly carried over to the animal kingdom with the Corals and their allies, were genuine Seaweeds, disguised by the incorporation of a great amount of lime into their tissues. And about this time, in connection with his friend and former pupil, Thuret, he discovered and illustrated the male organs of the *Fuci*, as well as the mode of impregnation and reproduction, thus initiating the investigations which in the hands of the late Thuret and others have revolutionized phycology.

Leaving these researches for his associate to complete and publish, thenceforth Decaisne turned all his attention to phanerogamous botany, morphological and systematic. Two orders were elaborated by him for De Candolle's "Prodromus," *Asclepiadaceæ* and *Plantaginaceæ*, the former demanding much minute research; he produced in 1868, in conjunction with Le Maout, that admirable text-book, the "Traité Générale de Botanique," profusely illustrated by his own facile pencil, which is well known in the original and in the English translation edited by Sir Joseph Hooker. But the works by which he will be most widely known, and which were connected especially with his directorship of the Jardin des Plantes, are that incomparable series of colored illustrations of fruits, together with descriptive text, known as "Le Jardin Fruitier du Muséum," and his subsidiary investigations and publications upon the *Pomaceæ* and their allies. These important publications began in the year 1858, and were completed only a year or two ago.

Decaisne never married: he lived his simple and devoted life in the house on Rue Cuvier in the Jardin des Plantes, where he died, regretted and beloved, the last of the line of illustrious botanists — such as Mirbel, Adrien de Jussieu, Gaudichaud, and Adolphe Brongniart — who were associated in the administration of this institution thirty or forty years ago.

GEORGE ENGELMANN.[1]

In the death of Dr. Engelmann, which took place on the 4th of February last, the American Academy has lost one of its very few Associate Fellows in the Botanical Section, and the science one of its most eminent and venerable cultivators.

He was born at Frankfort-on-the-Main, February 2, 1809, and had therefore just completed his seventy-fifth year. His father, a younger member of the family of Engelmanns who for several generations served as clergymen at Bacharach on the Rhine, was also educated for the ministry, and was a graduate of the university of Halle, but he devoted his life to education. Marrying the daughter of George Oswald May, a somewhat distinguished portrait-painter, they established at Frankfort, and carried on for a time with much success, a school for young ladies, such as are common in the United States, but were then a novelty in Germany.

George Engelmann was the eldest of thirteen children born of this marriage, nine of whom survived to manhood. Assisted by a scholarship founded by "the Reformed Congregation of Frankfort," he went to the university of Heidelberg in the year 1827, where he had as fellow-students and companions Karl Schimper and Alexander Braun. With the latter he maintained an intimate friendship and correspondence, interrupted only by the death of Braun in 1877. The former, who manifested unusual genius as a philosophical naturalist, after laying the foundations of phyllotaxy, to be built upon by Braun and others, abandoned, through some singular infirmity of temper, an opening scientific career of the highest promise, upon which the three young friends, Agassiz, Braun, and Schimper, and in his turn Engelmann, had zealously entered.

[1] Proceedings American Academy of Arts and Science, xix. 516. (1884.)

Embarrassed by some troubles growing out of a political demonstration by the students at Heidelberg, Engelmann in the autumn of 1828 went to the university of Berlin for two years; and thence to Würzburg, where he took his degree of Doctor in Medicine in the summer of 1831. His inaugural dissertation, "De Antholysi Prodromus," which he published at Frankfort in 1832, testifies to his early predilection for botany, and to his truly scientific turn of mind. It is a morphological dissertation, founded chiefly on the study of monstrosities, illustrated by five plates filled with his own drawings. It was therefore quite in the line with the little treatise on the Metamorphosis of Plants, published forty years before by another and the most distinguished native of Frankfort, and it appeared so opportunely that it had the honor of Goethe's notice and approval. Goethe's correspondent, Madame von Willema, sent a copy to him only four weeks before his death. Goethe responded, making kind inquiries after young Engelmann, who, he said, had completely apprehended his ideas of vegetable morphology, and had shown such genius in their development that he offered to place in this young botanist's hands the store of unpublished notes and sketches which he had accumulated.

The spring and summer of 1832 were passed at Paris in medical and scientific studies, with Braun and Agassiz as companions, leading, as he records, "a glorious life in scientific union, in spite of the cholera." Meanwhile, Dr. Engelmann's uncles had resolved to make some land investments in the valley of the Mississippi, and he willingly became their agent. At least one of the family was already settled in Illinois, not far from St. Louis. Dr. Engelmann, sailing from Bremen for Baltimore in September, joined his relatives in the course of the winter, made many lonely and somewhat adventurous journeys on horseback in southern Illinois, Missouri, and Arkansas, which yielded no other fruits than those of botanical exploration; and finally he established himself in the practice of medicine at St. Louis, late in the autumn of 1835. St. Louis was then rather a frontier trading-post than a town, of barely eight or ten thousand

inhabitants. He lived to see it become a metropolis of over four hundred thousand. He began in absolute poverty, the small means he had brought from Europe completely exhausted. In four years he had laid the foundations of success in his profession, and had earned the means for making a voyage to Germany, and, fulfilling a long-standing engagement, for bringing to a frugal home the chosen companion of his life, Dora Hartsmann, his cousin, whom he married at Kreuznach, on the 11th of June, 1840. On his way homeward, at New York, the writer of this memorial formed the personal acquaintance of Dr. Engelmann; and thus began the friendship and the scientific association which has continued unbroken for almost half a century.

Dr. Engelmann's position as a leading physician in St. Louis, as well among the American as the German and French population, was now soon established. He was even able in 1856, without risk, to leave his practice for two years, to devote most of the first summer to botanical investigation in Cambridge, and then, with his wife and young son, to revisit their native land, and to fill up a prolonged vacation in interesting travel and study. In the year 1868 the family visited Europe for a year, the son remaining to pursue his medical studies in Berlin. And lastly, his companion of nearly forty years having been removed by death in January, 1879, and his own robust health having suffered serious and indeed alarming deterioration, he sailed again for Germany in the summer of 1883. The voyage was so beneficial that he was able to take up some botanical investigations, which, however, were soon interrupted by serious symptoms. But the return voyage proved wonderfully restorative; and when, in early autumn, he rejoined his friends here, they could hope that the unfinished scientific labors, which he at once resumed with alacrity of spirit, might still for a while be carried on with comfort. So indeed they were, in some measure, after his return to his home, yet with increasing infirmity and no little suffering until the sudden illness supervened which, in a few days, brought his honorable and well-filled life to a close.

In the latter part of his life Dr. Engelmann was able to explore considerable portions of his adopted country, the mountains of North Carolina and Tennessee, the Lake Superior region, and the Rocky Mountains and contiguous plains in Colorado and adjacent territories, and so to study in place, and with the particularity which characterized his work, the *Cacti*, the *Coniferæ*, and other groups of plants which he had for many years been specially investigating. "In 1880 he made a long journey through the forests of the Pacific States, where he saw for the first time in the state of nature plants which he had studied and described more than thirty years before. Dr. Engelmann's associates [so one of them declares] will never forget his courage and industry, his enthusiasm and zeal, his abounding good-nature, and his kindness and consideration of every one with whom he came in contact." His associates, and also all his published writings, may testify to his acuteness in observation, his indomitable perseverance in investigation, his critical judgment, and a rare openness of mind which prompted him continually to revise old conclusions in the light of new facts or ideas.

In the consideration of Dr. Engelmann's botanical work — to which these lines will naturally be devoted — it should be remembered that his life was that of an eminent and trusted physician, in large and general practice, who even in age and failing health was unable, however he would have chosen, to refuse professional services to those who claimed them; that he devoted only the residual hours, which most men use for rest or recreation, to scientific pursuits, mainly to botany, yet not exclusively. He was much occupied with meteorology. On establishing his home at St. Louis, he began a series of thermometrical and barometrical observations, which he continued regularly and systematically to the last, when at home always taking the observations himself, — the indoor ones even up to the last day but one of his life. Even in the last week he was seen sweeping a path through the snow in his garden to reach his maximum and minimum thermometers. His latest publication (issued since his death by the St. Louis Academy of Sciences) is a digest and full representation of

the thermometrical part of these observations for forty-seven years. He apologizes for not waiting the completion of the half-century before summing up the results, and shows that these could not after three more years be appreciably different.

A list of Dr. Engelmann's botanical papers and notes, collected by his friend and associate, Professor Sargent, and published in Coulter's "Botanical Gazette" for May, 1884, contains about one hundred entries, and is certainly not quite complete. His earliest publication, his inaugural thesis already mentioned (De Antholysi Prodromus), is a treatise upon teratology in its relations to morphology. It is a remarkable production for the time and for a mere medical student with botanical predilections. There is an interesting recent analysis of it in "Nature" for April 24, by Dr. Masters, the leading teratologist of our day, who compares it with Moquin-Tandon's more elaborate "Tératologie Végétale," published ten years afterwards, and who declares that, "when we compare the two works from a philosophical point of view, and consider that the one was a mere college essay, while the other was the work of a professed botanist, we must admit that Engelmann's treatise, so far as it goes, affords evidence of deeper insight into the nature and causes of the deviations from the ordinary conformation of plants than does that of Moquin."

Transferred to the valley of the Mississippi and surrounded by plants most of which still needed critical examination, Dr. Engelmann's avocation in botany and his mode of work were marked out for him. Nothing escaped his attention; he drew with facility; and he methodically secured his observations by notes and sketches, available for his own after-use and for that of his correspondents. But the lasting impression which he has made upon North American botany is due to his wise habit of studying his subjects in their systematic relations, and of devoting himself to a particular genus or group of plants (generally the more difficult) until he had elucidated it as completely as lay within his power. In this way all his work was made to tell effectively.

Thus his first monograph was of the genus Cuscuta (published in the American Journal of Science, in 1842), of which when Engelmann took it up we were supposed to have only one indigenous species, and that not peculiar to the United States, but which he immediately brought up to fourteen species without going west of the Mississippi Valley. In the year 1859, after an investigation of the whole genus in the materials scattered through the principal herbaria of Europe and this country, he published in the first volume of the St. Louis Academy of Sciences a systematic arrangement of all the *Cuscutæ*, characterizing seventy-seven species, besides others classed as perhaps varieties.

Mentioning here only monographical subjects, we should next refer to his investigations of the Cactus family, upon which his work was most extensive and important, as well as particularly difficult, and upon which Dr. Engelmann's authority is of the very highest. He essentially for the first time established the arrangement of these plants upon floral and carpological characters. This formidable work was begun in his "Sketch of the Botany of Dr. A. Wislizenus's Expedition from Missouri to Northern Mexico," in the latter's memoir of this tour, published by the United States Senate. It was followed up by his account (in the American Journal of Science, 1852) of the Giant Cactus on the Gila (*Cereus giganteus*) and an allied species; by his synopsis of the *Cactaceæ* of the United States, published in the "Proceedings of the American Academy of Arts and Sciences," 1856; and by his two illustrated memoirs upon the southern and western species, one contributed to the fourth volume of the series of Pacific Railroad Expedition Reports, the other to Emory's Report on the Mexican Boundary Survey. He had made large preparations for a greatly needed revision of at least the North American *Cactaceæ*. But, although his collections and sketches will be indispensable to the future monographer, very much knowledge of this difficult group of plants is lost by his death.

Upon two other peculiarly American groups of plants, very difficult of elucidation in herbarium specimens, Yucca and Agave, Dr. Engelmann may be said to have brought his work

up to the time. Nothing of importance is yet to be added to what he modestly styles "Notes on the Genus Yucca," published in the third volume of the Transactions of the St. Louis Academy, 1873, and not much to the "Notes on Agave," illustrated by photographs, included in the same volume and published in 1875.

Less difficult as respects the material to work upon, but well adapted for his painstaking, precise, and thorough handling, were such genera as Juncus (elaborately monographed in the second volume of the Transactions of the St. Louis Academy, and also exemplified in distributed sets of specimens), Euphorbia (in the fourth volume of the Pacific Railroad Reports, and in the Botany of the Mexican Boundary), Sagittaria and its allies, Callitriche, Isoetes (of which his final revision is probably ready for publication), and the North American *Loranthaceæ*, to which Sparganium, certain groups of Gentiana, and some other genera, would have to be added in any complete enumeration. Revisions of these genera were also kindly contributed to Dr. Gray's Manual; and he was an important collaborator in several of the memoirs of his surviving associate and friend.

Of the highest interest, and among the best specimens of Dr. Engelmann's botanical work, are his various papers upon the American Oaks and the *Coniferæ*, published in the "Transactions of the St. Louis Academy," and elsewhere, the results of long-continued and most conscientious study. The same must be said of his persevering study of the North American Vines, of which he at length recognized and characterized a dozen species, — excellent subjects for his nice discrimination, and now becoming of no small importance to grape-growers, both in this country and in Europe. Nearly all that we know scientifically of our species and forms of Vitis is directly due to Dr. Engelmann's investigations. His first separate publication upon them, "The Grape Vines of Missouri," was published in 1860; his last, a reëlaboration of the American species, with figures of their seeds, is in the third edition of the Bushberg Catalogue, published only a few months ago.

Imperfect as this mere sketch of Dr. Engelmann's botanical authorship must needs be, it may show how much may be done for science in a busy physician's *horæ subsecivæ*, and in his occasional vacations. Not very many of those who could devote their whole time to botany have accomplished as much. It need not be said, and yet perhaps it should not pass unrecorded, that Dr. Engelmann was appreciated by his fellow-botanists both at home and abroad, that his name is upon the rolls of most of the societies devoted to the investigation of nature, that he was "everywhere the recognized authority in those departments of his favorite science which had most interested him," and that, personally one of the most affable and kindly of men, he was as much beloved as respected by those who knew him.

More than fifty years ago his oldest associates in this country — one of them his survivor — dedicated to him a monotypical genus of plants, a native of the plains over whose borders the young immigrant on his arrival wandered solitary and disheartened. Since then the name of Engelmann has, by his own researches and authorship, become unalterably associated with the Buffalo-grass of the plains, the noblest Conifers of the Rocky Mountains, the most stately Cactus in the world and with most of the associated species, as well as with many other plants of which perhaps only the annals of botany may take account. It has been well said by a congenial biographer, that "the western plains will still be bright with the yellow rays of *Engelmannia*, and that the splendid Spruce, the fairest of them all, which bears the name of Engelmann, will still, it is to be hoped, cover with noble forests the highest slopes of the Rocky Mountains, recalling to men, as long as the study of trees occupies their thoughts, the memory of a pure, upright, and laborious life."

OSWALD HEER.[1]

Oswald Heer, the most eminent investigator of the fossil plants and insects of the tertiary period, died on the 27th of September last, shortly after he had entered upon the seventy-fifth year of his age.

He was born at the hamlet of Nieder-Utzwyl, in Canton St. Gallen, Switzerland, August 31, 1809, passed most of his youth at Matt, in Canton Glarus, where his father was the parish clergyman, pursued his academic and professional studies at the university of Halle, and was ordained as minister of the Gospel in the year 1831. The next year he went to Zurich, where he resided for the rest of his life. Here he studied medicine for a time, but soon devoted himself seriously to entomology and botany, of which he was fond from boyhood. In 1834 he became Privat-docent of these sciences; in 1852, when the university of Zurich was developed, he became its professor of botany, and in 1855 he took a similar chair in the Polytechnicum. Most of his earlier publications were entomological; and it was by the way of entomology that he entered upon his distinguished career as a palæontologist. His life-long friend, the eminent Escher von der Linth, appreciating his rare powers of observation, induced him to undertake the study of the fossil insects of the celebrated tertiary deposits of Oeningen. The results of his labors in this virgin field were published between the years 1847 and 1853. His attention had from the first been attracted to the plants associated with the insect remains. His first palæo-botanical paper appeared in 1851; the three volumes of his "Flora Tertiaria Helvetiæ" were issued between 1855 and 1859; in 1862 his memoir on the fossil flora of Bovey-Tracey (Eng-

[1] Proceedings of the American Academy of Arts and Science, xix. 556. (1884.)

land) was published in the Philosophical Transactions of the Royal Society, London. About the same time also appeared a paper in the Journal of the Geological Society on certain fossil plants of the Isle of Wight. For the benefit of his health, always delicate and then much impaired, he passed the winter of 1854–55 in Madeira, and on his return published a paper on the fossil plants of that island, and an article on the probable origin of the actual flora and fauna of the Azores, Madeira, and the Canaries. In this, and in his work, published in 1860, on "Tertiary Climates in their Relation to Vegetation" (which the next year appeared also in a French translation by his young friend Gaudin), Heer brought out his theory of a Miocene Atlantis. His more extensive and popular treatise upon past climates as illustrated by vegetable palæontology, his "Urwelt der Schweiz," — a vivid portraiture of the past of his native country, — appeared in 1865, and afterwards in a revised French edition, with his friend Gaudin (who died soon after) for collaborator as well as translator. There was also an English translation by Heywood, published in 1876, and, indeed, it is said to have been translated into six languages.

In 1877 Heer completed his "Flora Fossilis Helvetiæ," a square-folio volume, with seventy plates, which extended and supplemented his Tertiary Flora of that country, being devoted to the illustration of the fossil plants of the Carboniferous, the Triassic, the Jurassic, and the Cretaceous, as well as the Eocene formations.

The life-long delicacy of Heer's health prevented his making any extensive explorations in person. But materials for his investigation came to him in even embarrassing abundance, not only from his own country, — where, even before he was widely known (as his fellow-countryman and his distinguished fellow-worker in palæo-botany, Lesquereux, informs us), a lady opened upon her property near Lausanne quarries and tunnels expressly for the discovery and collection of fossil plants, and sent them by tons to Zurich, — but from all parts of the world collections were pressed upon him, and his whole time and strength were given to their study. In this way he

became interested in the arctic fossil flora, of which he became the principal investigator and expounder. His first essay in the domain which he has made so peculiarly his own was in a paper on certain fossil plants of Vancouver's Island and British Columbia, published in 1865; and in 1868 he brought out the first of that most important series of memoirs upon the ancient floras of arctic America, Greenland, Spitzbergen, Nova Zembla, arctic and subarctic Asia, etc., which, collected, make up the seven quarto volumes of the "Flora Fossilis Arctica." The seventh volume of this monumental work was brought to a conclusion only a few months before the author's death.

Heer's researches into the fossil botany of the tertiary deposits were very important in their bearings. They made it certain that our actual temperate floras round the world had a common birthplace at the north, where the continents are in proximity; they essentially identified the direct or collateral ancestors of our existing forest-trees which flourished within the arctic zone when it enjoyed a climate resembling our own at present; and they leave the similarities and the dissimilarities of the temperate floras of the Old and the New World to be explained as simple consequences of established facts. Thus Heer himself did away with his own hypothesis of a continental Atlantis by bringing to light the facts which proved that there was no need of it. And while thus justifying the ideas which had been brought forward in one of the memoirs of the American Academy (in 1859) before these fossil data were known, he was not slow to adopt and to extend the tentative views which he had confirmed.

A list of Heer's scientific publications is given in the "Botanisches Centralblatt," No. 5, for 1884. They are seventy-seven in number, besides the seven quarto volumes of the "Flora Fossilis Arctica," which comprise a considerable number of independent memoirs. These works make an era in vegetable palæontology. Their crowning general interest is that they bring the vegetation of the past into direct connection with the present.

Although he lived to a good old age, and was never inac-

tive, Heer was for most of his life an invalid, suffering from pulmonary disease. For the last twelve years his work was carried on at his bedside or from his bed, assisted by a devoted and accomplished daughter; he seldom left his house, except to pass the last two winters in the milder climate of Italy. Last summer, having finished his "Flora Fossilis Arctica," in the hope of recruiting his exhausted strength he was removed to the most sheltered spot on the shores of the Lake of Geneva, but without benefit. He died at Lausanne, at his brother's house, on the 27th of September, 1883. It has been well said of him, in a tribute which a personal friend and fellow-naturalist paid to his memory, that "a man more lovable, more sympathetic, and a life more laborious and pure, one could scarcely imagine."

Heer was elected into the Academy in May, 1877. He is botanically commemorated in a genus of beautiful Melastomaceous plants indigenous to Mexico.

GEORGE BENTHAM.[1]

GEORGE BENTHAM, one of the most distinguished botanists of the present century, and at the time of his death one of the oldest, was born at Stoke, a suburb of Portsmouth, September 22, 1800. He died at his house, No. 25 Wilton Place, London, on the 10th of September, 1884, a few days short of eighty-four years old. His paternal grandfather, Jeremiah Bentham, a London attorney or solicitor, had two sons, who both became men of mark, Jeremy and Samuel. The latter and younger had two sons, only one of whom, the subject of this memoir, lived to manhood. George Bentham's mother was a daughter of Dr. George Fordyce, a Scottish physician who settled in London, was a Fellow of the Royal Society, a lecturer on chemistry, and the author of some able medical works, also of a treatise upon Agriculture and Vegetation. It was from his mother that George Bentham early imbibed a fondness for botany.

The early part of his life and education was somewhat eventful and peculiar, and in strong contrast with the later. His father, General, subsequently Sir Samuel Bentham, was an adept in naval architecture. At the age of twenty-two he visited the arsenals of the Baltic for the improvement of his knowledge; thence he traveled far into Siberia. He became intimate with Prince Potemkin, by whom he was induced to enter the civil and afterwards the military service of the Empress Catharine. He took part in a naval action against the Turks on the Black Sea, and was rewarded with the command of a regiment stationed in Siberia, with which he traversed the country even to the frontiers of China. After ten years he returned to England, where his inventive skill and experience found a fitting field in the service of the Admiralty, in

[1] Proceedings American Academy of Arts and Science, xx. 527. (1884.)

which he attained the post of Inspector-General of Naval Works. Among the services he rendered was that of bringing to England the distinguished engineer Isambard Mark Brunel. In the year 1805, General Bentham was sent by the Admiralty to St. Petersburg, to superintend the building in Russia of vessels for the British navy. He took his family with him ; and there began the education of George Bentham, in the fifth year of his age, under the charge of a Russian lady who could speak no English, where he learned to converse fluently in Russian, French, and German, besides acquiring the rudiments of Latin as taught by a Russian priest. On the way back to England, two or three years later, the detention of a month or two in Sweden gave opportunity for learning enough of Swedish to converse in that language and to read it with tolerable ease in after life. Returning to England, the family settled at Hampstead, and the children pursued their studies under private tutors. In the years 1812–13, during the excitement produced by the French invasion of Russia and the burning of Moscow, our young polyglot "budded into an author, by translating (along with his brother and sister) and contributing to a London magazine a series of articles from the Russian newspapers, detailing the operations of the armies." In 1814, upon the downfall of Napoleon, the Bentham family crossed over to France, prepared for a long stay, remained in the country (at Tours, Saumur, and Paris) during the hundred days preceding Napoleon's final overthrow, and in 1816 Sir Samuel Bentham set out upon a prolonged and singular family tour, *en caravane*, through the western and southern departments of France. To quote from the published account from which most of these biographical details are drawn, and which were taken from Mr. Bentham's own memoranda [1] —

"The *cortége* consisted of a two-horse coach fitted up as a sleeping apartment ; a long, low, two-wheeled, one-horse spring van for General and Mrs. Bentham, furnished with a library and piano ; and another, also furnished, for his daughters and their governess. The plan followed was to travel by day

[1] An article in "Nature," Oct. 2, 1884, by Sir Joseph Dalton Hooker.

from one place of interest to another, bivouacking at night by the road, or in the garden of a friend, or in the precincts of the prefectures, to which latter he had credentials from the authorities in the capital. In this way he visited Orleans, Tours, Angoulême, Bordeaux, Toulouse, Montpellier, and finally Montauban, where a lengthened stay was made in a country house hired for the purpose. From Montauban (the *cortége* having broken down in some way) they proceeded still by private conveyances to Carcassonne, Narbonnes, Nîmes, Tarascon, Marseilles, Toulon, Hyères."

It was in the early part of this tour that young Bentham's attention was first turned to botany. Happening to take up De Candolle's edition of Lamarck's "Flore Française," which his mother, who was fond of the subject, had just purchased, he was struck with the methodical analytical tables, and he proceeded immediately to apply them to the first plant he could lay hold of. "His success led him to pursue the diversion of naming every plant he met with." During his long stay at Montauban he entered as a student in the Protestant theological school of that town, pursuing "with ardor the courses of mathematics, Hebrew, and comparative philology, the latter a favorite study in after life," and at home giving himself to music, in which he was remarkably gifted, to Spanish, to botany, and, with great relish, to society. Soon after, the family was established upon a property of 2000 acres, purchased by his father in the vicinity of Montpellier. Here he resumed the intimacy of his boyhood with John Stuart Mill, who was five years his junior, and whose life-long taste for botany was probably fixed during this residence of seven or eight months in the Bentham family in the year 1820. About this time Bentham occupied himself with ornithology and then with entomology, finding time, however, for another line of study; for at the age of twenty he had begun a translation into French of his Uncle Jeremy's "Chrestomathia," which was published in Paris some years afterwards; and he soon after translated also the essay on "Nomenclature and Classification." This was followed by his own "Essai sur la Nomenclature et Classification," published in Paris. This,

his original scientific production, was one of some mark, for it is praised by Stanley-Jevons in his recent "History of the Sciences."

On attaining his majority, his elder and only brother having died, he was placed in management of his father's Provençal estate, an employment which he took up with alacrity and prosecuted with success, turning to practical account his methodical habits, his indomitable industry, and his familiarity with Provençal country life and language. The latter he spoke like a native. A language always seemed to come to him without effort. Meanwhile his leisure hours were given to philosophical studies, his holidays to botanical excursions into the Cevennes and the Pyrenees. In the year 1853, a visit to England upon business relating to his father's French estate, where it seemed probable that he was to spend his life, was followed by circumstances which gave him back to his native country. He brought to his Uncle Jeremy a French translation of the latter's "Chrestomathia"; he made the acquaintance of Sir James Edward Smith, Robert Brown, Lambert, Don, and the other English botanists of the day; visited Sir William, then Professor Hooker, at Glasgow, and Walker Arnott in Edinburgh; took the latter with him the next summer to France, where the two botanists herborized together in Languedoc and the Pyrenees; and, returning to London, he accepted his uncle's pressing invitation to remain and devote a portion of his time to the preparation of the latter's manuscripts for the press, at the same time pursuing legal studies at Lincoln's Inn. He was in due time called to the bar, and in 1832 he held his first and last brief. In that year Jeremy Bentham died, bequeathing most of his property to his nephew. This was much less than was expected, owing to bad management on his uncle's part and to the extravagant sums spent by his executors in the publication of the philosopher's posthumous work. But it sufficed, in connection with the paternal inheritance, which fell to him in the year previous, for the modest independence which allowed of undistracted devotion to his favorite studies. These were for a time divided between botany, jurisprudence, and logic, not to speak of edi-

torial work upon his father's papers relating to the management of the navy and the administration of the national dockyards.

The first publication was botanical, and was published in Paris, in the year 1826, — his "Catalogue des Plantes Indigènes des Pyrénées et du Bas Languedoc." To this is prefixed an interesting narrative of a botanical tour in the Pyrenees, and some remarks upon the mode of preparing such catalogues in order to bring out their greatest utility, — remarks which already evince the wisdom for which he was distinguished in after years. He also reformed and reëlaborated our difficult genera of the district, — Cerastium, Orobanche, Helianthemum, and Medicago. The next, perhaps, was an article upon codification — wholly disagreeing with his uncle — which attracted the attention of Brougham, Hume, and O'Connell; also one upon the laws affecting larceny, which Sir Robert Peel complemented and made use of, and another on the law of real property.

But his most considerable work of the period received scant attention at the time from those most interested in the subject, and passed from its birth into oblivion, from which only in these later years it has been rescued, yet without word or sign from its author. This work (of 287 octavo pages) was published in London in 1827, under the title of "Outline of a New System of Logic, with a critical examination of Dr. Whately's Elements of Logic." It was in this book that the quantification of the predicate was first systematically applied, in such wise that Stanley-Jevons[1] declares it to be "undoubtedly the most fruitful discovery made in abstract logical science since the time of Aristotle." Before sixty copies of the book had been sold, the publisher became bankrupt, and the whole impression of this work of a young and unknown author was sold for waste paper. One of the extant copies, however, came into the hands of the distinguished philosopher Sir William Hamilton, to whom the discovery of the quantification of the predicate was credited, and who, in claiming it, brought "an acrimonious charge of plagiarism" against Pro-

[1] In "Contemporary Review," xxi., 1873, p. 823.

fessor De Morgan upon this very subject. Yet this very book of Mr. Bentham is one of the ten placed by title at the head of Sir William Hamilton's article on logic in the Edinburgh Review for April, 1833, is once or twice referred to in the article, and, a dozen years later, in the course of the controversy with De Morgan, Sir William alluded to this article as containing the germs of his discovery. We may imagine the avidity with which De Morgan, injuriously attacked, would have seized upon Mr. Bentham's book if he had known of it. It is not so easy to understand how Mr. Bentham, although now absorbed in botanical researches, could have overlooked this controversy in the "Athenæum," or how, if he knew of it, he could have kept silence. It was only at the close of the year 1850 that Mr. Warlow sent from the coast of Wales a letter to the "Athenæum," in which he refers to Bentham's book as one which had long before anticipated this interesting discovery. Although Hamilton himself never offered explanation of his now unpleasant position (for the note obliquely referring to the matter in the second edition of his Discussions is not an explanation), Mr. Blaine did (in the "Athenæum for February 1, 1851) immediately endeavor to discredit the importance of Bentham's work, and again in 1873 ("Contemporary Review," xxi.), in reply to Herbert Spencer's reclamation of Bentham's discovery. To this Stanley-Jevons made reply in the same volume (pp. 821–824); and later, in his "Principles of Science" (ii. 387), this competent and impartial judge, in speaking of the connection of Bentham's work "with the great discovery of the quantification of the predicate," adds: —

"I must continue to hold that the principle of quantification is explicitly stated by Mr. Bentham; and it must be regarded as a remarkable fact in the history of logic, that Hamilton, while vindicating in 1847 his own claims to originality and priority as against the scheme of De Morgan, should have overlooked the much earlier and more closely related discoveries of Bentham."

It must be that Hamilton reviewed Bentham's book without reading it through, or that its ideas did not at the time leave

any conscious impression upon the reviewer's mind, yet may have fructified afterwards.

After his uncle's death in 1832, Mr. Bentham gave his undivided attention to botany. He became a Fellow of the Linnæan Society in 1828. Robert Brown soon after presented his name to the Royal Society, but withdrew it before the election, to mark the dissatisfaction on the part of scientific men with the management of the society when a royal duke was made president. Consequently he did not become F. R. S. until 1862. In 1829, when the Royal Horticultural Society was much embarrassed, he accepted the position of honorary secretary, with his friend Lindley as associate. Under their management it was soon extricated from its perilous condition, attained its highest prosperity and renown, and did its best work for horticulture and botany. In 1833 he married the daughter of Sir Harford Brydges, for many years British ambassador in Persia, and the next year he took up his residence in the house in Queen Square Place, Westminster, inherited from his uncle, in which Jeremy Bentham and his own paternal grandfather had dwelt for almost a century. The house no longer exists, but upon its site stands the western wing of the "Queen Anne Mansions." The summer of 1836 was passed in Germany, at points of botanical interest and wherever the principal herbaria are preserved, the whole winter in Vienna. Some account of this tour, and interesting memoranda of the botanists, gardens, and herbaria visited, communicated in familiar letters to Sir William Hooker, were printed at the time (without the author's name) in the second volume of the "Companion to the Botanical Magazine." Similar visits for botanical investigation, mingled with recreation, were made almost every summer to various parts of the continent; in one of them he revisited the scenes of his early boyhood in Russia, traveled with Mrs. Bentham to the fair at Nijni-Novgorod, and thence to Odessa, by the rude litter-like conveyances of the country.

In 1842 he removed with his herbarium to Pontrilas House in Herefordshire, an Elizabethan mansion belonging to his brother-in-law, and combined there the life of a country squire

with that of a diligent student, until 1854, when, returning to London, he presented his herbarium and botanical library to the Royal Gardens at Kew, where they were added to the still larger collections of Sir William Hooker. After a short interval Mr. Bentham took up his residence at No. 25 Wilton Place, between Belgrave Square and Hyde Park, which was his home for the rest of his life. Thence, autumn holidays excepted, with perfect regularity for five days in the week he resorted to Kew, pursued his botanical investigations from ten to four o'clock, then, returning, he wrote out the notes of his day's work before dinner, hardly ever breaking his fast in the long interval. With such methodical habits, with freedom from professional or administrative functions which consume the precious time of most botanists, with steady devotion to his chosen work, and with nearly all authentic materials and needful appliances at hand or within reach, it is not surprising that he should have undertaken and have so well accomplished such a vast amount of work; and he has the crowning merit and happy fortune of having completed all that he undertook.

Nor did he decline duties of administration and counsel which could rightly be asked of him. The presidency of the Linnæan Society, which he accepted and held for eleven years (1863 to 1874), was no sinecure to him; for he is said to have taken on no small part of the work of secretary, treasurer, and botanical editor. Somewhat to the surprise of his younger associates, who knew him only as the recluse student, he made proof in age of the fine talent for business and the conduct of affairs which had distinguished his prime in the management of the Horticultural Society; and in his annual presidential addresses, which form a volume of permanent value, his discussions of general as well as of particular scientific questions and interests bring out prominently the breadth and fullness of his knowledge and the soundness of his judgment.

The years which followed his retirement from the chair of the Linnæan Society, at the age of seventy-three, were no less laborious or less productive than those preceding; at the age of eighty (as the writer can testify) the diminution of bodily strength had wrought no obvious abatement of mental power

and not much of facility; and he was able to finish in the spring of 1883 the great work upon which he was engaged. As was natural, his corporeal strength gave way when his work was done. After a year and a half of increasing debility he died simply of old age — the survivor of his wife for three or four years, the last of the Benthams, for he had no children, nor any collateral descendants of the name.

A large part of his modest fortune was bequeathed to the Linnæan Society, to the Royal Society, for its scientific relief fund, and in other trusts for the promotion of the science to which his long life was so perseveringly devoted.

The record of no small and no unimportant part of a naturalist's work is to be found in scattered papers, and those of George Bentham are quite too numerous for individual mention. The series begins with an article upon *Labiatæ*, published in "Linnæa" in 1831; it closes with one in the "Journal of the Linnæan Society," read April 19, 1883, indicating the parts taken by the two authors in the elaboration of the "Genera Plantarum," then completed. Counting from the date of the Catalogue of Pyrenean plants, 1826, there are fifty-seven years of authorship. His first substantial volume in botany was the "Labiatarum Genera et Species, or a description of the genera and species of plants of the order *Labiatæ*, with their general history, characters, affinities, and geographical distribution," an octavo of almost 800 pages, of which the first part was published in 1832, the last in 1836. He found even the European part of this large order in much confusion; his monograph left its seventeen hundred and more of species so well arranged (under 107 genera and in tribes of his own creation) that there was little to alter, except as to the rank of certain groups, when he revised them for the "Prodromus" in 1848, and finally revised the genera (now increased to 136, and with estimated species almost doubled) for the "Genera Plantarum" in 1876. Although the work of a beginner, it took rank as the best extant monograph of its kind, namely, one of a large natural order, without plates. In it Mr. Bentham first set the example in any large way, of consulting all the available herbaria for the in-

spection and determination of type-specimens. To this end he made journeys to the continent every year from 1830 to 1834, visiting nearly all the public and larger private herbaria.

In the years during which the monograph of *Labiatæ* was in progress, Mr. Bentham elaborated and published the earlier of the papers which have particularly connected his name with North American botany. These are, first, the reports on some of the new ornamental plants raised in the Horticultural Society's Garden from seeds collected in western North America by Douglas, under the auspices of that society, by which were first made known to botanists and florists so many of the characteristic genera and species of Oregon and California, now familiar in gardens, Gilias and Nemophilas, Limnanthes, Phacelias, Brodiæas, Calochorti, Eschscholtzias, Collinsias, and the like; then the monograph of *Hydrophylleæ* (1834), followed the next year by that on Hosackia, and that on the *Eriogoneæ*, — all American and chiefly North American plants, — the first-fruits of a great harvest which even now has not wholly been gathered in; the field is so vast, though the laborers have not been few. Later, the "Plantæ Hartwegianæ," an octavo volume begun in 1839, but finished in 1857 with the Californian collections; and in 1844, the "Botany of the Voyage of the Sulphur," in quarto, the first part of which relates to Californian botany. The various papers upon South American botany are even more numerous; one of them being that in which Heliamphora, of British Guiana, a new genus of Pitcher Plants, of the Sarracenia family, was established.

Bentham's labors upon the great order *Leguminosæ* began early, with his "Commentationes de Leguminosarum Generibus," published in the Annals of the Vienna Museum, being the work of a winter's holiday (1836–7) passed in that capital in the herbarium then directed by Endlicher. This was followed by a series of papers, mostly monographs of genera, in Hooker's "Journal of Botany," in the "Journal of the Linnæan Society," and elsewhere, by the elaboration of the order for the imperial "Flora Brasiliensis," and later, by the "Revision of the Genus Cassia," and that of the suborder

Mimoseæ, in the "Transactions of the Linnæan Society," the latter (a quarto volume in size) published as late as the year 1875. Both are perfect models of monographical work.

An important series of monographs in another and more condensed form was contributed to De Candolle's "Prodromus," namely, the Tribe *Ericeæ* in the seventh volume, the *Polemoniaceæ* in the ninth, the *Scrophulariaceæ* in the tenth, the *Labiatæ* forming the greater part of the twelfth, and the *Eriogoneæ* in the fourteenth; these together filling 1133 pages according to the surviving editor. If not quite the largest collaborator of the De Candolles, as counted in pages, he was so in the number of plants described, and his work was of the best. It was also ready in time, which is more than can be said of the collaborators in general.

There are few parts of the world upon the botany of which Mr. Bentham has not touched — Tropical America, in the ample collections of Mr. Spruce, and those of Hartweg, distributed, and the former partly and the latter wholly determined by him, as also Hinds' collections made in the voyage of the Sulphur, besides what has already been adverted to; Polynesia, from Hinds' and Barclay's collections; Western Tropical Africa, in the Niger Flora, most of the "Flora Nigritiana" being from his hand; the "Flora Hongkongensis," in which he began the series of British colonial floras; and finally that vast work, the "Flora Australiensis," in seven volumes, which the author began when he was over sixty years old and finished when he was seventy-seven. Nor did he neglect the cultivation of the narrow and more exhausted field of British botany. His "Handbook of the British Flora," for the use of beginners and amateurs, published in 1858, has gone through four large editions. Its special object was to enable a beginner or a mere amateur, with little or no previous scientific knowledge and without assistance, to work out understandingly the characters by which the plants of a limited flora may be distinguished from each other, these being expressed as much as possible in ordinary language, or in such technical terms as could be fully explained in the book itself and easily apprehended by the learner. The immediate and continued

popularity of this handy volume, bringing the light of full knowledge and sound methods to guide the beginner's way, illustrates the advantage of having elementary works prepared by a master of the subject, whenever the master will take the necessary pains. To the same end, the author prepared for this volume an excellent and terse introduction to structural and descriptive botany, which has been prefixed to all the Colonial Floras. In the first edition to this British Flora it was attempted to use or to give English names to the genera and species throughout. This could be done only in such a familiar and well-trodden field as Britain, where almost every plant was familiar; but even here it failed, and in later editions the popular names were relegated to a subordinate position.

It has been stated that Mr. Bentham was over sixty years old when he undertook the "Flora Australiensis," and he was seventy-seven when he brought this vast work to completion, assisted only in notes and preliminary studies by Baron von Mueller of Melbourne. About the same time he courageously undertook the still greater task of a new "Genera Plantarum," to be worked out, not, like that of Endlicher, mainly by the compilation of published characters into a common formula, but by an actual examination of the extant materials, primarily those of the Kew herbarium, — this work, however, in conjunction with his intimate associate, Sir Joseph Hooker. This work is the only "joint production" in which Mr. Bentham ever engaged. The relations and position of the two authors made the association every way satisfactory, and the magnitude of the task made it necessary. The training and the experience of the two associates were very different and in some ways complemental, one having the greatest herbarium knowledge of any living botanist, the other, the widest and keenest observer of vegetable life under "whatever climes the sun's bright circle warms," as well as of antarctic regions which it warms very little. It would be expected, on the principle "juniores ad labores," that the laboring oar would be taken by the younger of the pair. It was long and severe work for both; but the veteran was hap-

pily quite free from, and his companion heavily weighted by, onerous official duties and cares; and so it came to pass that about two thirds of the orders and genera were elaborated by Mr. Bentham. In April, 1883, the completion of the work (*i. e.*, of the genera of Phænogamous plants, to which it was limited) closed this long and exemplary botanical career; and the short account which he gave to the Linnæan Society on the nineteenth of that month, specifying the conduct of the work and the part of the respective authors, was his last publication.

In this connection mention should also be made of the essays (which he simply calls "Notes") upon some of the more important orders which he investigated for the "Genera Plantarum," — the *Compositæ*, the Campanulaceous and the Oleaceous orders, the *Monocotyledoneæ* as to classification, the *Euphorbiaceæ*, the Orchis family, the *Cyperaceæ* and the *Gramineæ*. These are not mere abstracts, issued in advance, but critical dissertations with occasional discussions of some general or particular question of terminology or morphology. When collected they form a stout volume, which, along with the volume made up of his anniversary addresses when president of the Linnæan Society, and the paper on the progress and state of systematic botany, read to the British Association for the Advancement of Science in 1874, should be much considered by those who would form a just idea of the largeness of Mr. Bentham's knowledge and the character of his work.

It will have been seen that Mr. Bentham confined himself to the Phænogamia, to morphological, taxonomical, and descriptive work, not paying attention to the Cryptogamia below the Ferns, nor to vegetable anatomy, physiology, or palæontology. He was what will now be called a botanist of the old school. Up to middle age and beyond he used rather to regard himself as an amateur, pursuing botany as an intellectual exercise. "There are diversities of gifts;" perhaps no professional naturalist ever made more of his, certainly no one ever labored more diligently, nor indeed more successfully over so wide a field, within these chosen lines. For extent

and variety of good work accomplished, for an intuitive sense of method, for lucidity and accuracy, and for insight, George Bentham may fairly be compared with Linnæus, De Candolle, and Robert Brown.

His long life was a perfect and precious example, much needed in this age, of persevering and thorough devotion to science while unconstrained as well as untrammeled by professional duty or necessity. For those endowed with leisure, to "live laborious days" in her service, it is not a common achievement.

The tribute which the American Academy of Sciences pays to the memory of a deceased foreign honorary member might here fittingly conclude. But one who knew him long and well may be allowed to add a word upon the personal characteristics of the subject of this memorial; the more so that he is himself greatly indebted for generous help. For, long ago, when in special need of botanical assistance, Mr. Bentham invited him and his companion to his house at Pontrilas, and devoted the greater part of his time for two months to this service. Mr. Bentham's great reserve and dryness in general intercourse, and his avoidance of publicity, might give the impression of an unsympathetic nature; but he was indeed most amiable, warm-hearted, and even genial, "the kindest of helpmates," the most disinterested of friends.

AUGUSTUS FENDLER.[1]

After Dr. Engelmann's death, the beginning of a notice of Mr. Fendler was found upon his table, from which it was learned that he had died at Trinidad, some time previous. Inquiries sent to the Port of Spain, where he had for several years resided, remain unanswered. An autobiographical account which he addressed to a correspondent (and which, with some of his letters, we hope will before long be printed) enables us to state that Mr. Fendler was born at Gumbinnen, on the easternmost borders of Prussia, January 10, 1813, lost his father in infancy, was sent to the gymnasium of the town when twelve years old, but was at sixteen apprenticed to the town clerk, where, perhaps, he perfected the neat and clear handwriting with which his correspondents are familiar. Having a fondness for mathematics and chemistry, he obtained in 1834, upon examination, a nomination to the Royal Polytechnic School at Berlin, but relinquished it after a year on account of delicate health. In 1836 he came from Bremen to Baltimore, "with a couple of dollars in his pocket," worked in a tan-yard in Philadelphia, then in a lamp factory in New York; in 1838 he traveled in the most economical way to St. Louis, which required thirty days, and was employed by a lamp-maker who made "spirit-gas" for lighting public-houses, coal-gas being then unknown so far west. Soon after, he made his way to New Orleans and to Texas, where he was witness to the ravages of yellow fever in the summer and fall of 1839. He returned to Illinois, broken in health and empty in purse, taught school for some time; then, the spirit of wandering and of solitude coming strongly upon him, he took possession of an uninhabited island in the Missouri River, about three hundred miles above St. Louis, where he enjoyed a hermit's life for six months, and until a great spring rise of the

[1] American Journal of Science and Arts, 3 ser., xxix. 169. (1885.)

river threatened to sweep away his cabin, when he took to his canoe, and dropped down the stream among the floating logs and masses of ice. In 1844 he returned to Old Prussia on a visit, at Königsberg made the acquaintance of Ernest Meyer, the professor of botany, and learned from him — what he would have been most glad to know before — that dried specimens of plants for the herbarium might be disposed of at a reasonable price. Returning to St. Louis, he began to collect plants in this view, took the botanical specimens to Dr. Engelmann, who gave him botanical assistance and encouragement. In 1846 Dr. Engelmann and the writer of this notice obtained permission for the transportation of Mr. Fendler and his luggage along with the body of United States troops which took possession of Santa Fé, New Mexico; there he remained for about a year, and made his well-known New Mexican collection, the first-fruits of the botany of that interesting district. In 1849 he attempted another western botanical expedition, this time with Salt Lake in view. But on the plains he lost all his drying-paper in a flood of the Little Blue River; and he returned to St. Louis, to find that all his collections, books, journals, and other possessions had been burnt in the great conflagration which had just devastated that city. He now sought a different climate, and, at the approach of winter, went to the Isthmus of Panama for four months, made at Chagres an interesting botanical collection, returned by way of New Orleans to Arkansas, and to Memphis on the Tennessee side of the river, where for three years he carried on the camphene-light business, botanizing in the vicinity when he could. In 1854, the introduction of gas having made his occupation unprofitable, and a craving for new scenes being strong upon him, he sailed for La Guayra, went up to Caraccas and thence to Colonia Tovar, 6500 feet above the sea, built his cabin on the mountain side, where he lived four or five years and amassed his large and fine Venezuelan collections of dried plants, so well known in the principal herbaria of the world. His principal companions were his thermometer and barometer, and his careful meteorological observations were published by the Smithsonian Institution, in the report

of the year 1857. Returning to Missouri in 1864, he bought some wild land at Allenton, cultivated and lived on it for seven years (except one winter passed in the herbarium at Cambridge), having the companionship and assistance of a half-brother who had joined him, and whom, being rather feeble-minded, he took care of for the rest of his life. In 1871, having sold his place in Missouri, he returned again to Prussia, intending to remain in his native country. But he soon longed for the New World, to which he returned in 1873; he settled in Wilmington, Delaware, where, having the botanical companionship of Mr. Canby, he again interested himself in his favorite pursuits, — but now much more in speculative physics. For years the thoughts of his solitary hours had turned upon the cause of gravitation and its probable connection with other forces, and while at Wilmington he wrote (and unhappily printed at his own expense) a thin octavo volume, entitled "The Mechanism of the Universe." Repeated attacks of acute rheumatism constrained him to seek again a tropical climate, this time the island of Trinidad. He and his brother landed at the Port of Spain in June, 1877, where he passed the remainder of his days, living mainly on the products of the small plot of land which he purchased, renewing his old interest and activity in making botanical observations and collections, especially among the Ferns, of which he sent to Professor Eaton collections worthy of his better days. But, having exhausted in this respect the field within his immediate reach, and lost the vigor needed for laborious excursions, little had been heard of him for the past few years, and it is only indirectly that the fact of his death has been made known to us.

It is needless to say that Fendler was a quick and keen observer and an admirable collector. He had much literary taste, and had formed a very good literary style in English, as his descriptive letters show. He was excessively diffident and shy, but courteous and most amiable, gentle, and delicately refined. Many species of his own discovery commemorate his name, as also a well-marked genus, a Saxifragaceous shrub, which is winning its way into ornamental cultivation.

CHARLES WRIGHT.[1]

Charles Wright died on the 11th of August, at Wethersfield, Connecticut, at the home where he was born on the 29th of October, 1811, and where the early as well as the later years of his life were passed. He received his education in the grammar school of his native village and at Yale College, which he entered in 1831, graduating in 1835. His fondness for botany was developed while he was in college, although, so far as we can learn, he had no teacher. The opportunity of gratifying this predilection in an inviting region may have determined his acceptance, almost immediately after graduation, of an offer to teach in a private family at Natchez, Mississippi. Within a year pecuniary reverses of his employer terminated this engagement. At this time there was a flow of immigration into Texas, then an independent republic; and Mr. Wright joining in it, in the spring of 1837 made his way from the Mississippi to the Sabine, and over the border, chiefly on foot, botanizing as he went. Making his headquarters for two or three years at a place then called Zarvala, on the Neches, he occupied himself with land-surveying, explored the surrounding country, "learned to dress deer-skins after the manner of the Indians, and to make moccasins and leggins," "became a pretty fair deer-hunter," and inured himself to the various hardships of a frontier life at that period. When the business of surveying fell off he took again to teaching; and in the year 1844 he opened a botanical correspondence with the present writer, sending an interesting collection of the plants of eastern Texas to Cambridge. In 1845 he went to Rutersville in Fayette County, and for a year or two he was a teacher in a so-called college at that place, or in private families there and at Austin, devoting all his leisure

[1] American Journal of Science and Arts, 3 ser., xxxi. 12. (1886.)

to his favorite avocation. In the summer of 1847–8 he had an opportunity of carrying his botanical explorations farther south and west. His friend, Dr. Veitch, whom he had known in eastern Texas, raised a company of volunteers for the Mexican war, then going on (Texas having been annexed to the United States), and gave Mr. Wright a position with moderate pay and light duties. This took him to Eagle Pass on the Mexican frontier, where he botanized on both sides of the river. He returned to the north in the autumn of that year, with his botanical collections, and passed the ensuing winter in Connecticut and at Cambridge.

In the spring of 1849, Mr. Wright returned to Texas, and, at the beginning of the summer, with some difficulty obtained leave to accompany the small body of United States troops which was sent across the unexplored country from San Antonio to El Paso on the Rio Grande. Notwithstanding some commendatory letters from Washington, no other assistance was afforded than the conveyance of his trunk and collecting paper. He made the whole journey on foot, boarded with one of the messes of the transportation train, and endured many privations and hardships. The return to the sea-board, in autumn, was by rather a more northerly route and under somewhat less untoward conditions. The interesting collection thus made first opened to our knowledge the botany of the western part of Texas. It was published, as to the *Polypetalæ* and *Compositæ*, in the third volume of the "Smithsonian Contributions to Knowledge," as "Plantæ Wrightianæ," Part I, in 1852.

A year and more was then passed in the central portion of Texas, awaiting the opportunity for other distant explorations, supporting himself in part by teaching a small school. At length, in the spring of 1851, he joined the party under Colonel Graham, one of the commissioners for surveying and determining the United States and Mexican boundary from the Rio Grande to the Pacific, accepting a position partly as botanist, partly as one of the surveyors, which assured a comfortable maintenance and the wished for opportunity for botanical exploration in an untouched field. Attached only

to Colonel Graham's party, he returned with him without reaching farther westward than about the middle of what is now the territory of Arizona, and in the summer of 1852 he returned with his extensive collections to San Antonio, and thence to St. Louis, to deliver his *Cactaceæ* to Dr. Engelmann, and with the remainder to Cambridge. These collections were the basis of the second part of "Plantæ Wrightianæ," published in 1853, and, in connection with those of Dr. Parry, Professor Thurber, and Dr. J. M. Bigelow, etc., of the Botany of the Mexican Boundary Survey, published in 1859. As Mr. Wright collected more largely than his associate botanists, and divided his collections into sets, his specimens are incorporated into a considerable number of herbaria, at home and abroad, and are the types of many new species and genera. No name is more largely commemorated in the botany of Texas, New Mexico, and Arizona than that of Charles Wright. It is an Acanthaceous genus of this district, of his own discovery, that bears the name of Carlowrightia. Surely no botanist ever better earned such scientific remembrance by entire devotion, acute observation, severe exertion, and perseverance under hardship and privation.

Mr. Wright's next expedition was made under more pleasant conditions. It was a long one around the world, as botanist to the North Pacific Exploring Expedition, fitted out under Captain Ringgold, who was during the cruise succeeded by Commander John Rodgers. After passing the winter of 1852–3 at his home in Connecticut and at Cambridge, he joined this expedition in the spring, and sailed in the United States ship "Vincennes" from Norfolk, Virginia, on the 11th of June. The collections made when touching at Madeira and Cape Verde were of course unimportant; but at Simon's Bay, just round the Cape of Good Hope, a stay of six weeks resulted in a very considerable collection of about 800 species within a small area, the Cape being wonderfully crowded with all kinds of plants. The voyage was thence to Sydney and through the Coral Sea to Hongkong, which was reached about the middle of March, 1854. The collection of over 500 species of Phænogamous plants, which was made during that

spring and summer upon this little island, and supplemented in the spring of 1855, was in part the basis of Bentham's "Flora Hongkongensis." In the autumn of 1854, interesting collections were made on the Bonin and Loo Choo Islands, and later upon the islands between the latter and Japan. Still more extensive and important were the collections made in Japan, especially those of the northern island, although the stay was brief. Also those made in Behring's Straits, mainly on Kiene or Arakamtchetchene Island, on the verge of the Polar Sea, where the scientific members of the expedition passed the month of August and a part of September, 1855. Reaching San Francisco in October, the season being unpropitious for botany, Mr. Wright was detached from the expedition, and came home by way of San Juan del Sur and Nicaragua, botanizing for a few weeks upon an island in the Lake, and thence by way of Greytown to New York.

In the following autumn (of 1856) Mr. Wright began his prolific exploration of the botany of Cuba. Landing at Santiago de Cuba, on the southeastern part of the island, he passed the winter of 1856–7 and the greater part of the ensuing summer in that nearly virgin district, most hospitably entertained by his countryman, Mr. George Bradford, and among the caffetals of the mountains by M. Lescaille, returning home with his rich collections early in the autumn. A year later he revisited Cuba, was again received by his devoted friends, extended his botanical explorations to the northern coast, and also farther westward, exchanging the dense virgin forest for open Pine-woods, like those of the Atlantic southern States, stopping at various *hatos* or cattle-farms on his route, but reaching better accommodations at Bayamo, when his kind host, Dr. Don Manuel Yero, assisted him in making some profitable mountain excursions. In the winter and spring of 1861 he was again domiciled with the Lescailles at Monte Verde and at the other coffee plantations of this kind family; and from thence he was able to extend his herborizations to the eastern coast from Baracoa to Cape Maysi. The next winter he made his way westward to near the centre of the island, making headquarters at the sugar-plantation of the

hospitable Don Simon de Cardenas, thence visiting the Sienaga de Zapata, a great marshy tract toward the south coast. In the early summer he transferred his indefatigable operations to the Vuelt-abajo, as it is called, or that part of Cuba westward of Habana, making his home at Balestena, a cattle-farm at the southern base of the mountains opposite Bahia Honda, where he was long most hospitably entertained by Don Jose Blain and Don F. A. Sauvalle. From thence he pushed his explorations nearly to the southwestern extremity of the island at Cape San Antonio. In the summer of 1864 he came home with his large collections, remaining there and at Cambridge for about a year.

In the autumn of 1865 he went again and for the last time to Cuba, again traversed the Vuelt-abajo in various directions, proceeded by steamer to Trinidad, and botanized in the mountains behind that town ; thence by way of Santiago he revisited the scenes of his earlier explorations and the surviving friends who had efficiently promoted them. The oldest and best of them, the elder Lescaille, was now dead. In the month of July, 1867, our persevering explorer came home.

Mr. Wright's Cuban botanical collections, from time to time distributed into sets, with numbers, were acquired by several of the principal herbaria, the fullest sets of the Phænogamous and vascular Cryptogamous plants, by the herbarium of Cambridge, and by the late Professor Grisebach of Göttingen. Professor Grisebach was in these years engaged with his "Flora of the British West Indies"; so that he gladly undertook the determination of the plants of Cuba. They were accordingly mainly published in Grisebach's two papers, "Plantæ Wrightianæ e Cuba Orientali," in the "Memoirs of the American Academy of Arts and Sciences," 1860 and 1862, and in his "Catalogus Plantarum Cubensium exhibens collectionem Wrightianam aliasque minores ex Insula Cuba missas," an 8vo volume, published in Leipsic in 1866. The latter work enumerates the Ferns and their allies, but those for the earlier part were published in 1860 by Professor Eaton, in his "Filices Wrightianæ et Fendlerianæ," a paper in the eighth volume of the "Memoirs of the American Acad-

emy." The later collections were incompletely published in the "Flora Cubana," a volume issued by F. A. Sauvalle at Habana, in 1873 and later, a revision of Grisebach's Catalogue (without the references, but with Spanish vernacular names attached) which was made by Mr. Wright, who added the descriptions of a good many new species. The only other direct publication by Mr. Wright is his "Notes on Jussiæa," in the tenth volume of the Linnæan Society's Journal. As to the lower Cryptogams, Mr. Wright's very rich collections were distributed in sets and published by specialists: the *Fungi*, by Berkeley and the late Dr. Curtis; the *Musci*, by the late Mr. Sullivant; the *Lichenes*, by Professor Tuckerman in large part, and certain tribes quite recently by Müller of Geneva. So Mr. Wright's name is deeply impressed upon the botany of the Queen of the Antilles.

There was a prospect that he might do some good work for the botany of San Domingo; for in 1871 a government vessel was sent to make some exploration of that island, and Mr. Wright went with it. It was in winter, the dry season, and the excursion across the country was hurried and unsatisfactory; so that the small collection made in this, his last distant botanizing, was not of much account.

Mr. Wright's botanizing days were now essentially over. He made, indeed, a visit to the upper part of Georgia in the spring of 1875. But this was mainly for recuperation from the effects of a transient illness, and for seeing again a relative and companion of his youth, from whom he had long been separated. A large part of several years was passed at Cambridge, taking a part of the work of the Gray Herbarium; and one winter was passed at the Bussey Institution, in aiding his associate of the South Pacific cruise, Professor Storer. Of late there fell to him the principal charge of the family at Wethersfield, consisting of a brother who had become an invalid, and of two sisters in feeble health, all unmarried and aging serenely together. By degrees his own strength was sapped by some organic disease of the heart, which had given him serious warning; and on the 11th of August he suddenly succumbed, while making his usual round at evening to

look after the domestic animals of the homestead. Not returning when expected, he was sought for; the body was found as if in quiet repose, but the spirit had departed.

Mr. Wright was a person of low stature and well-knit frame, hardy rather than strong, scrupulously temperate, a man of simple ways, always modest and unpretending, but direct and downright in expression, most amiable, trusty, and religious. He accomplished a great amount of useful and excellent work for botany in the pure and simple love of it; and his memory is held in honorable and grateful remembrance by his surviving associates.

GEORGE W. CLINTON.[1]

George W. Clinton died, at Albany, on the 7th of September last, in the seventy-eighth year of his age. He was the son of DeWitt Clinton, one of the most distinguished governors, and the grand-nephew of George Clinton, the first governor of the State of New York. He was born on the 13th of April, 1807, whether in the city of New York or in the home on Long Island is uncertain. He became a student in Albany Academy in the year 1816, when his father entered upon his first tenure of office as governor, entered Hamilton College in 1821, was graduated in 1825, was led by his early scientific tastes to the study of medicine, which he pursued for a year or two; at least he attended two courses of lectures at the then flourishing country medical school at Fairfield, New York. There his acquaintance with Professor James Hadley further developed his fondness for chemistry and botany, as it did that of the writer of this notice a few years afterwards. He also came under the instruction or companionship of Dr. Lewis C. Beck, a younger brother of his medical preceptor Dr. T. Romeyn Beck, attended a course of private lectures on botany given by Dr. William Tully, entered into correspondence with Rafinesque, Torrey, etc., and so bid fair to give himself to scientific studies, as we may suppose with the approval of his father, who, it is well known, had a decided scientific bent. But Governor Clinton's death in February, 1828, wrought a change in his prospects and in the course of his life. Acting upon the advice of his father's friend, Ambrose Spencer, the distinguished chief justice of the State, he took up the study of law, attended the law lectures of Judge Gould at Litchfield, Connecticut, and continued his studies in Canandaigua, New York, in the office of John C. Spencer, whose daughter he

[1] American Journal of Science and Arts, 3 ser., xxxi. 17. (1886.)

married. Admitted to the bar in 1831, he established himself at Buffalo in 1836, and practised his profession most acceptably until the year 1854, when he became judge of the superior court of that city. This honorable position he continued to hold with entire approbation until January, 1878, when he retired under the provision of the constitution upon attaining the age of seventy years. Then he resumed the practice of the law for two or three years; but at length he took up his residence in Albany, partly for the more convenient rendering of his service as a regent of the university of the State, and its vice-chancellor, but mainly for investigating and editing the papers and writings of his great-uncle George Clinton. On the afternoon of the 7th of September he took an accustomed walk in the Rural Cemetery of Albany, and there he died, probably quite instantaneously; for when his body was found, two or three hours later, some withered sprays of White Melilot, which he had gathered, were still clasped in his hand.

Judge Clinton's professional life need not here be considered. I did not know him, but knew of him, as a botanist in his younger days. About the year 1860, after buying a botanical book for his daughter, the turning over its pages revived an almost forgotten delight; and when his attention was again given to the flowers he had so long neglected, we soon came into correspondence. "I might have become a respectable naturalist," he writes, "but was torn from it in my youth. . . . To become a botanist is now hopeless; I am, and must remain a mere collector. But then I collect for my friends and for the Buffalo Society of the Natural Sciences. If I can please my friends and help the Society it pleases me. I want it to succeed. Money I cannot give it, and I give it all I can, the benefit of my example and pleasant labors." An instructive and pleasant, and on his part a sprightly correspondence it has been, and most ardent and successful were his efforts in the development of the Society of the Natural Sciences over which he presided, and especially of its herbarium which he founded. In the spring of 1864 he wrote: "To-morrow I believe I shall be able to mail

you my 'Preliminary List of the Plants of Buffalo.' And I demand that immediately upon its reception, you write me, saying 'pretty well for you.' I do feel gratified that I have at last made the mitiest mite of a contribution to science. I know how extremely minute it is. I would not be so exacting but for the fact that my letter-book is just full, and I want to commence a new one with a letter from you, I mean with a note from you, a letter is too ambitious."

As this modest Preliminary List exemplifies the beginning, so the full and critically prepared "Catalogue of the Native and Naturalized Plants of the City of Buffalo and its Vicinity," (pp. 215), published in 1882–3, marks the conclusion and shows the fruits of Judge Clinton's work upon the flora of the district around Buffalo. This catalogue was indeed prepared and published by his near friend and associate Mr. Day, with a thoroughness and judgment which have been much commended. But the collection and elaboration of the materials, the critical determination of the species, and the preparation of the "Clinton Herbarium," as it is now appropriately called, were essentially his own work in the *horæ subsecivæ* of a busy professional life. If during middle life and while making his way in his chosen vocation he abandoned his early scientific avocation, he took it up again when he had achieved a position which allowed some well-earned leisure, and he pursued it with an added zeal, energy, and acumen, which should give his name a place among the botanical worthies, — to be remembered after those who knew and appreciated and loved him have passed away. A little Scirpus specifically bears his name; but I never see the modest Liliaceous plant of our northern woods, called Clintonia in honor of the father, without associating it with the son.

Judge Clinton's contributions to the literature of the legal profession consisted mainly of his "Digest of the Decisions of the Law and Equity Courts of the State of New York," in three stout volumes. But he was a not unfrequent and a fascinating writer in the newspapers of the city, an occasional lecturer upon historical as well as scientific topics, and an organizer or promoter of every good civic work. He was a per-

son of marked and distinct individuality. It had been said of him that "he was not like anybody else, did not look like anybody else, and did not talk like anybody else." But his ways and conversation were peculiarly winning and delightful. Of a rather large family of children, four survive, two of them sons, and a goodly number of grandchildren.

EDMOND BOISSIER.[1]

Edmond Boissier died on the 25th of September, at his country residence in Canton Vaud, Switzerland, at the age of seventy-five years and three months. Having known him personally almost from the beginning of his botanical career, which has been so honorable and distinguished, it is a melancholy satisfaction as well as a duty, to pay this passing tribute to his memory.

Boissier came from one of those worthy families which were lost to France and gained to Geneva by the revocation of the Edict of Nantes, — a family that has proved its talents and high character in more than one of its members. Madame the Countess de Gasparin is a sister next to him in age, and the two had their education very much in common. He was born at Geneva, May 25, 1810, brought up and educated there, except that the summers were passed at his father's place at Valeyres, which he in time inherited, and where his life was closed. From his youth he was fond of natural history and of travel. It was not in his disposition, nor of the Genevese spirit of that day, to lead an aimless life ; so, when he came to choose what may be called his profession, it was natural that, at Geneva, in the days of the elder De Candolle, he took to botany. He showed his great good sense by his early judicious choice of a field and by his unbroken devotion to it. To the Mediterranean region, to southern Spain, and the Orient most of his work relates. After a year or two of careful preparation he went to Spain, in 1837, explored especially Granada and the eastern Pyrenees, and between 1839 and 1845 brought out his "Voyage Botanique dans le midi de l'Espagne," in two large quarto volumes, the first of narrative and plates, one hundred and eighty in number, the

[1] American Journal of Science and Arts, xxxi. 20. (1886.)

second of descriptive matter relating to the Granadan flora. Among the species he brought to light was the *Abies Pinsapo*, the beautiful Fir-tree now so well known in cultivation. His narrative, besides its botanical interest, is charming reading.

In 1842, after his marriage to his cousin, of the De la Rive family, he traveled with his wife in Greece, Anatolia, Syria, and Egypt. It was to his dear companion that he dedicated two of their joint discoveries, *Omphalodes Luciliæ* and *Chionodoxa Luciliæ*. In 1849 he experienced the great sorrow of his life in her death from typhoid fever, during a second journey in the south of Spain. Between 1842 and 1854 he published the first series of his "Diagnoses Plantarum Orientalium Novarum," filling two volumes, and in 1855 the second series of almost equal extent; in 1848 he completed his monograph of the *Plumbaginaceæ*; in 1862 he promptly finished his conscientious elaboration of the great genus Euphorbia for De Candolle's "Prodromus," and in 1866 brought out the "Icones Euphorbiarum," of one hundred and twenty folio plates from outline drawings by Heyland. In 1881 he made a trip to Norway with his associate Reuter. Not to mention other journeys, he was again in Spain and adjacent countries in 1877, and lastly in 1881, his eighth visit, — then in wretched health. Passing by scattered papers of his, we come to his great work, the "Flora Orientalis," in five octavo volumes. It comprehends Greece and Turkey up to Dalmatia and the Balkans; the Crimea; Egypt up to the first cataracts; northern Arabia down to the tropical line; Asia Minor, Armenia, Syria, and Mesopotamia; Turkestan up to 45° of latitude, Persia, Afghanistan, and Beloochistan — that is, up to the borders of India. The first volume was published in 1867; the fifth, in 1884, brings the work down to its conclusion with the *Pteridophytes;* and the manuscript for a supplementary volume, for recent discoveries and some reëlaboration, was about half finished when he laid down his pen under an attack seemingly no worse than the many he had recovered from, but which now terminated his earthly life.

It was a noble life, shadowed by an early bereavement, and in later years worn by painful disease, — the manly life of one who lived simply and wrought industriously where many others with his independent fortune would have lived idly and luxuriously; and he was no less a loyal and public-spirited citizen. Upon an occasion when, long ago, we met him at Geneva, he had no time for botanical parlance, for he was doing duty in the ranks of the federal army. Later, at a time of commotion at Geneva, he helped to quell a revolutionary riot, and received a painful bayonet wound in the service. True to his ancestry, he was a devoted Protestant Christian, a trusted member of the synod of the Free Church in Canton Vaud, where he lived when not in winter residence at Geneva, and where his assiduous attentions to the poor and sick will be remembered. He was a man of fine presence, and till past middle life of much bodily vigor. As a botanist he gave himself to systematic work only, for which he had a fine tact, and, like the school in which he was bred, perhaps a faculty of excessive discrimination. No man living knew the Europeo-Caucasian plants so well, or could describe them better; and his herbarium must be, with possibly one rival, the most extensive and valuable private collection in Europe. He loved living flowers as well, and rejoiced in his choice conservatory collections at Rivage, on the shores of the Leman, and in his well-stocked rock-works of alpine plants which adorn his grounds at Valeyres.

A charming biographical notice by one who knew him well through his whole life, M. De Candolle, is contained in the "Archives des Science" of the "Bibliothèque Universelle de Geneva" for October last.

JOHANNES AUGUST CHRISTIAN RŒPER.[1]

Johannes August Christian Rœper died on the 17th of March, 1884, at the age of eighty-four. He had been for some time the oldest botanist we know of, at least the oldest botanical author; for his first work, a monograph of the German species of Euphorbia, was published in 1824. He was director of the Botanic Garden at Basle in 1828, when he published his classical paper "De Organis Plantarum," and he may have been so in 1826, when he contributed to Seringe's "Mélanges Botaniques" his paper on the nature of flowers and inflorescences, which first put the latter upon a scientific basis and essentially established the present nomenclature. He was botanical professor there in 1830, when he published his tract "De Floribus et Affinitatibus Balsaminearum." In these essays he gave the promise of being one of the foremost morphological botanists of the age. Some time before the year 1840 he was translated to Rostock, where he held the botanical professorship for more than forty years, but without fulfilling the promise of his youth by additional contributions to the science of any considerable importance. There are, however, some articles from him in the "Botanische Zeitung," and other German periodicals, the latest in the year 1859. In 1851 he was chosen a Foreign Member of the Linnæan Society of London. We find no record of the time or place of his nativity, but we infer from a statement in the preface of his work on Euphorbia, which was published at Göttingen, that he was a German and not a Swiss. He is said to have been most amiable, and of deep religious convictions.

[1] American Journal of Science and Arts, 3 ser., xxxi. 22. (1886.)

LOUIS AGASSIZ.

THERE is no need to give an abstract of the contents of these fascinating volumes,[1] for everybody is reading them. Most are probably wishing for more personal details, especially of the American life; but the editorial work is so deftly and delicately done, and "the story of an intellectual life marked by rare coherence and unity" is so well arranged to tell itself and make its impression, that we may thankfully accept what has been given us, though the desired "fullness of personal narrative" be wanting.

Twelve years have passed since Agassiz was taken from us. Yet to some of us it seems not very long ago that the already celebrated Swiss naturalist came over in the bloom of his manly beauty to charm us with his winning ways, and inspire us with his overflowing enthusiasm, as he entered upon the American half of that career which has been so beneficial to the interests of natural science. There are not many left of those who attended those first Lowell Lectures in the autumn of 1846, — perhaps all the more taking for the broken English in which they were delivered, — and who shared in the delight with which, in a supplementary lecture, he more fluently addressed his audience in his mother-tongue.

In these earliest lectures he sounded the note of which his last public utterance was the dying cadence. For, as this biography rightly intimates, his scientific life was singularly entire and homogeneous, — if not uninfluenced yet quite unchanged by the transitions which have marked the period. In a small circle of naturalists, almost the first that was assembled to greet him on his coming to this country, and of which the writer is the sole survivor, when Agassiz was inquired of as to

[1] "Louis Agassiz, his Life and Correspondence." (The Andover Review, January, 1886, p. 39.)

his conception of "species," he sententiously replied: "A species is a thought of the Creator." To this thoroughly theistic conception he joined the scientific deduction which he had already been led to draw, that the animal species of each geological age, or even stratum, were different from those preceding and following, and also unconnected by natural derivation. And his very last published works reiterated his steadfast conviction that "there is no evidence of a direct descent of later from earlier species in the geological succession of animals." Indeed, so far as we know, he would not even admit that such "thoughts of the Creator" as these might have been actualized in the natural course of events. If he had accepted such a view, and if he had himself apprehended and developed in his own way the now wellnigh assured significance of some of his early and pregnant generalizations, the history of the doctrine of development would have been different from what it is, a different spirit and another name would have been prominent in it, and Agassiz would not have passed away while fighting what he felt to be — at least for the present — a losing battle. It is possible that the "whirligig of time" may still "bring in his revenges," but not very probable.

Much to his credit, it may be said that a good share of Agassiz's invincible aversion to evolution may be traced to the spirit in which it was taken up by his early associate Vogt, and, indeed, by most of the German school then and since, which justly offended both his scientific and his religious sense. Agassiz always "thought nobly of the soul," and could in no way approve either materialistic or agnostic opinions. The idealistic turn of his mind was doubtless confirmed in his student days at Munich, whither he and his friend Braun resorted after one session at Heidelberg, and where both devotedly attended the lectures of Schelling, — then in his later glory, — and of Oken, whose "Natur-Philosophie" was then in the ascendant. Although fascinated and inspired by Oken's *à priori* biology (built upon morphological ideas which had not yet been established but had, in part, been rightly divined), the two young naturalists were not carried

away by it, — probably because they were such keen and conscientious observers, and were kept in close communion with work-a-day Nature. As Agassiz intimates, they had to resist "the temptation to impose one's own ideas upon Nature, to explain her mysteries by brilliant theories rather than by patient study of the facts as we find them," and that "overbearing confidence in the abstract conceptions of the human mind as applied to the study of nature;" although, indeed, he adds, "the young naturalist of that day who did not share, in some degree, the intellectual stimulus given to scientific pursuits by physio-philosophy would have missed a part of his training." That training was not lost upon Agassiz. Although the adage in his last published article, "A physical fact is as sacred as a moral principle," was well lived up to, yet ideal prepossessions often had much to do with his marshaling of the facts.

Another professor at Munich, from whom Agassiz learned much, and had nothing to unlearn, was the anatomist and physiologist Döllinger. He published little; but he seems to have been the founder of modern embryological investigation, and to have initiated his two famous pupils, first Von Baer, and then Agassiz, into at least the rudiments of the doctrine of the correspondence between the stages of the development of the individual animal with that of its rank in the scale of being, and the succession in geological time of the forms and types to which the species belongs: a principle very fertile for scientific zoölogy in the hands of both these naturalists, and one of the foundations of that theory of evolution which the former, we believe, partially accepted, and the other wholly rejected.

The botanical professor, the genial Von Martius, should also be mentioned here. He found Agassiz a student, barely of age; he directly made him an author, and an authority in the subject of his predilection. Dr. Spix, the zoölogical companion of Martius in Brazilian exploration, died in 1826; the fishes of the collection were left untouched. Martius recognized the genius of Agassiz, and offered him, and indeed pressed him to undertake their elaboration. Agassiz brought

out the first part of the quarto volume on the Fishes of the Brazilian Expedition of Spix and Martius before he took his degree of Doctor of Philosophy, and completed it before he proceeded to that of Doctor in Medicine in 1830. The work opened his way to fame, but brought no money. Still, as Martius defrayed all the expenses, the net result compared quite favorably with that of later publications. Moreover, out of it possibly issued his own voyage to Brazil in later years, under auspices such as his early patron never dreamed of.

This early work also made him known to Cuvier; so that when he went to Paris, a year afterwards, to continue his medical and scientific studies, — the one, as he deemed, from necessity, the other from choice, — he was received as a fellow-savant. Yet at first with a certain reserve, probably no more than was natural in view of the relative age and position of the two men; but Agassiz, writing to his sister, says: "This extreme but formal politeness chills you instead of putting you at your ease; it lacks cordiality, and to tell the truth, I would gladly go away if I were not held fast by the wealth of material of which I can avail myself." But only a month later he writes — this time to his uncle — that, while he was anxious lest he "might not be allowed to examine, and still less to describe, the fossil fishes and their skeletons in the Museum, . . . knowing that Cuvier intended to write a work on this subject," and might naturally wish to reserve the materials for his own use, and when the young naturalist, as he showed his own sketches and notes to the veteran, was faintly venturing to hope that, on seeing his work so far advanced, he might perhaps be invited to share in a joint publication, Cuvier relieved his anxiety and more than fulfilled his half-formed desires.

"He desired his secretary to bring him a certain portfolio of drawings. He showed me the contents: they were drawings of fossil fishes, and notes which he had taken in the British Museum and elsewhere. After looking it through with me, he said he had seen with satisfaction the manner in which I had treated this subject; that I had, indeed, anticipated him,

since he had intended at some future time to do the same thing; but that, as I had given it so much attention, and had done my work so well, he had decided to renounce his project, and to place at my disposition all the materials he had collected and all the preliminary notes he had taken."

Within three months Cuvier fell under a stroke of paralysis, and shortly died. The day before the attack he had said to Agassiz, "Be careful, and remember that work kills." We doubt if it often kills naturalists, unless when, like Cuvier, they also become statesmen.

But to live and work, the naturalist must be fed. It was a perplexing problem how possibly to remain a while longer in Paris, which was essential to the carrying on of his work, and to find the means of supplying his very simple wants. And here the most charming letters in these volumes are, first, the one from his mother, full of tender thoughtfulness, and making the first suggestion about Neuchâtel and its museum, as a place where the aspiring naturalist might secure something more substantial than "brilliant hopes" to live upon; next, that from Agassiz to his father, who begs to be told as much as he can be supposed to understand of the nature of this work upon fossil fishes, which called for so much time, labor, and expense; and, almost immediately, Agassiz's letter to his parents, telling them that Humboldt had, quite spontaneously and unexpectedly, relieved his present anxieties by a credit of a thousand francs, to be increased, if necessary. Humboldt had shown a friendly interest in him from the first, and had undertaken to negotiate with Cotta, the publisher, in his behalf; but, becoming uneasy by the delay, and feeling that "a man so laborious, so gifted, and so deserving of affection . . . should not be left in a position where lack of serenity disturbs his power of work," he delicately pressed the acceptance of this aid as a confidential transaction between two friends of unequal age.

Indeed, the relations between the "two friends," one at that time sixty-three, and the other twenty-five, were very beautiful, and so continued, as the correspondence shows. Humboldt's letters (we wish there were more of them) are

particularly delightful, are full of wit and wisdom, of almost paternal solicitude, and of excellent counsel. He enjoins upon Agassiz to finish what he has in hand before taking up new tasks (this is in 1837), not to spread his intellect over too many subjects at once, nor to go on enlarging the works he had undertaken; he predicts the pecuniary difficulties in which expansion would be sure to land him, bewails the glacier investigations, and closes with "a touch of fun, in order that my letter may seem a little less like preaching. A thousand affectionate remembrances. No more ice, not much of echinoderms, plenty of fish, recall of ambassadors *in partibus*, and great severity toward booksellers, an infernal race, two or three of which have been killed under me."

The ambassadors *in partibus* were the artists Agassiz employed and sent to England or elsewhere to draw fossil fishes for him in various museums, at a cost which Humboldt knew would be embarrassing. The ice, which he would have no more of, refers to the glacier researches upon which Agassiz was entering with ardor, laying one of the solid foundations of his fame. Curiously enough, both Humboldt and Von Buch, with all their interest in Agassiz, were quite unable to comprehend the importance of an inquiry which was directly in their line, and, indeed, they scorned it; while the young naturalist, without training in physics or geology, but with the insight of genius, at once developed the whole idea of the glacial period, with its wonderful consequences, upon his first inspection of the phenomena shown him by Charpentier in the valley of the Rhône.

It is well that Humboldt's advice was not heeded in this regard. Nevertheless, he was a wise counselor. He saw the danger into which his young friend's enthusiasm and boundless appetite for work was likely to lead him. For of Agassiz it may be said, with a variation of the well-known adage, that there was nothing he touched that he did not aggrandize. Everything he laid hold of grew large under his hand, — grew into a mountain threatening to overwhelm him, and would have overwhelmed any one whose powers were not proportionate to his aspirations. Established at Neuchâtel, and

giving himself with ardor to the duties of his professorship, it was surely enough if he could do the author's share in the production of his great works on the fossil and the fresh-water fishes, without assuming the responsibilities and cares of publication as well, and even of a lithographic establishment which he set up mainly for his own use. But he carried on, *pari passu*, or nearly so, his work on fossil Mollusca, — a quarto volume with nearly a hundred plates, — his monographs of Echinoderms, living and fossil, his investigations of the embryological development of fishes, and that laborious work, the "Nomenclator Zoölogicus," with the "Bibliographia," later published in England by the Ray Society. Moreover, of scattered papers, those of the Royal Society's Catalogue which antedate his arrival in this country are more than threescore and ten. He had help, indeed; but the more he had the more he enlarged and diversified his tasks, Humboldt's sound advice about his zoölogical undertakings being no more heeded than his fulminations against the glacial theory.

In the midst of all this, Agassiz turned his glance upon the glaciers, and the "local phenomenon" became at once a cosmic one. So far a happy divination; but he seems to have believed quite to the last that not only the temperate zones, but whole intertropical continents — at least the American — had been sheeted with ice. The narrative in the first volume will give the general reader a vivid but insufficient conception of the stupendous work upon which he so brilliantly labored for nearly a decade of years.

Cœlum non animum mutant who come with such a spirit to a wider and, scientifically, less developed continent. First as visitor, soon as denizen, and at length as citizen of the American republic, Agassiz rose with every occasion to larger and more various activities. What with the Lowell Institute, the college in Charleston, South Carolina, and Cornell University, in addition to Harvard, he may be said to have held three or four professorships at once, none of them sinecures. He had not been two months in the country before a staff of assistants was gathered around him and a marine zoölogical

laboratory was in operation. The rude shed on the shore and the small wooden building at Cambridge developed under his hand into the Museum of Zoölogy, — if not as we see it now, yet into one of the foremost collections. Who can say what it would have been if his plans and ideas had obtained full recognition, and "expenditure" had seemed to the trustees, as it seemed to him, "the best investment," or if efficient filial aid, not then to be dreamed of, had not given solid realization to the high paternal aspirations! In like manner grew large under his hand the Brazilian exploration so generously provided for by a Boston citizen and fostered by an enlightened emperor; and on a similar scale was planned, and partly carried out, the "Contributions to the Natural History of the United States," as the imperial quarto work was modestly entitled, which was to be published "at the rate of one volume a year, each volume to contain about three hundred pages and twenty plates," with simple reliance upon a popular subscription; — and so, indeed, of everything which this large-minded man undertook.

While Agassiz thus was a magnanimous man, in the literal as well as the accepted meaning of the word, he was also, as we have seen, a truly fortunate one. Honorable assistance came to him at critical moments, such as the delicate gift from Humboldt at Paris, which perhaps saved him to science; such as the Wollaston prize from the Geological Society in 1834, when he was struggling for the means of carrying on the Fossil Fishes. The remainder of the deficit of this undertaking he was able to make up from his earliest earnings in America. For the rest, we all know how almost everything he desired — and he wanted nothing except for science — was cheerfully supplied to his hand by admiring givers. Those who knew the man during the twenty-seven years of his American life can quite understand the contagious enthusiasm and confidence which he evoked. The impression will in some degree be transmitted by these pleasant and timely volumes, which should make the leading lines of the life of Agassiz clear to the newer generation, and deepen them in the memory of an older one.

EDWARD TUCKERMAN.[1]

On the 15th of March last, the Academy lost one of the older and more distinguished members of the botanical section, the lichenologist, Edward Tuckerman.

He was born in Boston, December 7, 1817, was the eldest son of a Boston merchant of the same name and of Sophia (May) Tuckerman. He was prepared for college at the Boston Latin School, whence, in obedience to his father's choice rather than of his own, he went to Union College at Schenectady. Entering as a sophomore, he took his B. A. degree in 1837. He then entered the Harvard Law School, took his degree in 1839, and remained in residence in Cambridge for a year or two longer. In the year 1841 he went to Germany and Scandinavia, going as far north as Upsala, devoting himself, as in a subsequent visit, to philosophical, historical, and botanical studies. On his return, in September, 1842, he made, with the writer of this notice, a botanical excursion to the White Mountains of New Hampshire, with which he was already familiar. At the close of that or early in the following year he took up his residence at Union College, proceeded to the M. A. degree, and there prepared and privately published one of the smaller, but noteworthy, of his botanical papers.

In the year 1844 or 1845 he returned to Cambridge, and in the autumn of 1846, in his twenty-ninth year, he became again an undergraduate. Applying for admission to the incoming senior class, he remarked to President Quincy that his father had broken the family tradition by sending him to another college, and that he proposed to correct the mistake. To the suggestion, that, being already an alumnus of the Law School as well as of Union, the University would willingly concede to him the earlier degrees he sought, he replied that

[1] Proceedings of the American Academy of Arts and Sciences, new ser. xiii., 539. (1886.)

he proposed to receive them in the ordinary way. He accordingly passed the regular examinations, took the whole routine of the studies of his class, and so was graduated with distinction in the class of 1847, — a unique but characteristic illustration of a loyal spirit, becoming "small by degrees and beautifully less."

His passion for university study was not yet quite satiated. For, two or three years later, he entered the Harvard Divinity School, passed through its course of study and prescribed exercises, — among them the delivery of a sermon in one of the Cambridge churches, — and so, in the year 1852, he became for the third time an alumnus of Harvard.

In May, 1854, he married in Boston Sarah Eliza Sigourney Cushing, who survives him, without offspring. Removing that year to Amherst, he built with excellent taste, upon a beautiful site, the house which has ever since been their abode. Although mainly devoted to botanical investigations, his first official connection with Amherst College was that of lecturer in history, then that of professor of oriental history, down to the year 1858, when he was collated to the chair of botany, which he held to the end of his life, although of late years relieved from the duty of class instruction. The college did itself the honor to confer upon its professor the degree of LL. D.

We cannot say when or how Professor Tuckerman became a botanist. But at an early period he was intimate with Dr. Harris, then University librarian, and with the ardent William Oakes of Ipswich, upon whom, through Dr. Osgood of Danvers, descended the mantle of Manasseh Cutler, of Essex County, the earliest New England botanist.

He must have been attracted to the Lichens almost from the beginning; for his first publications were upon Lichens of New England, largely those of his own collecting in the White and Green Mountains, in two papers, one communicated to the Boston Natural History Society, in 1838 or 1839, the other in 1840. These were soon followed by papers on Phænogamous botany, namely: one "On Oakesia a new Genus of the Order Empetreæ," a contribution made while

he was abroad, in the summer of 1842, to Hooker's "London Journal of Botany." Unfortunately, the interesting plant which he thus dedicated to his botanical associate, William Oakes, who well deserved such commemoration, proved to be a second species of Corema. In 1843, at Schenectady, he privately printed and issued his "Enumeratio Methodica Caricum quarundam" (pp. 21, 8vo), in which he displayed not only his critical knowledge of the large and difficult genus Carex, but also his genius as a systematizer; for this essay was the first considerable, and a really successful, attempt to combine the species of this genus into natural groups. It is wholly in Latin, which he much affected for scientific disquisition as well as for technical characters, and used with facility and elegance. In the same year also appeared, in the American Journal of Science, the first of his "Observations on some interesting Plants of New England." This was followed in 1848 by a second, and in 1849 by a third paper in the same Journal; these containing, *inter alia*, his elaboration of our species of Potamogeton, then for the first time critically studied. These papers — with one or two in Hovey's Magazine and elsewhere, at about the same date — may be said to have ended his work in Phænogamous botany, although his interest in the subject never died out; for when he accepted the chair of botany at Amherst he began the preparation of "A Catalogue of Plants growing without cultivation within thirty miles of Amherst College," which he published in the year 1875, the late Mr. Charles Frost of Brattleborough contributing the lower Cryptogamia other than the Lichens. In matter and form, as well as in typography (in which Professor Tuckerman had exquisite taste), this catalogue is one of the very best.

But it was to Lichenology that his strength, as indeed almost his whole life, was most assiduously devoted. When, in his youth, the active members of the newly organized Natural History Society of Boston divided among themselves the work of making better known the animals, plants, and minerals of Massachusetts, the study of the Lichens either was assigned to him or he volunteered to undertake it. From

this came those earliest papers which have already been mentioned. Also his "Synopsis of the Lichens of New England, the other Northern States, and British America," communicated to this Academy in the autumn of 1847, which is the most considerable botanical contribution to the first volume of the Proceedings. The fourth, fifth, sixth, and seventh volumes contain other of his Lichenological papers, of wholly original matter and critical character, — largely upon collections which had begun to come to him from the Rocky Mountain region, from Texas, the Pacific coast, the Sandwich Islands, and especially from the rich materials gathered in Cuba and elsewhere by the late Charles Wright. In these years, too, he much helped the study of his favorite plants by the preparation and issue of his "Lichenes Americæ Septentrionalis Exsiccati," in six fasciculi, or three volumes, highly valued by those who fortunately possess them. Equally fortunate are the herbaria which possess the "Lichenes Caroli Wrightii Cubæ curante E. Tuckerman," which authenticate his thorough work upon that portion of Mr. Wright's Cuban collections that he undertook to elaborate.

Passing without notice various subsidiary contributions both to journals and to the reports of exploring expeditions, we come to a pamphlet which he independently published at Amherst, in 1866, entitled "Lichens of California, Oregon, and the Rocky Mountains, so far as yet known," which, small though it be (pp. 35, 8vo), is particularly noteworthy; for in this he lays down the principles and matured opinions which he had adopted, and which he firmly adhered to, for the taxonomy and classification of Lichens. These are fully exemplified in the two systematic works to which Professor Tuckerman's later years and maturest powers were persistently devoted, — works which, partly from their publication somewhat out of the ordinary channels, are by no means so well known as they should be, but which surely secure to their author the position of a master in his department, — in which, indeed, we suppose he has left behind him no superior. These works are, first, the "Genera Lichenum, an Arrangement of the North American Lichens" (pp. 283, 8vo), pub-

lished at Amherst in the year 1872; second, the "Synopsis of the North American Lichens," Part I, comprising the *Parmeliacei*, *Cladoniei*, and *Cœnogoniei*, published in Boston in 1882. It is hoped, but it is not yet certain, that some portions of the remainder, relating to the less conspicuous but more difficult tribes, may have been substantially made ready for the printer. The loss, we fear, is irreparable; for the work cannot be completed by other hands upon quite the same lines, nor in our day with the same knowledge and insight; and Professor Tuckerman's mode of exposition is inimitable.

That which Professor Tuckerman did accomplish, however, suffices to show the wide reach and remarkable precision of his knowledge, his patience and thoroughness in investigation, his sagacity in detecting affinities, and his philosophical and rather peculiar turn of mind. He wrote in a style which — though perhaps founded on that of his botanical model, Fries, for succinctness, and that of his favorite German philosophical masters for involution — was yet all his own, and which was the more pronounced in advancing years, when, owing to increasing deafness and delicate health, he led a more secluded life. In disquisition, the long and comprehensive sentences which he so carefully constructs are unmistakably clear to those who will patiently plod their way through them, and his choice even of unusual words is generally felicitous; but sometimes the statements are so hedged about and interpenetrated by qualifications or reservations, and so pregnant with subsidiary although relevant considerations, that they are far from easy reading. Like nests of pill-boxes, they are packed into least bulk; but for practical use they need to be taken apart.

That Professor Tuckerman could write idiomatic and clear-flowing English upon occasion, the delightful introduction to his edition of Josselyn's "New England's Rarities" demonstrates; and in the framing of botanical descriptive phrases, Latin or English, in which clearness and brevity with just order and proportion are desiderata, he had hardly a superior.

As has been said, his botanical model was Elias Fries. He

had visited him at Upsala, and he kept up a correspondence with him to the end of the venerable botanist's life. He caught from Fries, or he developed independently, and cultivated to perfection, that sense of the value of the indefinable something which botanists inadequately express by the term "habit," which often enables the systematist to divine much further than he can perceive in the tracing of relationships. Upon this, in direct reference to Fries, and with a use of the term that seems to correlate it with "insight," Tuckerman remarks: "So great is the value of Habit in minds fully qualified to apprehend and appreciate its subtleties, that such minds may not only anticipate what the microscope is to reveal, but help us to understand its revelations." It should be remembered, however, that when Fries did the best of his work there were no microscopes of much account; and it is probable that Tuckerman would have done more, and perhaps have reached some different conclusions, if he had earlier and more largely used the best instrumental appliances of the time. One advantage, however, of his way of study, and his philosophical conception of an ideal connection of forms which are capable of a wide play of variation, was that he took broad views of genera and species. So he was quite unlike that numerous race of specialists who, in place of characterizing species, describe specimens, and to whom "genus" means the lowest recognizable group of species.

As to the vexed question in Lichenology, which came to him rather late and seemed to threaten the stability of his work, it was most natural that, at his time of life, he did not take kindly to the Algo-fungal notion of Lichens, and that he was convinced of its falsity by questionable evidence.

Professor Tuckerman was much more than an excellent specialist. Happily, he did not become such until he had laid a good foundation, for the time, in general systematic botany; and his early studies show that he was a man of scholarly culture over an unusually wide range. He was at home in the leading modern languages; he wrote Latin with reasonable facility, and botanical Latin remarkably well; he had given serious attention to law, divinity, philosophy, and history; and

he was fond of antiquarian and genealogical researches. He privately published (without date) a handsome edition of Josselyn's "New England's Rarities Discovered," with copious critical annotations, of 134 pages, including an introduction of 27 pages, which contains a biography of Josselyn and a sketch of the earlier sources of our knowledge of New England plants and of some of the people who made them known.[1] Among them is a biographical notice of Manasseh Cutler, one of the very first elected Fellows of this Academy, the earliest botanical contributor to its Memoirs, — pastor, naturalist, and statesman, the builder of New England in Ohio, probably the originator of the Dane Resolutions in Congress, — a man whose name deserves larger remembrance than it has yet received.

Professor Tuckerman was elected into this Academy in May, 1845. He was one of the corporate members of the National Academy of Sciences at Washington, and an honorary member of several of the learned societies and academies of Europe. He was still young when Nuttall dedicated to him the genus Tuckermania, founded upon one of the handsomer of the Californian *Compositæ*, which is retained as a subgenus. For one who did not attain the age of sixty-seven, his publications span a remarkably wide interval. It is said that he contributed several short articles on antiquarian topics to the "Mercantile Journal," in the year 1832; also that, in 1832 and 1833, he assisted the late Mr. Samuel G. Drake in the preparation of his "Book of the Indians" and "Indian Wars." Then, between 1834 and 1841, he contributed to the "New York Churchman" no less than fifty-four articles, under the title of "Notitia Literaria" and "Adversaria," upon points in history, biography, and theology. His latest botanical article was contributed to the "Bulletin of the Torrey Botanical Club" in 1884. A little later, possibly, are some of his contributions to the "Church Eclectic," mostly pseudonymous, — critical notices of recent theological works. He was a keen critic, and very independent in his judgments. He had

[1] It appears that this was a contribution to the fourth volume of the "Archæologia Americana," published in 1860.

sounded in his time the depths of various opinions. But as he was born into, so he died, as he had lived, devoutly, in the communion of the Protestant Episcopal Church. With some interruptions, and of late under increasing infirmities, he yet continued his Lichenological studies until within a few weeks before the end. Living for a long while in comparative seclusion, few of our younger botanists can have known him personally, or much by correspondence; and most of his old associates and near friends, who knew him best and prized him highly for his sterling character, have gone before him.

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